Is Corruption Compensation? Evidence from Local Public Office in India

Dahyeon Jeong∗
Ajay Shenoy†
Laura Zimmermann‡§

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Abstract

The old idea that corruption “greases the wheels” of government has recently been revived to propose that local officials in developing countries will better implement welfare programs if improved performance also allows more corruption. We test this corruption-as-compensation hypothesis in Indian villages whose presidents, in charge of running a massive public works scheme, claim such behavior is widespread. We link millions of administrative job records to election outcomes, enabling us to measure both the presidents’ performance and their self-dealing. Despite the seemingly ideal context, our results are more consistent with imperfect monitoring than corruption as compensation. In the year after the election, winners of close elections receive 3 times as many workdays as losers and typical villagers. But self-dealing declines over time in villages with plausibly better monitoring capacity, even though there is no decline in performance. In villages without such capacity, self-dealing persists.

JEL Codes: D72, D73, H53, H75, I38

∗University of California, Santa Cruz; email at dajeong@ucsc.edu. Website: people.ucsc.edu/~dajeong.
†University of California, Santa Cruz; email at azshenoy@ucsc.edu. Phone: (831) 359-3389. Website: http://people.ucsc.edu/~azshenoy. Postal Address: Rm. E2455, University of California, M/S Economics Department, 1156 High Street, Santa Cruz CA, 95064.
‡University of Georgia; email at lvzimmer@uga.edu. Phone: (706) 542-1311. Website: https://sites.google.com/site/lauravanessazimmermann. Postal Address: Rm. B410 Amos Hall, University of Georgia, 620 South Lumpkin Street, Athens GA, 30602.
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1 Introduction

Around the world, the responsibility for administering welfare programs lies increasingly with local governments. While such decentralization may have large benefits, it may also overburden administrative capacity and lead to an under-provision of government benefits. If under-provision arises because local officials have little incentive to run the program effectively, some have argued that the opportunity to collect rents from the program could perversely realign incentives towards good service (Ravallion, 2018). Similar to classic theoretical models in the ‘greasing the wheels’ corruption literature, voters might grudgingly allow politicians to take informal compensation for themselves in exchange for improved service delivery (Huntington, 1968; Leff, 1964; Lui, 1985).

But testing the corruption-as-compensation hypothesis is challenging. The hypothesis assumes voters have some means to observe both corruption and the performance it compensates. Though the assumption may be plausible in many situations, there are few instances where it is directly verifiable. Even if that condition is met, applying the test in practice would also require that the researcher is able to detect what the voters observe. Corruption is always difficult to measure, and in this context even measuring “performance” is a challenge. Most prior work in the literature has either focused on other potential explanations for corruption, or has studied officials whose performance yields amorphous returns like local economic growth or perceptions of efficient local governance. As there is usually no unambiguous link from corruption to performance it is hard to confirm whether one is compensation for the other. As a result, the burgeoning literature on corruption has typically been unable to test this channel convincingly.

This paper tests the corruption-as-compensation channel in a context where both performance and corruption are measurable. We study how local politicians in the Indian state of Uttarakhand implement the world’s largest anti-poverty program. The National Rural Employment Guarantee Scheme (NREGS) funds short-term make-work jobs building public works within the village. Village council presidents play a key role in both bringing jobs to their villages and in allocating these jobs between constituents. Their success in generating public works jobs is a measure of performance. A clearly related measure of corruption in this context is the num-
ber of jobs presidents allocate to their own households. While this behavior is not illegal, such self-dealing meets the most common definition of corruption as the misuse of public office for private gain (Svensson, 2005).

The program’s unusual level of transparency allows us to create a unique dataset that directly captures an individual president’s self-dealing of NREGS benefits as well as her own performance in generating NREGS employment for the village. Each employment spell is published on a publicly available government website in close to real time and identifies the recipient by name and location. We scrape millions of these reports, covering over 90 percent of rural households in Uttarakhand.\(^1\) We match these NREGS reports to the election returns for candidates competing in thousands of village council elections. This dataset shows how winning candidates allocate NREGS jobs to their own household as compared to those of typical villagers. Since election winners are likely to differ from typical villagers on a number of observable and unobservable characteristics, we compare the labor allocations of winners and runners-up in close elections decided by a few votes. Our regression discontinuity design ensures that the labor quota of the runner-up is a good counterfactual for that of the winner had she remained out of office. We also observe the NREGS employment the politician creates for the entire village, a direct measure of program performance.

Our context is well-suited to study the corruption-as-compensation channel. To be plausible, voters need to be able to observe and punish corruption, but instead allow self-dealing proportional to performance to incentivize politicians to put in more unobservable effort. In contrast to most other contexts, self-dealing in our case is plausibly visible to voters through the website at any point and in close to real time. Figure 1 provides an example. It shows the job card record for the council president alongside the closest runner-up in the election with household location down to the village level and a list of all adult household members. Below that information the website provides employment and wage details on all job spells under the program with their exact dates and the project name. In the example, Figure 1 shows that the household of the council president has received dozens of days of labor, whereas the runner-up—who lost by just 3 votes—has received no jobs at all. While voters in other contexts may be aware that money for welfare benefits goes missing, voters in Uttarakhand can directly observe how much money

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\(^1\) Households can be registered for the program without ever having worked on it.
flows into the president's own bank account.

Even villagers that do not access the website may often be able to observe self-dealing through other means. Since NREGS is a public-works program, worksites around the village are directly visible to constituents. Villages in Uttarakhand are small, with 80 percent of villages having fewer than 1000 inhabitants. This means that villagers are very likely to know the council president personally and can monitor his actions more easily than in larger polities. Under NREGS, villagers also have the right to audit the physical muster rolls of the program. Additionally, accountability is likely to be much higher in this setting than is typical of local leaders in developing
countries. Village council presidents in Uttarakhand are directly elected through competitive local elections, and by design our sample focuses on close elections determined by a small number of votes. This restriction excludes villages where elite capture or nonexistent political opposition have rendered the president unaccountable to voters.

To further establish that corruption-as-compensation is plausible in our context, we ask the village council presidents themselves using an original survey of about 200 presidents. They answer a number of questions about their work and how they run NREGS but without being told that we are studying self-dealing or monitoring their NREGS allocations. They report that NREGS takes a lot of effort to implement, and more than half of survey respondents claim that presidents who create more NREGS jobs for the village would be expected to also take more NREGS jobs for themselves. The majority of village council presidents therefore rationalize self-dealing by claiming villagers accept corruption-as-compensation.

Overall, our test of the corruption-as-compensation mechanism therefore occurs in a highly plausible context based on qualitative and descriptive evidence. We now turn to testing this channel empirically. We first establish that there is a sizeable amount of corruption. The winners of close elections receive nearly 3 times as many days of labor as losers in the year after the election. On average individuals who visit the website would therefore see that the list of job spells for the president is three times longer than their own in the year after the election, an easily detectable difference even when just casually comparing job card records. Consistent with self-dealing as compensation for good performance, we find a correlation between the extent of self-dealing and the average NREGS benefits enjoyed by other households in the village.

But the corruption-as-compensation hypothesis does not withstand more rigorous scrutiny. We find that self-dealing declines over time while benefits for other villagers remain roughly constant or even rise. Tellingly, the overall decline in corruption is entirely driven by villages best able to monitor the president. If villagers accept self-dealing that is proportional to the overall NREGS performance to incentivize presidents to put in unobservable effort, then the “corruption contract” should be most likely to arise in villages where voters can perfectly monitor performance and self-dealing. We proxy for monitoring capacity with the distance to an internet cafe, which may grant direct access to the NREGS website to villagers and may be correlated with closer scrutiny from media, local activists, and the political opposition. Stakeholders
living far from a cafe may have to rely more strongly on physical muster rolls that can be more easily hidden or manipulated. We find that self-dealing falls to zero in villages close to a cyber cafe but shows no decline at all in villages far from a cafe—the opposite of what is predicted by corruption-as-compensation. By contrast, NREGS benefits for villagers other than the president are equally high in both sets of villages—more evidence that good performance is possible even in the absence of self-dealing.

Instead, the evidence is more consistent with imperfect monitoring. While monitoring NREGS in Uttarakhand is plausibly easier than in other contexts, it may not be perfect in areas without reliable access to the internet. Presidents may therefore be able to hide self-dealing in villages without internet cafes as long as it remains a small amount of the total NREGS benefits. This would explain why self-dealing appears to be proportional to the NREGS implementation quality in the village. In contrast, in areas with better monitoring capacity, pressure from villagers seems to lead to a complete eradication of self-dealing over time.

Our results contribute to the ‘greasing the wheels’ literature. Several theoretical models predict that corruption may be a second-best solution (Huntington, 1968; Leff, 1964; Lui, 1985). Typically, these models focus on interactions between firms and bureaucrats, showing that corruption can increase allocation efficiency if the most efficient firms are willing to pay the highest bribes. But while these models focus on the interactions between actors at the micro level, the existing empirical evidence has mostly been confined to analyzing their predictions at the macro level using cross-country datasets (Fisman and Svensson, 2007; Méon and Sekkat, 2005; Méon and Weill, 2010; Wei, 2000). Two rare exceptions are Mironov and Zhuravskaya (2016) and Weaver (2018). Mironov and Zhuravskaya (2016) reject a ‘greasing the wheels’ explanation, since procurement contracts in more corrupt Russian localities are allocated to less efficient rather than more efficient firms. Weaver (2018), on the other hand, finds that the allocation of health bureaucracy jobs to the person willing to pay the highest bribe leads to higher-quality hires than decisions based on a knowledge test. Our paper therefore contributes to the existing literature in two ways. First, we provide new micro-level evidence on the topic to a very small empirical literature. In contrast to Weaver (2018) and despite politicians’ claims in our original survey, we find little evidence for a corruption-as-compensation channel in the empirical analysis.\(^2\) Sec-

\(^2\) This makes our results more consistent with the macro literature, which overwhelmingly finds no support for
ond, our paper extends the existing theoretical and empirical literature to interactions between
citizens and local politicians and focuses on service delivery rather than allocative efficiency.

More broadly, our paper contributes to the literature on corruption (Avis et al., 2018; Bertrand
et al., 2007; Campante and Do, 2014; Di Tella and Schargrodsky, 2003; Ferraz and Finan, 2008;
Niehaus and Sukhtankar, 2013b; Olken, 2007; Reinikka and Svensson, 2004). The existing liter-
ature tends to focus on leakages, where it may often be unclear where exactly the money goes,
for example whether bureaucrats or politicians take the biggest cut. One strength of our context
is that we directly observe that payments from NREGS land directly in the bank account of the
politician, making it unusually clear where exactly the money disappears to, at least initially.

This means that specific NREGS benefits can be directly attributed to an individual politician,
and are therefore plausibly taken as a direct indication of the politician’s greed. Given the nature
of the self-dealing, it should also be unusually salient that the NREGS jobs that the president
received could have gone to other villagers. These features also allow us to contribute to the
small literature on unofficial returns to office in developing countries. Our results suggest that
even in a context where transparency and accountability are plausibly high, corruption can only
be fully eliminated if citizens have the capacity to monitor behavior. Having an internet cafe
nearby is likely to provide local stakeholders with easier access to the internet, allowing for more
effective monitoring of NREGS allocations. While Muralidharan et al. (2016) have shown that
the reforms that linked NREGS benefits to biometric identification information have improved
targeting substantially, we show that self-dealing persists.

3 For a literature overview see Aidt (2003); Bardhan (1997); Olken and Pande (2012); Svensson (2005).
4 We do not find any evidence that politicians are misusing NREGS to provide preferential access to their extended
family or other members of the village council. While we cannot rule out that some of the NREGS benefits
politicians allocate themselves are used to pay off supporters or other officials in the system, directly providing
them with NREGS benefits would seem to be an easier way of making those payments in many cases.
5 Existing studies typically focus on constructing broad measures of wealth changes for politicians in developed
countries since direct information on self-dealing is often unavailable (Albertus, 2019; Baturu, 2017; Diermeier
et al., 2005; Eggers and Hainmueller, 2009; Klašnja, 2015; Querubín and Snyder, 2013; Reinikka and Svensson,
2004). But those returns could come from a variety of sources. The prestige of public office can bring customers
to a politician’s side business or yield invitations for paid speeches, for example, which are less inherently un-
ethical income opportunities than self-dealing welfare benefits. For the small literature on developing countries
see Asher and Novosad (2018); Fisman et al. (2014); Foltz and Opoku-Agyemang (2015).
6 Consistent with an improvement in the implementation of NREGS, our estimates of self-dealing in a con-
text with plausibly high levels transparency and accountability are substantially lower than the Niehaus and
Sukhtankar (2013a) estimates from the early days of NREGS implementation when off-the-books corruption
2 Background

2.1 Village Council Elections in Uttarakhand

Village council presidents in Uttarakhand are directly elected every five years, most recently in 2014. Local elections are run by the State Election Commission of Uttarakhand, an independent body that sets the election date and monitors nominations and campaigns. Elections are widely perceived to be free and fair. Roughly 90% of elections for council president are contested, and over 90% of respondents to the 2006-2008 Rural Economic Development Survey say they feel free to vote as they desire. Uttarakhand is also somewhat uniquely free of the “politics of fear” (as modeled by Padró i Miquel, 2007) that arise in other parts of India from caste and religious conflict. The state has only a tiny population of the so-called Other Backward Classes, and there have not been large attempts to create a unified political identity among more disadvantaged castes or the relatively small Muslim community. The support of an ethnic community is thus less likely to insulate the president in Uttarakhand than elsewhere.

Aside from elections the president faces checks on her authority while in office. She is in theory monitored by the other members of the village council (gram panchayat), who are independently elected. The voters themselves can by law file a no-confidence motion against the president if one-quarter of adults in the village sign a petition against her. A district bureaucrat then convenes a village meeting where the president can be removed from office if a majority of villagers favor her recall.

2.2 National Rural Employment Guarantee Scheme

NREGS, the National Rural Employment Guarantee Scheme, is the world’s largest public-works program. The primary goal of the scheme is to provide a flexible safety net for rural households in times of need by offering an income transfer conditional on the willingness to perform manual work. The scheme was much easier to pull off.

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7 See e.g. The Indian Express, ‘Uttarakhand elections: Across the border; next door to UP, new caste calculus’, February 15, 2017.
8 In our survey of village council presidents in Uttarakhand, very few presidents report that they relied on a particular caste, party or religious group in their election campaign. See Figure 4 below. This is in stark contrast to India as a whole, where large fractions of candidates for the council presidency report in the 2006-2008 Rural Economic Development Survey (REDS) that they relied on a caste group for support.
labor at the minimum wage (Zimmermann, 2018). There are no further means tests (Dey et al., 2006; Government of India, 2018). Most projects are routine tasks, such as clearing bushes or digging holes, that do not create substantial public investment.

In theory, NREGS guarantees every rural household up to 100 days of public employment per year at the minimum wage, on demand whenever requested by the household. But in practice the program is supply- rather than demand-driven. In Uttarakhand as well as in other Indian states, excess demand for NREGS jobs is common. Households can only get employment when it is made available, rather than taking up work when they may need it most (Dutta et al., 2012; Mukhopadhyay et al., 2015). For example, many households report having to wait passively for jobs to be provided rather than actively applying for work.

The necessary rationing of employment due to excess demand gives the village council president a key role in the allocation of jobs among households in the area she governs (which is also called the gram panchayat, or panchayat for short). Aside from registering households and proposing local projects to block and district officials, the village council and president also effectively control the allocation of jobs. A worker who wants NREGS labor must apply at the council office. Though in theory a joint decision by the entire council, in practice council presidents make the decisions either themselves or jointly with their spouse. These allocations are then submitted to higher-level officials, who approve the wage payments. Since there are never enough jobs to meet villagers’ needs the council can exercise discretion in how jobs are allocated.

To create transparency the government now requires all NREGS related information to be entered into a software application called NREGASoft. The system contains multiple modules to track different aspects of the scheme, such as employment demanded by workers and jobs allo-

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9 See Berg et al. (2018), Imbert and Papp (2015) and Zimmermann (2018) for analyses of the economic impacts of the program.

10 Newspaper coverage on Uttarakhand is typically very negative, noting the low job availability and the absence of a well-functioning planning process at gram panchayats. Both are symptoms of a supply-driven NREGS implementation with a centralization of decision-making power in the village council president. Only 3 to 5 percent of households in Uttarakhand get the full 100 days of employment. See e.g. Live Mint, ‘MGNREGA, a cash transfer scheme?’, March 18, 2013; Mainstream Weekly, ‘Working of NREGA Voices from Panchayats’, April 2, 2009; Financial Express, ‘The state of MGNREGA performance: If inefficient states perform at par, huge gains are possible’, March 14, 2017. See also India Spend, ‘The Whys and Whats of India’s Rural Jobs Scheme’, November 4, 2014.

11 According to our own survey of council presidents.
cated, proposed and approved works projects, as well as modules for managing funds and labor budgets (Government of India, 2013). To cut down on corruption the Indian government opens bank accounts for NREGS beneficiaries and directly transfers wages for completed work into those accounts, cutting out middlemen who might pocket part of the payment. Additionally, job cards are now linked directly to each individual's Aadhar number, a national identification number linked to biometric markers. These changes have been shown to improve household benefits from the program, likely because it is more difficult to engage in hidden corruption through made-up work spells or underpayment of wages (Muralidharan et al., 2016).

In short, any NREGS payment must be reflected in the online system. The resulting records are fed in real-time to a publicly available website. That makes off-the-books corruption, as documented in the early days of NREGS, more difficult than open self-dealing. Aside from letting villagers, local media, NGOs and political challengers monitor the council president, this website is also the source of data for this study.

3 Data and Research Design

3.1 Data

We use publicly available administrative data on NREGS employment that we scraped from the official NREGS website, which is maintained by the Government of India. The dataset contains digital versions of the paper trail that is mandated by the scheme, which provides us with data on NREGS employment at a highly disaggregated level. Every registered job card has an online record with the details of the job card holder, typically the household head, and his or her family members. The household's district, block, panchayat, and village are recorded. The record also includes the name, gender, and age of every household member registered to work, as well as the start date and length of each job spell, wages paid, and the name of the project they worked on. Additionally, we have information on the name of the household head's father or husband, the household's broad caste category, and the date of initial registration for the job card that made the household eligible to work under NREGS.

12 https://nrega.nic.in
13 See Afridi and Iversen (2014); Niehaus and Sukhtankar (2013a,b).
We merge the NREGS data to publicly available information from the local election for the president from June 2014. The election dataset contains the name, closest male relative, and vote count of the winner and runner-up of each gram panchayat election. This information allows us to match the winner and runner-up to their NREGS job card profile. We attempt to match these top-two candidates for all elections decided by a margin of 7.5% or less, dropping any cases in which there is no unique match. We link this dataset to the 2011 Indian Census by collapsing statistics measured by census village to the level of the panchayat. We draw several variables from the Census, most importantly a categorical variable that reports whether the distance of the village to the nearest cyber cafe is less than 5 kilometers, more than 10 kilometers, or somewhere in between. We aggregate the census data to the level of the panchayat, then merge to our linked job card-election dataset.

To better understand the wages and motives of council presidents, we surveyed a sample of them by phone. We matched the winning candidates in our sample to contact information posted on the website of the Uttarakhand Ministry of Panchayati Raj. We assigned a random ordering to this sample and hired contractors in India to work down the list making calls in the month just before the 2018 monsoon season. The contractors made as many calls as possible in this period, yielding a final sample of 207 complete or partial interviews. The response rate was roughly 30 percent, where nonresponse arose mainly because our interviewer could not connect (likely because the phone was off or out of cell phone range). Conditional on someone picking up the response rate was close to 100 percent. The connection issues seem transient—several of those who could not be initially contacted were successfully interviewed when called later. We detect no statistically significant difference on observables between our survey sample and the presidents who were not surveyed, making differential non-response less likely to be a concern.

Table 1 reports summary statistics for four samples: all candidates that were successfully

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14 While the next local election took place in 2019, consistent detailed data on election results is not yet available. We therefore cannot analyze whether the presidents in our dataset participated in the 2019 election.

15 In the appendix we also use as controls the Census reports of distance to the district and sub-district headquarters, the literacy rate, and the fraction of the population classified as scheduled caste or tribe. We also use village geo-coordinates to measure the distance to the state capital, the distance to the state's border with Uttar Pradesh, and 2011 night-time light emissions.

16 During the first phase of the survey we had to modify the wording of some questions after our interviewers reported that respondents did not understand the original wording. As a result we do not have 207 responses for some questions.
matched to their NREGS records, the subset within the bandwidth used to estimate our main specification, the set of presidents within the full matched sample, and the subset in our survey sample. The samples are broadly similar on all characteristics except those that differ by construction (e.g. all winning candidates have a positive vote margin). As noted above there are no significant differences between the survey sample and the unsurveyed presidents in the matched sample. In particular, the two samples earn almost identical amounts of NREGS labor in the year after they become president (2015).

The one difference between samples that is both clear and not mechanical is that winning candidates worked somewhat more days of labor in 2015 (after the election) than the samples that include both winners and runners-up. This difference foreshadows our main result that winners receive more labor than losers.

### Table 1
Descriptive Statistics and Sample Sizes

<table>
<thead>
<tr>
<th></th>
<th>Winners and Runners-Up</th>
<th>Winners Only</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Full Matched Sample</td>
<td>In Bandwidth</td>
</tr>
<tr>
<td><strong>Winner</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.54</td>
<td>0.44</td>
</tr>
<tr>
<td></td>
<td>(0.50)</td>
<td>(0.50)</td>
</tr>
<tr>
<td><strong>Female</strong></td>
<td>0.47</td>
<td>0.47</td>
</tr>
<tr>
<td></td>
<td>(0.50)</td>
<td>(0.50)</td>
</tr>
<tr>
<td><strong>Scheduled Caste/Tribe</strong></td>
<td>0.21</td>
<td>0.21</td>
</tr>
<tr>
<td></td>
<td>(0.40)</td>
<td>(0.41)</td>
</tr>
<tr>
<td><strong>Vote Margin</strong></td>
<td>0.85</td>
<td>-4.57</td>
</tr>
<tr>
<td></td>
<td>(19.77)</td>
<td>(16.64)</td>
</tr>
<tr>
<td><strong>In Bandwidth</strong></td>
<td>0.82</td>
<td>1.00</td>
</tr>
<tr>
<td></td>
<td>(0.39)</td>
<td>(0.00)</td>
</tr>
<tr>
<td><strong>Surveyed</strong></td>
<td>0.13</td>
<td>0.10</td>
</tr>
<tr>
<td></td>
<td>(0.33)</td>
<td>(0.30)</td>
</tr>
<tr>
<td><strong>Days of labor (2015)</strong></td>
<td>40.10</td>
<td>36.30</td>
</tr>
<tr>
<td></td>
<td>(40.66)</td>
<td>(38.65)</td>
</tr>
<tr>
<td><strong>Days of labor (2013)</strong></td>
<td>23.13</td>
<td>23.95</td>
</tr>
<tr>
<td></td>
<td>(34.55)</td>
<td>(35.13)</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>1650</td>
<td>1351</td>
</tr>
<tr>
<td><strong>Panchayats</strong></td>
<td>1148</td>
<td>1003</td>
</tr>
</tbody>
</table>

*Note*: Each cell gives the mean and standard deviation of a characteristic of candidates for council president (rows) when conditioned on a specific subsample (column). “Full Matched Sample” is the set of all candidates we are able to find in the job card data. “In Bandwidth” is the subset whose vote margin falls within the bandwidth of our main specification. “All” is the subset of winning candidates within the full matched sample. “Surveyed” is the subset we were able to interview for our survey of council presidents.
3.2 Research Design

We estimate the causal effect of being the council president using the regression discontinuity induced by close elections. We restrict our sample to the winner and runner-up in each election. Let $i$ be one of these two candidates in the election for panchayat $p$. Our running variable is the vote margin, which we define as

$$[Margin]_{ip} = \begin{cases} 
\text{[Winner Votes]} - \text{[Runner-Up Votes]} & \text{if } i \text{ won election in } p \\
-\left(\text{[Winner Votes]} - \text{[Runner-Up Votes]}\right) & \text{if } i \text{ lost election in } p
\end{cases}$$

This definition generates a discontinuity at zero.\footnote{In practice, the official election law for Uttarakhand breaks ties by randomly drawing the name of the winner among candidates with the same number of votes and then adding a vote to the winner's vote count in the election records.} For our research strategy to identify a causal effect, any unobserved factors that are correlated with being council president must be continuous in the margin of votes. We therefore zoom in on a small window around the cutoff and control for a linear spline in the vote margin.

The continuity assumption holds if political candidates standing for election cannot perfectly manipulate the number of votes they receive. We verify the assumption with placebo tests using pre-determined outcomes that cannot be changed by the election.

We estimate:

$$[Outcome]_{ip} = \pi_0 + \pi_1 [Margin]_{ip} + \pi_2 [Margin]_{ip} \times [Win]_{ip} + \beta [Win]_{ip} + \nu_{ip}$$

where $[Win]_{ip}$ is a dummy for whether $[Margin]_{ip} > 0$ and $[Margin]_{ip}$ is restricted within a bandwidth centered on 0. We use the method suggested in Calonico et al. (2014) to choose the optimal bandwidth for our main specification, but also explore the robustness of our results to a wide range of alternative bandwidths.

4 Is Corruption-as-Compensation Plausible?

Observers like Ravallion (2018) have suggested that given its many administrative challenges, NREGS may in part be sustained by the prospect of corruption. He sketches a model in which
“There is a pecuniary benefit to the official that naturally depends on the level of employment. We can think of this as the official’s cut on the wages paid.” Our survey of council presidents gives reason to believe this hypothesis. Three questions were specifically designed to get presidents’ self-reported opinion on the plausibility of a corruption-as-compensation channel. We first asked whether a president can make special efforts to “bring back more jobs for their constituents.” As the top bar of Figure 2 shows, nearly all presidents agree with that statement. Consistent with the anecdotal accounts from Section 2, village council presidents are the key player in the introduction of NREGS. To measure whether these efforts should be rewarded, we measure whether a president answers yes to either of two “corruption as compensation” questions:

Do you believe a typical person in a village in your block would agree that a Gram Pradhan who makes those efforts deserves a few more NREGS jobs than the typical household in the village?

and

Suppose a Gram Pradhan in a typical village in your block manages to get a new work-site approved for his village. Would people in the village expect the Pradhan’s household to do NREGS labor on the newly approved worksite?

The bottom bar shows that a majority of council presidents agree with one or both of these
In our survey, village council presidents also claim that NREGS is transparent and that they feel accountable to voters. Figure 3 shows their responses to a number of survey questions asking whether villagers could reasonably be expected to be aware of NREGS allocations. Almost all respondents tell us that most villagers would know when someone in the village gets a NREGS job (left-hand panel of Figure 3). Every panchayat is supposed to keep paper records of the official “muster rolls,” which list all the workers who have received NREGS jobs on every project. Nearly all presidents say most panchayats in their block keep these records, and over two-thirds of presidents report that a villager will ask to check the muster rolls at least once per year (left-hand panel of Figure 3).

We also ask about the NREGS website. Over two-thirds of presidents believe most of the villagers in their panchayat know about the NREGS website (see right-hand panel of Figure 3). Although few presidents say most villagers in their panchayat have actually visited the site, over 80 percent say that at least a few have.

Lastly, we ask about the role of the Gram Rozgar Sahayak (GRS), the village technical assistant. Though the president decides how to allocate NREGS jobs, the majority of presidents re-
port that the GRS actually submits those decisions to the NREGS online system, and almost no presidents actually enter the information themselves (see right-hand panel of Figure 3). There is thus at least one district-level bureaucrat aware of the president's actions and in principle able to share her knowledge with villagers or district officers.

Figure 4 shows presidents' responses to questions about the accountability of a typical president to her villagers. The survey asked each respondent whether a typical president in her block would be formally or informally sanctioned for making NREGS allocations that are unacceptable to her constituents. The overwhelming majority said most presidents would be sanctioned (see left-hand panel of Figure 4). As the figure shows, presidents' beliefs on accountability are comparable to citizens' beliefs in the 2006-2008 REDS household survey that it is not difficult to hold local officials accountable. When asked what types of informal sanctions were likely, the most common answers were that the president would be confronted by angry villagers, suffer exclusion from social events, or even be threatened with violence. It is also unlikely that presidents maintain power by appealing to an ethnic group willing to ignore bad governance for the sake of keeping its own in power (e.g. Padró i Miquel, 2007). Less than 10 percent of council presidents in our sample report having relied on the support of a caste, religion or political party in their campaign for office.

Overall, the survey responses of the village council presidents therefore suggest that a corruption-as-compensation channel is plausible in the implementation of NREGS. Respondents claim that NREGS allocations are transparent, that presidents who make unpopular NREGS allocations would be held accountable, and that presidents who create more jobs would be expected to also take more jobs for themselves. At face value these responses imply that voters are aware of NREGS allocations and could punish a politician who is too greedy, but will not do so as long as self-dealing is proportional to the total program benefits created for the village.

5 Main Results: There is Self-Dealing

Most of the other presidents report that the decisions are submitted by the village secretary or some other assistant.

This is in stark contrast to India as a whole, where large fractions of candidates for the council presidency report in the 2006-2008 Rural Economic Development Survey (REDS) that they relied on a caste group for support.
5.1 How Much of Compensation Comes from Self-Dealing?

To test for corruption-as-compensation we must first test whether there is corruption. We estimate Equation 1 on candidates whose vote margin is within a bandwidth of 15 votes. As the election was in mid-2014 we test for a discontinuity in the total days of labor allocated to the household of the candidate in 2015. The left-hand panel of Figure 5 shows the regression line of best fit alongside the average days of labor earned by households whose candidate had each possible winning margin. The figure shows a large discontinuity when the margin switches from negative to positive—that is, when a candidate switches from barely losing to barely winning. The winner receives an extra 37 days of labor—nearly 3 times as many as the loser—suggesting she heavily favors her own household over others.

Panel A of Table 2 shows this estimate (in Column 1) together with several robustness checks. In some panchayats we were unable to match both the winner and runner-up to their job card record. These observations are included in the main specification, but in Column 2 we verify that...
Figure 5
Winners of Close Elections Receive 3 Times as Much Labor

Note: Standard errors are clustered by panchayat. The bin size is 1 vote. Each dot shows the average of the outcome within the bin.

For the result is robust to including only panchayats for which we are able to match both candidates. As noted in Section 3.2 we generally define the running variable as the margin of votes in levels. Column 3 verifies that defining the margin as a proportion of all votes cast does not qualitatively change the results.\(^{21}\) Columns 5—7 estimate Equation 1 for other outcomes. Column 5 shows that winners receive 3 more jobs than losers (who receive 2). Column 6 shows that winners are 37 percentage points more likely to have gotten a job at all in 2015. Column 7 shows that their NREGS payments are nearly 6000 rupees higher on average. According to both our survey of council presidents and newspaper reports from Uttarakhand,\(^{22}\) the median annual salary is 9000 rupees. Column 7 thus implies the president earns excess NREGS returns equal to nearly two-thirds of the official salary. Finally, as we show in Appendix A.2.3, we cannot reject that presidents elected in constituencies reserved for women or members of lower castes (Scheduled Castes and Scheduled Tribes) self-deal the same amount as presidents elected in unreserved seats.\(^{23}\)

\(^{21}\) Since this new running variable is on a completely different scale we calculate a different optimal bandwidth using the method of Calonico et al. (2014).


\(^{23}\) We also do not find evidence that presidents allocate excess NREGS benefits to extended family or other village
Table 2
Main Results

Panel A: Main Results

<table>
<thead>
<tr>
<th></th>
<th>Days of Labor</th>
<th>Other Outcomes</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) Basic</td>
<td>(2) Dual Matches</td>
</tr>
<tr>
<td>RD Estimate</td>
<td>37.402*** (4.464)</td>
<td>39.935*** (5.635)</td>
</tr>
<tr>
<td>Outcome at Disc.</td>
<td>20.99</td>
<td>21.17</td>
</tr>
<tr>
<td>Observations</td>
<td>1105</td>
<td>696</td>
</tr>
<tr>
<td>Panchayats</td>
<td>757</td>
<td>348</td>
</tr>
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</table>

Panel B: Placebo and Specification Tests

<table>
<thead>
<tr>
<th></th>
<th>Labor (2013)</th>
<th>SCT Cand.</th>
<th>Female Cand.</th>
<th>Name Length</th>
<th>Name Length (M. Rel.)</th>
<th>Matched?</th>
</tr>
</thead>
<tbody>
<tr>
<td>RD Estimate</td>
<td>-3.761 (4.262)</td>
<td>0.012</td>
<td>0.010</td>
<td>-0.287</td>
<td>0.216</td>
<td>0.028</td>
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<tr>
<td>Outcome at Disc.</td>
<td>26.85</td>
<td>0.18</td>
<td>0.47</td>
<td>10.46</td>
<td>8.61</td>
<td>0.32</td>
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<tr>
<td>Observations</td>
<td>1105</td>
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<td>1105</td>
<td>1105</td>
<td>1105</td>
<td>2400</td>
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<tr>
<td>Panchayats</td>
<td>757</td>
<td>757</td>
<td>757</td>
<td>757</td>
<td>757</td>
<td>1200</td>
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</table>

Panel C: Robustness to Bandwidth

<table>
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<tr>
<th></th>
<th>(1) h = 25</th>
<th>(2) h = 22.5</th>
<th>(3) h = 20</th>
<th>(4) h = 17.5</th>
<th>(5) h = 15</th>
<th>(6) h = 12.5</th>
<th>(7) h = 10</th>
<th>(8) h = 7.5</th>
</tr>
</thead>
<tbody>
<tr>
<td>RD Estimate</td>
<td>36.565*** (3.519)</td>
<td>36.094*** (3.791)</td>
<td>34.875*** (3.954)</td>
<td>36.583*** (4.186)</td>
<td>37.402*** (4.464)</td>
<td>37.615***</td>
<td>36.110***</td>
<td>32.689***</td>
</tr>
<tr>
<td>Observations</td>
<td>1467</td>
<td>1380</td>
<td>1336</td>
<td>1216</td>
<td>1105</td>
<td>898</td>
<td>752</td>
<td>472</td>
</tr>
<tr>
<td>Panchayats</td>
<td>1009</td>
<td>949</td>
<td>916</td>
<td>834</td>
<td>757</td>
<td>623</td>
<td>518</td>
<td>331</td>
</tr>
</tbody>
</table>

Note: “Outcome at Disc.” gives the estimate of the counterfactual outcome at the cutoff in the absence of treatment (that is, the left limit at the cutoff). Standard errors are clustered by panchayat. See text for description of each specification.
*p=0.10 **p=0.05 ***p=0.01

5.2 Are the Identification Assumptions Valid?

Our main result shows that the winner of the election gets more days of labor than the loser, but is it possible the difference arises only because the loser is given fewer days of labor than other households? Though punishing a political rival is clearly misconduct, it does not earn any financial return for the president. In jargon the question is whether the Stable Unit Treatment Value Assumption is violated. We test for a violation by reassigning every winning candidate the number of days earned by the average household in the panchayat (excluding both winning council members (Appendix A.2.2)).
and loser). If our estimates are driven by harm to the loser rather than benefit to the winner, this estimate should be similar to the estimate in Column 1 of Table 2.A. But Column 4 shows that the estimate is close to zero, suggesting losers are treated no differently than the typical household. This implies that one year of self-dealing by the politician amounts to roughly the same amount of NREGS benefits a typical villager receives in three years.

We then test the key assumption behind the regression discontinuity design, that the households of candidates who barely lose are similar to those who barely win in all ways except that they lost the election. Like much of the literature, we test the assumption by testing for discontinuities in pre-determined outcomes. Since the election was in 2014, winning or losing should not affect outcomes determined before 2014—for example, the number of days of labor allocated in 2013. Any discontinuity would suggest the type of household that received more labor in 2013 was able to sort itself onto the winning side of the cutoff (say, by manipulating the vote count).

The right-hand panel of Figure 5 estimates and plots Equation 1 in exactly the same way as was done to construct the left-hand panel, but using as the outcome the days of labor in 2013. There is no sign of a discontinuity. Columns 1—5 of Table 2.B report applying the same procedure to several other pre-determined outcomes. Column 1 is the same as Panel B of Figure 5. Column 2 tests for differences in whether the winner is a member of a scheduled caste or tribe (SCT), both historically disadvantaged groups. Column 3 tests for whether the winner is more or less likely to be a woman. Ideally we would also test other measures of income or social status, but the job card data are relatively sparse. One very rough measure of social status is the length of the candidate’s name, as higher caste candidates are likely to have a last name related to their caste (e.g. Kothari) whereas lower caste candidates tend to have “default” names that hide their caste (e.g. Devi). Columns 4 and 5 test for differences in the length of the winner’s own name and that of the closest male relative (husband or father). None of these placebo tests show a difference that is statistically or economically significant.

It is also common in the literature to apply a test for discontinuities in the empirical density of the running variable. But the density of vote margins is continuous (and actually symmetric) because every winner to the right of the cutoff has a loser to the left. Then a discontinuity in our matched dataset can only arise if it is systematically easier to make a match between the
election records and the job card data for winners. That is especially a concern if losers are less likely to get a NREGS job card, without which they would not even appear in the job card data. We test for whether there is a discontinuity in the match rate by taking the full set of candidates we attempted to match, restricting to the bandwidth of our main specification, and estimating Equation 1 on a dummy for whether the candidate was matched. Reassuringly, Column 6 suggests there is no discontinuity.  

Finally, we verify that the results are not sensitive to the choice of bandwidth. Table 2.C estimates Equation 1 for bandwidths ranging from as wide as 25 votes to as narrow as 7.5. The estimates are all similar.

5.3 Is it Necessarily Self-Dealing?

Is it possible that there is a more innocent explanation for why the president gets more days of NREGS labor than anyone else? For example, the president might be supervising the projects to make sure they are completed properly, and thus needs to be on nearly every project. But each NREGS project has an official work site supervisor, the “Mate,” and thus does not need an unofficial supervisor. The Mate is supposed to be chosen based on technical expertise that most presidents lack. Over 80 percent of presidents who answered our survey confirm that neither they nor any member of their household has served as a mate since the election. In any case, mates are paid directly for their labor through the project budget, not through NREGS labor.

The other innocent explanation is that the president is stepping in to keep work on NREGS projects continuing at times of the year when no one else needs employment. As noted in Section 2, demand for NREGS jobs generally far outstrips what is available. It is unlikely that there would have been a lack of interest in NREGS labor in 2015 when Uttarakhand suffered poor rainfall. But we can test this hypothesis directly by checking whether presidents take less excess labor during the season when NREGS demand tends to be highest. Not surprisingly, the overwhelming majority of presidents (83 percent) report that NREGS demand is highest during the

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24 Though this is the most direct test for the underlying source of any discontinuity in the density, we also find no evidence of bunching in the final matched running variable (results available on request).
25 Since the number of votes is discrete a fractional bandwidth is effectively rounded down.
dry season (rabi). But when we estimate Equation 1 separately on labor in the dry season and labor during the monsoon season, we find very similar estimates that lie within a single standard error of one another. There is no evidence to suggest the president’s own NREGS allocation varies by season.

6 Potential Mechanisms

6.1 Corruption-as-Compensation Appears Plausible in the Cross Section...

While the presidents in our survey give us a coherent explanation for the self-dealing, they may have wanted to present us with what they believe to be the “correct” or least problematic response. Our interviewers made it clear that they were not affiliated with the government (and respondents seemed to accept that, as evidenced by their tendency to make unsolicited complaints about how the government runs NREGS). They also did not know that we were linking their NREGS allocation records from the website with their election results and other information. Nevertheless, presidents may have been unwilling to honestly report behavior that could be deemed unethical or embarrassing, such as feeling unaccountable to their citizens or self-dealing without a good reason. We therefore turn to testing the plausibility of the corruption-as-compensation channel with our data.

The most straightforward prediction of this mechanism is that there should be a correlation between the level of self-dealing and the total NREGS employment provided in the village. Unfortunately our sample size is too small to directly test whether the regression discontinuity estimates are larger in villages with larger average allocations. We instead define 3 measures of corruption, which we use as the dependent variable in simple OLS regressions. Let $D_{it}$ be the days of NREGS labor allocated to household $i$ in year $t$, $I_p$ the set of households in panchayat $p$, $27$ For the monsoon season the discontinuity is 17.9 days with a standard error of 2.6, and for the dry season it is 16.0 with a standard error of 2.6. Regression output is available on request.
\( j \in I_p \) be the household of the president of \( p \), and \( \hat{j} \in I_p \) that of the runner-up. Define

\[
\begin{align*}
\text{[Corruption 1]}_p &= D_{j,2015} - \frac{\sum_{i \in I_p \setminus \{j\}} D_{i,2015}}{|I_p \setminus \{j\}|} \quad (2) \\
\text{[Corruption 2]}_p &= D_{j,2015} - D_{\hat{j},2015} \quad (3) \\
\text{[Corruption 3]}_p &= D_{j,2015} - D_{j,2013} \quad (4)
\end{align*}
\]

Corruption 1 is the difference between the president’s labor allocation and the average allocation to all other households in the panchayat. Corruption 2 is the difference between the allocation of the president and the runner-up in the election, which is only defined for panchayats where we are able to match both winner and runner-up to their job card record. Corruption 3 is the change in the president’s allocation from the year before the election (2013) to the year after (2015). When we restrict our sample to elections won by 12 or fewer votes, the means of these three measures are 38.2 for Corruption 1, 37.6 for Corruption 2, and 35.0 for Corruption 3. All are similar to the estimated discontinuity in the main specification reported in Table 2.

We test the corruption-as-compensation channel by measuring the correlation between these three measures and the aggregate per household labor generated for the panchayat. Since the prediction applies specifically to differences in labor that are not driven by observable aggregate factors we control for block fixed effects. Since blocks are relatively small, weather and access to markets will be similar within a block. More importantly, the NREGS budget is fixed for each block and distributed between panchayats by a block-level program officer. A president who manages to bring home more jobs relative to other presidents appealing to the same officer might be seen as performing well.

We estimate an OLS regression of each measure of corruption on a set of block fixed effects and the average labor for all households in the panchayat excluding the president’s own household (otherwise there would be a mechanical correlation). Though these are not regression discontinuity estimates, we nevertheless restrict to elections won by no more than 12 votes to exclude panchayats where elections are uncompetitive because, for example, one family monopolizes power.\(^\text{28}\) Columns 1—3 of Table 3 show that Corruption 1 and 3 both show a significant

\(^{28}\) We restrict to an even closer set of elections because unlike before we are comparing simple means rather than
positive correlation. Corruption 2 does not follow the pattern, but that may be because as noted earlier we cannot compute it for many panchayats, forcing us to drop roughly half the sample. These results are at least somewhat supportive of the prediction of corruption as compensation.

One may wonder whether block fixed-effects are adequate to control for aggregate factors other than the president’s performance. In unreported results we run a placebo test for whether corruption in 2015 is positively correlated with average labor in 2013. Since 2013 is reflective of the previous president’s performance it should not impact the pay of the current president, which is what we find.

### 6.2 ...but Does Not Withstand Closer Scrutiny in the Panel

But focusing solely on 2015 could be misleading because the NREGAsoft system, introduced in 2013, was still relatively new. The presidents elected in mid-2014 were the first whose NREGS allocations could be monitored through the website from the first day in office. Since local stakeholders may not have much prior experience using the system to check on their leaders, it is worth asking whether they tamp down on corruption in later years.

Figure 6 suggests they do. The left-hand panel shows difference-in-discontinuities estimates that measure the size of the discontinuity relative to its size in 2013. Since presidents only took office in mid-2014, 2015 is the first true post-election year (which is why it is our focus in Section 5). But 2015 is the high-point in self-dealing. By 2018 it has fallen by roughly one quarter.

If the corruption-as-compensation hypothesis were true, the decline in self-dealing should

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*Note: The panchayat is the unit of observation. “Avg. Labor” is average NREGS labor for all households in the panchayat excluding the pradhan. Standard errors are robust to heteroskedasticity. All regressions control for block fixed-effects.*

---

**Table 3**

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>Corruption 1</td>
<td>Corruption 2</td>
<td>Corruption 3</td>
</tr>
<tr>
<td>Avg. Labor (2015)</td>
<td>0.391**</td>
<td>-0.009</td>
<td>0.911***</td>
</tr>
<tr>
<td></td>
<td>(0.172)</td>
<td>(0.316)</td>
<td>(0.211)</td>
</tr>
<tr>
<td>Mean Outcome</td>
<td>38.3</td>
<td>37.9</td>
<td>35.1</td>
</tr>
<tr>
<td>Observations</td>
<td>478</td>
<td>274</td>
<td>478</td>
</tr>
</tbody>
</table>
Note: RD estimates of main specification calculated separately for each calendar year. Standard errors are clustered by panchayat.

be matched by a similar decline in performance. But the right-hand panel, which shows yearly averages of NREGS labor to regular households in the village actually rises from 2015 to 2018. The pattern seems inconsistent with the idea that voters face a trade-off between corruption and performance.

If the decline in corruption is somehow related to voters learning to monitor corruption, it should be stronger in villages with better infrastructure to monitor corruption. We proxy for better monitoring capacity with the distance to the nearest cyber cafe. Villagers with access to a cyber cafe might be more likely to access the NREGS website to check actual program benefits, for example because they can access the internet in the cafe, because having a cyber cafe is correlated with a stronger internet connection than may be available elsewhere, or because cyber cafes are likely to be located near a tech-savvy local population that creates more demand for such a service.

The availability of a cyber cafe, which is based on information from the Indian Census, is positively correlated with presidents in our survey reporting that most villagers know of the NREGS website and that most villagers have visited the NREGS website, although given the smaller sur-
survey sample size the estimate is noisy. In either case, it seems unlikely that the proxy would exclusively capture villagers’ direct monitoring of NREGS implementation. It may also proxy for the presence of journalists and local activists who can monitor a president’s behavior and publicize their findings among the village population and beyond. Similarly, the political opposition will likely find it easier to scrutinize the president’s behavior in office and should have an incentive to do so since it only narrowly lost a competitive election.

But our proxy does not necessarily have to imply that villagers themselves are monitoring the president’s behavior online.\textsuperscript{29}

Panel a of Figure 7 shows that average NREGS benefits were similar in villages close to and far from a cyber cafe and remain roughly constant over the studied time period. But Panel b, which shows the RD estimates of self-dealing, shows a very different pattern. Self-dealing is lower in villages closer to a cyber cafe and disappears entirely over time. The estimate for 2018 is even negative, implying that close election winners take fewer NREGS days than election losers. In villages far away from an internet cafe, on the other hand, self-dealing remains high and, with the exception of a temporary drop in 2017, at roughly the same magnitude. The divergence between performance and corruption in villages near cyber cafes contradicts the idea that corruption is compensation.

The year-by-year RD discontinuity graphs (Appendix Figures A1 and A2) confirm that these estimates are not a fluke. And we show in Appendix Table A1 that the decline in corruption near cyber cafes is robust to including a large number of control variables such as night lights, literacy, the fraction of low-caste (SC/ST) individuals, the distance to the state capital, the border, or district- and sub-district headquarters. This suggests that the heterogeneity by distance to internet cafe does not just proxy for other factors such as remoteness, better living conditions or higher economic growth.

Though not causal, this pattern suggests corruption arises through imperfect monitoring rather than to compensate performance. Corruption-as-compensation predicts that corruption should be no lower in villages with better monitoring because voters choose not to punish self-

\textsuperscript{29} While we cannot directly test whether presidents actually show up to work on the NREGS projects or just receive the benefits, we can rule out that presidents are able to hide self-dealing by making up phantom projects with no or very few other workers (Appendix Table A3).
dealing to induce the president to put more unobservable effort into running NREGS. If anything the link between corruption and performance should be stronger in these areas because better monitoring enables stricter adherence to the corruption contract. Figure 7 is inconsistent with this prediction.

6.3 A Potential Explanation of the Results

A plausible explanation of the empirical patterns needs to be consistent with three key results: First, self-dealing declines and ultimately disappears in villages close to cyber cafes, plausibly because local stakeholders can better monitor their president. By contrast, self-dealing persists in villages far from cafes where such monitoring is more difficult. Second, the average provision of NREGS jobs for villagers is the same irrespective of voters’ monitoring capacity. Third, there is a positive correlation between president performance and self-dealing. Though Table 3 showed only the correlation in a single year, Appendix Table A2 shows that this is robust at least in villages far from internet cafes.

These results are not consistent with a corruption-as-compensation explanation. The first key result suggests a role for imperfect monitoring of self-dealing by voters that is alleviated by a shorter distance to an internet cafe. But a simple model where voters have no ability to monitor performance or corruption cannot explain the second and third stylized facts.

Average NREGS employment may be similar in villages both close and far from cyber cafes because it is easier to monitor than self-dealing. Households perfectly observe their own NREGS allocation and can observe total employment by visiting physical worksites and talking to other villagers. Even without easy access to the NREGS website, voters can observe performance. But self-dealing may be harder to observe if the president does not actually work the jobs he is paid for and hides or manipulates the physical muster rolls. Then it would be hard to detect self-dealing without accessing the NREGS website.

But the correlation between average NREGS employment and self-dealing cannot be explained by a model where program performance is observable but self-dealing is not. Presidents should then maintain good performance while self-dealing as much as possible regardless of performance, breaking any correlation between the two. Instead, such a correlation might arise
**Figure 7**
Dynamic Effects by Internet Cafe Distance

a) There is No Decline in Average NREGS Jobs for Citizens...

![Graph showing average NREGS jobs for non-candidates by year and internet cafe distance]

b.) ...But a Steady Decline in Corruption in Villages Near Cyber Cafes

![Graph showing RD estimates for corruption by year and internet cafe distance]

*Note:* All standard errors are clustered by panchayat. All years are estimated simultaneously to allow for correlation in the coefficients. a.) Graphs show average NREGS days provided to villagers, not RD estimates. Cafe refers to the nearest internet cafe according to Census data. b.) Graphs show RD estimates separately for each calendar year. Cafe refers to the nearest cyber cafe according to Census data.
if there is an institutional constraint imposed by the project management software itself. Presidents have to create NREGS projects, assign workers to those projects, and then feed all of this information to the NREGASoft software that generates the information published on the website. Politicians can self-deal by adding their name to the muster rolls, but they can only add their name once. This means that presidents who want to self-deal more have to create more projects, leading to a correlation of self-dealing and NREGS performance.\(^\text{30}\)

This explanation—that self-dealing is hard to monitor but the reporting technology limits the amount of self-dealing to one per muster roll—can reconcile all three stylized facts. In such a scenario, the excess allocations presidents make to themselves could well be a form of second-best corruption rather than being completely wasteful. Even in areas with imperfect monitoring, institutional constraints would incentivize presidents to put in more effort without the tacit approval from voters. This is in contrast to the corruption-as-compensation explanation where voters grudgingly accept corruption in return for better program implementation.

7 Conclusion

Using a unique dataset, our paper tests whether local politicians use excess welfare benefits that they allocate to themselves as compensation for a better implementation of a major welfare program. While the politicians themselves claim that such behavior is reasonable and expected in their area, our results do not support such a mechanism. We find that village council presidents receive three times the benefits of a typical villager in the year after the election. While village-level jobs under NREGS remain constant or even rise over time, a better monitoring capacity in villages close to an internet cafe appears to lead to the eradication of self-dealing. In contrast, self-dealing remains high in areas where monitoring is plausibly more difficult, although most presidents do not exploit all corruption possibilities. While we cannot provide causal evidence of the exact explanation, the easiest explanation is that institutional constraints and the working of existing transparency and accountability mechanisms put an upper bound on corruption.

Our results suggest that completely eliminating self-dealing may require additional invest-

\(^{30}\) Alternatively, the detection probability may increase rapidly when it becomes a large enough proportion of total NREGS benefits. This would incentivize presidents to keep self-dealing at a small percentage of overall NREGS benefits (‘needle in the haystack’).
ments in monitoring capacity such as access to technology as well as in creating the socio-economic conditions that allow citizens to use those tools to effectively hold politicians accountable. Transparency and accountability are already unusually high in our context when compared to other contexts in developing countries. This is the combined result of large-scale reforms to the implementation of NREGS by the Indian government, Uttarakhand’s local institutions and geographical position which creates small villages, and our focus on competitive elections determined by a few votes. But even here, the simple availability of information in close to real time and the ability to hold a politician accountable alone do not seem to be enough to eradicate corruption. Citizens and other players like the media or local NGOs may also have to be better enabled to pro-actively monitor program implementation.
References


JEONG, SHENOY, AND ZIMMERMANN


A Empirical Appendix (For Online Publication)

A.1 Additional Graphs and Tables

The results in this sub-section provide additional results or robustness checks for the results presented in the main paper.

Table A1
Robustness of Internet Cafe Results

<table>
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<th>(4)</th>
<th>(5)</th>
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<tr>
<td>Days of Labor for President</td>
<td></td>
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<td></td>
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</tr>
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<td>(4.203)</td>
<td>(4.270)</td>
<td>(4.312)</td>
<td>(4.401)</td>
<td>(4.657)</td>
<td></td>
</tr>
<tr>
<td>Avg. Days</td>
<td>1.199***</td>
<td>1.175***</td>
<td>1.188***</td>
<td>1.192***</td>
<td>1.457***</td>
</tr>
<tr>
<td>(0.110)</td>
<td>(0.111)</td>
<td>(0.112)</td>
<td>(0.110)</td>
<td>(0.139)</td>
<td></td>
</tr>
<tr>
<td>2011 Night Lights</td>
<td>-0.672</td>
<td>-0.539</td>
<td>-0.540</td>
<td>-1.102*</td>
<td></td>
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<tr>
<td>(0.409)</td>
<td>(0.429)</td>
<td>(0.467)</td>
<td>(0.642)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance to Border</td>
<td>-0.089</td>
<td>-0.099</td>
<td>-0.324</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(0.092)</td>
<td>(0.094)</td>
<td>(0.418)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance to Dehradun</td>
<td>0.104***</td>
<td>0.110***</td>
<td>0.278</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(0.039)</td>
<td>(0.041)</td>
<td>(0.377)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance to Sub-Dist HQ</td>
<td>0.108</td>
<td>0.103</td>
<td>0.199</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(0.097)</td>
<td>(0.097)</td>
<td>(0.129)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance to District HQ</td>
<td>-0.013</td>
<td>-0.021</td>
<td>-0.187*</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(0.050)</td>
<td>(0.052)</td>
<td>(0.109)</td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Literacy</td>
<td>5.642</td>
<td></td>
<td>11.268</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(29.014)</td>
<td></td>
<td>(35.173)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SCT Fraction</td>
<td>-12.179</td>
<td>-4.814</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(10.405)</td>
<td></td>
<td>(10.058)</td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Constant</td>
<td>25.218***</td>
<td>27.722***</td>
<td>17.598**</td>
<td>17.260</td>
<td>11.147</td>
</tr>
<tr>
<td>(2.933)</td>
<td>(3.343)</td>
<td>(8.911)</td>
<td>(22.129)</td>
<td>(51.200)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>340</td>
<td>340</td>
<td>340</td>
<td>340</td>
<td>340</td>
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<tr>
<td>Block FEs</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: Cafe refers to the nearest internet cafe according to Census data. Standard errors are robust to heteroskedasticity.
*p=0.10 **p=0.05 ***p=0.01
Figure A1
RD Results for Long Internet Cafe Distance

a. Cafe Far Away (>5km)

Note: Figure shows the RD discontinuity graphs for each calendar year starting with the election year (2014). Standard errors are clustered by panchayat.
**Figure A2**

RD Results for Short Internet Cafe Distance

b. Cafe Nearby (<5km)

Note: Figure shows the RD discontinuity graphs for each calendar year starting with the election year (2014). Standard errors are clustered by panchayat.
Table A2
Dynamic Effects by Cafe Distance

<table>
<thead>
<tr>
<th></th>
<th>2012</th>
<th>2013</th>
<th>2014</th>
<th>2015</th>
<th>2016</th>
<th>2017</th>
<th>2018</th>
</tr>
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<tbody>
<tr>
<td>Non-Candidate Average Days</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Main Effect</td>
<td>0.150</td>
<td>-0.104</td>
<td>0.529***</td>
<td>0.424***</td>
<td>0.320**</td>
<td>0.363**</td>
<td>0.522***</td>
</tr>
<tr>
<td></td>
<td>(0.136)</td>
<td>(0.154)</td>
<td>(0.156)</td>
<td>(0.157)</td>
<td>(0.149)</td>
<td>(0.154)</td>
<td>(0.140)</td>
</tr>
<tr>
<td>× Near Cafe</td>
<td>0.012</td>
<td>0.326</td>
<td>-0.031</td>
<td>-0.182</td>
<td>0.106</td>
<td>0.099</td>
<td>-0.325**</td>
</tr>
<tr>
<td></td>
<td>(0.322)</td>
<td>(0.259)</td>
<td>(0.204)</td>
<td>(0.186)</td>
<td>(0.140)</td>
<td>(0.183)</td>
<td>(0.158)</td>
</tr>
<tr>
<td>Observations</td>
<td>2863</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Panchayats</td>
<td>409</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fixed-Effects</td>
<td>Block-Year</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*Note: All coefficients in the table come from a single regression. Standard errors are clustered within panchayat.*

Though Figure 7 suggests there is no link between self-dealing and the number of jobs generated for the village, it is possible that the average estimates hide a correlation like that found in Table 3. Table A2 jointly estimates the correlation in each year and for villages that are close versus far from a cyber cafe. The upper half of the table shows the estimated coefficients on self-dealing from interacting each year between 2012 and 2018 with the average NREGS days created in the village. The lower half of the table estimates the triple interaction effect of calendar year and village-level NREGS benefits with an indicator variable for a distance of less than 5 kilometers to the nearest internet cafe.

If access to the internet allows voters to better observe both self-dealing and performance, we would expect the post-election coefficients (2014 to 2018) for the triple-interaction effects to be positive and significant, whereas the main effects should be small or close to zero. Instead, the results show the opposite. With one exception, the interaction effects of the post-election years (2014 to 2018) with closeness to a cafe are insignificant. 3 out of 5 coefficients, including the only statistically significant estimate in 2018, are also negative instead of positive. Instead, the main effects are typically large compared to the interaction effects and highly statistically significant. The overall pattern is therefore not consistent with internet access allowing voters to better observe both self-dealing and performance.
A.2 Extensions of the Analysis

A.2.1 Are Presidents Creating Phantom Projects?

Potentially the easiest way of self-dealing is to create phantom projects. For example, the president might assign himself to projects with very few other workers, or even to projects where he is the only worker. Such tricks would lower the probability that voters can detect self-dealing, at least at the physical worksites, since villagers would not be able to directly observe who gets jobs on these projects. If presidents are using this maneuver, we would expect the average number of workers on projects worked on by the president to be significantly lower than those worked on by the runner-up candidates in the election. We test this below using the NREGS project as the unit of analysis. Among the set of projects worked on by either winner or runner-up (but not both) we assign the running variable of the candidate. The results show that there is no significant discontinuity in the number of workers or in the fraction that are single-person projects (which in any case are very rare).

<table>
<thead>
<tr>
<th></th>
<th># Workers</th>
<th>Solo Project</th>
</tr>
</thead>
<tbody>
<tr>
<td>RD Estimate</td>
<td>3.637</td>
<td>-0.003</td>
</tr>
<tr>
<td>(3.021)</td>
<td>(0.003)</td>
<td></td>
</tr>
<tr>
<td>Outcome at Disc.</td>
<td>19.900</td>
<td>0.003</td>
</tr>
<tr>
<td>Observations</td>
<td>1404</td>
<td>1404</td>
</tr>
<tr>
<td>Panchayats</td>
<td>607</td>
<td>607</td>
</tr>
</tbody>
</table>

*Note: “Outcome at Disc.” gives the estimate of the counterfactual outcome at the cutoff in the absence of treatment (that is, the left limit at the cutoff). Standard errors are clustered by panchayat. *p=0.10 **p=0.05 ***p=0.01*

A.2.2 Greed or Patronage?

Since political factions in India often coalesce around powerful families (George and Ponattu, 2019), we test whether presidents reward their extended family. For each president’s household, we know the name of the household head, who is almost always a man. The household head could be a male president himself, a female president’s husband, the president’s father, or the president’s father-in-law. For all other households, we know the closest male relative of
the household head, which we use as a proxy for extended family. We define a household as extended family to the president’s household if it lists the president’s household head as closest male relative. We assign these family members the vote margin of their contesting relative (excluding cases where the winner and runner-up are part of the same extended family).

**Figure A3**
Extended Family Does Not Get Any Extra NREGS Labor

![Figures showing discontinuity in NREGS labor days for different bandwidths.](image)

*Note: Standard errors are clustered by panchayat.*

**Table A4**
Regression Specifications Shown in Figure A3

<table>
<thead>
<tr>
<th></th>
<th>(1) BW=15</th>
<th>(2) BW=12.5</th>
<th>(3) BW=10</th>
</tr>
</thead>
<tbody>
<tr>
<td>RD Estimate:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>–Candidate</td>
<td>35.607***</td>
<td>35.790***</td>
<td>33.999***</td>
</tr>
<tr>
<td></td>
<td>(4.662)</td>
<td>(5.382)</td>
<td>(5.729)</td>
</tr>
<tr>
<td>–Family</td>
<td>14.623**</td>
<td>9.635</td>
<td>2.625</td>
</tr>
<tr>
<td></td>
<td>(6.423)</td>
<td>(6.332)</td>
<td>(6.974)</td>
</tr>
<tr>
<td>Outcome at Disc.</td>
<td>22.257</td>
<td>22.430</td>
<td>24.180</td>
</tr>
<tr>
<td>Observations</td>
<td>2422</td>
<td>1914</td>
<td>1521</td>
</tr>
<tr>
<td>Panchayats</td>
<td>725</td>
<td>595</td>
<td>494</td>
</tr>
</tbody>
</table>

*Note: “Outcome at Disc.” gives the estimate of the counterfactual outcome at the cutoff in the absence of treatment (that is, the left limit at the cutoff). Standard errors are clustered by panchayat.

*p=0.10  **p=0.05  ***p=0.01*

Figure A3 is drawn analogously to Figure 5, but showing NREGS days allocated to extended family. These estimates are more sensitive to the choice of bandwidth than our estimates from
Figure 5, so we present the same regression for three different choices of bandwidth. Though at the widest bandwidth (left panel) the estimate is positive, it is clearly an artifact of a bandwidth that is too wide. The estimate shrinks to insignificance at narrower choices of bandwidth (center and right panel), and the magnitude of the estimated discontinuity shrinks to almost zero.

Table A4 shows the regression estimates of Figure A3—estimates of excess labor for the extended family—alongside the estimates for the household of the candidates themselves (analogous to the estimates in Panel C of Table 2). We estimate both discontinuities simultaneously to correct for correlation in the coefficients. The estimates confirm that excess payments to family members shrink to insignificance as we shrink the bandwidth while those for the candidate remain unchanged. That suggests it is only the council president who receives extra NREGS labor, not her extended family.

Another form of patronage is to buy the complicity of officials who could otherwise check the president’s power. Since the village council is in principle the most likely check, we asked each president in our survey to name the three most senior members of the council to test whether these members are disproportionately likely to receive large NREGS transfers. But we find no evidence that the council members are more likely to appear among the biggest NREGS recipients than would be expected by chance. In summary, there is no evidence that presidents reward supporters or form conspiracies with other politicians to self-deal NREGS jobs.

A.2.3 Does Reservation Affect the Size of Outside Payments?

Some prior work has proposed that between-group conflict can allow rent-seeking leaders to remain in power because their group fears that removing them will allow the other group to take power (Padró i Miquel, 2007). Conversely, some studies have found that reducing between-group conflict through caste reservation can induce better political selection (Munshi and Rosenzweig, 2008). Meanwhile, there is a body of work suggesting that female leaders in India govern better on some measures, but that the traditionally male-dominated system of politics in India effectively selects out these female leaders (Clots-Figueras, 2011; Chattopadhyay and Duflo, 2004).

Our data lets us test for whether villages selected for caste or gender reservation attract lead-
ers who extract fewer excess days of NREGS labor. Table A5 shows that although this interaction term is negative for both forms of reservation, it is small and statistically insignificant. Self-dealing is 35 days under Female Reservation versus 39 days in panchayats not reserved for women (Column 1), and 36 days under Caste Reservation versus 37.5 days in other panchayats (Column 2).

**Table A5**
Reservations and Outside Payments

<table>
<thead>
<tr>
<th></th>
<th>(1) Female Reservation</th>
<th>(2) Caste Reservation</th>
</tr>
</thead>
<tbody>
<tr>
<td>RD Estimate</td>
<td>39.298***</td>
<td>37.412***</td>
</tr>
<tr>
<td></td>
<td>(6.320)</td>
<td>(4.910)</td>
</tr>
<tr>
<td>RD Estimate (Interaction)</td>
<td>-4.342</td>
<td>-1.580</td>
</tr>
<tr>
<td></td>
<td>(8.883)</td>
<td>(11.230)</td>
</tr>
<tr>
<td>Outcome at Disc.</td>
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<td>20.010</td>
</tr>
<tr>
<td>Observations</td>
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<td>1105</td>
</tr>
<tr>
<td>Panchayats</td>
<td>757</td>
<td>757</td>
</tr>
</tbody>
</table>

*Note:* “Outcome at Disc.” gives the estimate of the counterfactual outcome at the cutoff in the absence of treatment (that is, the left limit at the cutoff). Standard errors are clustered by panchayat.

*p=0.10 **p=0.05 ***p=0.01