Earnings and Takings: Self-Dealing in Local Public Office in India

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Abstract

Models of political selection predict that when public office enables lucrative self-dealing it attracts politicians motivated by greed. But since self-dealing is typically unobserved there are few good estimates of its magnitude. Linking millions of job records to election outcomes, we estimate the returns earned by Indian village council presidents from self-dealing public-works jobs. We find that the monetary benefits are large. Winners of close elections receive 3 times as many workdays as losers, yielding extra wages of two-thirds of the median president's official salary. We show suggestive evidence consistent with the idea that these returns induce negative political selection.

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1 Introduction

The theory of political selection predicts that if public office affords the chance to self-deal public funds, it may disproportionately attract candidates motivated by greed over altruism (Besley, 2005; Caselli and Morelli, 2004). But the returns from self-dealing are understandably difficult to measure with precision. Though officeholders are known to accumulate wealth more rapidly than can be explained by their official salary,[1] these "unofficial" returns to public office need not arise from self-dealing. For example, the prestige of public office can bring customers to a politician's side business or yield invitations for paid speeches. But the returns from these sources may not induce the same negative political selection as the inherently unethical returns from self-dealing. Only a direct test—one that shows whether there are substantial returns from self-dealing—can establish whether a society will attract leaders whose motives are aligned with its own.

This paper directly measures self-dealing by village council presidents in the Indian state of Uttarakhand. The National Rural Employment Guarantee Scheme (NREGS) funds short-term make-work jobs building public works within the village. Village council presidents make the key decision in allocating these jobs on behalf of the central government. We test whether village council presidents award disproportionately large labor quotas to their own households.

Although self-dealing is typically unobserved, we can measure it in NREGS by creating a unique dataset. Every employment spell granted through the scheme is reported on a publicly accessible website that identifies the recipient by name and location. Each report gives the dates worked, the wages received, and the project worked on. We scrape millions of these reports, covering over 90 percent of rural households in Uttarakhand. 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, a direct measure of self-dealing.

But this comparison alone is unlikely to give a clean estimate because election winners differ from typical villagers on a number of observable and unobservable characteristics. We mitigate

[1] See, for example: Albertus (2019); Baturo (2017); Diermeier et al. (2005); Eggers and Hainmueller (2009); Fisman et al. (2014); Klašnja (2015); Querubín and Snyder (2013); Reinikka and Svensson (2004)

this concern by comparing labor allocations between winners and runners-up in close elections decided by a few votes. Our regression discontinuity design ensures winning and losing candidates have similar observable and unobservable characteristics, making the labor quota of the runner-up a good counterfactual for that of the winner had she remained out of office.

We find that winners of close elections receive nearly 3 times as many days of labor as losers in the year after the election. The result does not seem to be driven by punishment of the loser by the winner. Losers receive roughly the same allocation as the typical household, and much the same allocation as they received in the year before the election. Instead, the difference is entirely explained by an increase in allocations to the winner. The winner's excess payouts sum to 6000 rupees in a year, roughly two thirds of the median official returns to office.

This increase is unlikely to have an innocent explanation. We find no evidence that the winner is taking jobs to supervise projects or working to complete projects at times when market work is plentiful and other households generally do not want government jobs. Workers were likely particularly desperate in the year after the election, when Uttarakhand suffered a drought. Jobs allocated to the president were likely coming at the expense of vulnerable households and thus unpopular. In an original survey of roughly 200 village presidents, the presidents themselves tell us that most villagers would find it unusual or upsetting if a president prioritized their own family in assigning program benefits.[2]

We then present evidence suggestive of how the prospect to self-deal affects political selection. We first test whether a politician's extended family or other members of the local council get extra jobs, which might imply presidents use their power as much to maintain a system of patronage as for personal enrichment. We find no evidence of either form of patronage, giving no reason to expect any differential selection towards those skilled in its use. Next we compare presidents elected under caste or gender reservation to other presidents. Despite prior work suggesting reservation can induce better political selection(e.g. Munshi and Rosenzweig, 2008; Chattopadhyay and Duflo, 2004), we find no evidence that these presidents are less likely to self-deal. Nevertheless there is heterogeneity in self-dealing, and politicians who self-deal the most are most likely to report in our survey that they plan to seek re-election. While not causal, this

[2] This is consistent with Chauchard et al. (Forthcoming), who find that Indian voters interpret a rapid wealth accumulation of politicians as signals for corruption and higher willingness to use violence.

pattern could be consistent with politicians least willing or least able to reap unethical returns selecting out of public office.

While high, our estimates of the returns from self-dealing are likely to be lower bounds on those available to local officials in a typical developing country. First, it is much easier to observe self-dealing in NREGS than is typical for a public program anywhere in the world. The government website that logs the employment spells is publicly accessible, allowing anyone who is interested to look up all the present and past jobs and to compare them across households. Since it is a public-works program, worksites around the village are also 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.

Second, 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 means that we are excluding villages where elite capture or nonexistent political opposition have rendered the president unaccountable to voters. Instead, in this context political opponents should have a higher incentive to monitor the president's behavior, and losing the support of a few voters may lead to defeat in the next election. Presidents are also, at least in theory, overseen by an independently elected village council and can be recalled by petition.

Our results contribute to the literatures on political selection and on how politicians behave in office.[3] The bulk of this work measures how changes in official compensation change political selection (Besley, 2004, 2005; Braendle, 2015; Carnes and Hansen, 2016; Di Tella and Fisman, 2004; Ferraz and Finan, 2011; Fisman et al., 2015; Kotakorpi and Poutvaara, 2011; Messner and Polborn, 2004). A few studies measure outside payments from other jobs held while in office, returns that are neither illegal nor unethical (Becker et al., 2009; Gagliarducci et al., 2010). The studies closest to our own construct broad measures of unofficial returns by measuring changes in the wealth of politicians (Albertus, 2019; Baturo, 2017; Diermeier et al., 2005; Eggers and Hainmueller, 2009; Fisman et al., 2014; Klašnja, 2015; Querubín and Snyder, 2013;

[3] For a recent literature overview see Dal Bo and Finan (2018).

Reinikka and Svensson, 2004).[4] But some sources of unofficial returns, such as better job opportunities that come with the visibility of public office, may induce different political selection than self-dealing, particularly if they can be expected to continue for a long time even after politicians leave office.

The developing country context is also severely understudied in this literature, even though self-dealing and unofficial returns from public office more broadly may be especially attractive in developing countries. To our knowledge the only paper that has attempted to estimate unofficial returns in a developing country focuses on changes in assets over time (Fisman et al., 2014). In contrast, we directly measure the returns to self-dealing that politicians derive while in office, and show suggestive evidence that self-dealing may induce a negative change in the pool of political candidates over time.

Our paper also relates to the literature on corruption.[5] While self-dealing in our context is not illegal, it falls under the most widely used definition of corruption as the misuse of public office for private gain. While the existing literature tends to focus on leakages,[6] in these contexts, it is usually unclear where exactly the money goes, for example whether bureaucrats or politicians take the biggest cut, or whether a politician keeps the money for himself or uses it to reward supporters. One strength of our context is that payments from NREGS land directly in the bank account of the politician. The evidence suggests presidents use their power mainly for personal enrichment, and that fact may require a reevaluation of the costs of corruption. If the chance to self-deal induces negative political selection, the total costs of corruption may be much larger than the direct monetary costs from program leakages.

Finally, our analysis adds some nuance to the existing literature. While it is typically assumed that a dishonest political class arises because the prospect of self-dealing draws dishonest politicians into office, in our context honest politicians may simultaneously be squeezed out of office. The official returns to office in Uttarakhand are about half of the median household income, implying an honest politician may take a sizable pay-cut and might even be unable to afford

[4] Some papers focus on proxies for effort like absenteeism rates of elected politicians, which may imply more time and effort spent on unofficial returns to office (Galasso and Nannicini, 2011; Gavoille, 2018; Mocan and Altindag, 2013).

[5] For a literature overview see Olken and Pande (2012).

[6] See Avis et al. (2018); Di Tella and Schargrodsky (2003); Ferraz and Finan (2008); Niehaus and Sukhtankar (2013b); Olken (2007)

holding elected office. Even if there is a vast reservoir of honest and public-spirited citizens, they may be effectively barred from office.

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.[7] 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 his 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

[7] See e.g. The Indian Express, 'Uttarakhand elections: Across the border; next door to UP, new caste calculus', February 15, 2017.

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.[8] 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.[9] 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 allocated, 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,

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

[9] According to our own survey of council presidents.

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.[10] That makes off-the-books corruption, as documented in the early days of NREGS[11], more difficult than open self-dealing. Aside from letting villagers 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 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.

We merge the NREGS data to publicly available information from the last 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

[10] https://nrega.nic.in

[11] See Afridi and Iversen (2014); Niehaus and Sukhtankar (2013a,b).

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 merge the panchayat-level data 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.[12] 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 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

[12] 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.

winners receive more labor than losers.

Table 1
Descriptive Statistics and Sample Sizes

	Winners and Runners-Up		Winners Only	
	Full Matched Sample	In Bandwidth	All	Surveyed
Winner	0.54	0.44	1.00	1.00
	(0.50)	(0.50)	(0.00)	(0.00)
Female	0.47	0.47	0.48	0.43
	(0.50)	(0.50)	(0.50)	(0.50)
Scheduled Caste/Tribe	0.21	0.21	0.20	0.21
	(0.40)	(0.41)	(0.40)	(0.41)
Vote Margin	0.85	-4.57	13.96	14.65
	(19.77)	(16.64)	(11.63)	(14.37)
In Bandwidth	0.82	1.00	0.66	0.65
	(0.39)	(0.00)	(0.47)	(0.48)
Surveyed	0.13	0.10	0.23	1.00
	(0.33)	(0.30)	(0.42)	(0.00)
Days of labor (2015)	40.10	36.30	57.19	57.40
	(40.66)	(38.65)	(42.15)	(39.00)
Days of labor (2013)	23.13	23.95	22.08	26.59
	(34.55)	(35.13)	(34.27)	(37.71)
Observations	1650	1351	887	207
Panchayats	1148	1003	887	207

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

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

This definition generates a discontinuity at zero.[13] For our research strategy to identify a

[13] 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.

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:

$$[Outcome]_{ip} = \pi_0 + \pi_1[Margin]_{ip} + \pi_2[Margin]_{ip} \times [Win]_{ip} + \beta[Win]_{ip} + \nu_{ip} \tag{1}$$

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 Main Results: There is Self-Dealing

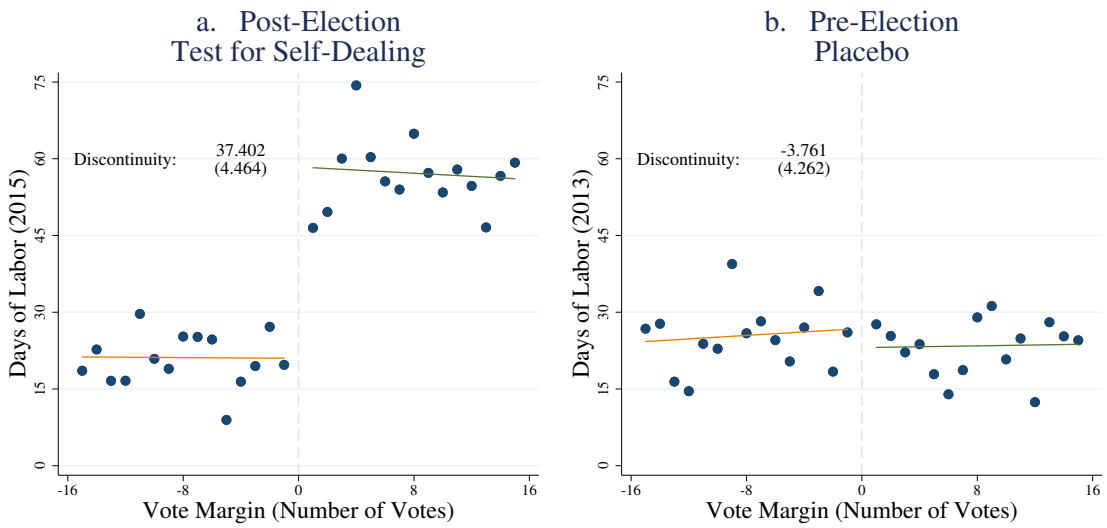
4.1 How Much of Compensation Comes from Self-Dealing?

We estimate Equation 1 on candidates whose vote margin is within a bandwidth of 15 votes.[14] 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 1 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

[14] Unless otherwise specified we use this same bandwidth as we test other outcomes or specifications to avoid conflating the effect of changing specifications with the effect of changing the bandwidth. But the results are qualitatively similar when we vary the bandwidth.

Figure 1
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.

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.[15] 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,[16] 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. Given that NREGS is only one of the budget items under the president's control, this estimate is almost certainly a lower bound on the total returns from self-dealing.

[15] 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).

[16] National Herald, accessed on 26 July 2019. https://www.nationalheraldindia.com/national/5000-gram-pradhans-resign-after-ukhand-slashes-gram-sabha-funds

Table 2
Main Results

Panel A: Main Results

	Days of Labor				Other Outcomes		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Basic	Dual Matches	Vote Share as RV	Test: SUTVA	# of Jobs	Any Job?	NREGS Payments
RD Estimate	37.402***	39.935***	24.781***	-0.043	3.305***	0.373***	5957.528***
	(4.464)	(5.635)	(6.777)	(2.736)	(0.419)	(0.053)	(708.960)
Outcome at Disc.	20.99	21.17	23.90	20.99	1.97	0.54	3333.89
Observations	1105	696	400	1105	1105	1105	1105
Panchayats	757	348	283	757	757	757	757

Panel B: Placebo and Specification Tests

	(1)	(2)	(3)	(4)	(5)	(6)
	Labor (2013)	SCT Cand.	Female Cand.	Name Length	Name Length (M. Rel.)	Matched?
RD Estimate	-3.761	0.012	0.010	-0.287	0.216	0.028
	(4.262)	(0.042)	(0.044)	(0.312)	(0.409)	(0.032)
Outcome at Disc.	26.85	0.18	0.47	10.46	8.61	0.32
Observations	1105	1105	1105	1105	1105	2400
Panchayats	757	757	757	757	757	1200

Panel C: Robustness to Bandwidth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$h = 25$	$h = 22.5$	$h = 20$	$h = 17.5$	$h = 15$	$h = 12.5$	$h = 10$	$h = 7.5$
RD Estimate	36.565***	36.094***	34.875***	36.583***	37.402***	37.615***	36.110***	32.689***
	(3.519)	(3.791)	(3.954)	(4.186)	(4.464)	(5.088)	(5.386)	(6.625)
Outcome at Disc.	22.13	21.10	20.99	20.89	20.99	19.73	20.22	20.24
Observations	1467	1380	1336	1216	1105	898	752	472
Panchayats	1009	949	916	834	757	623	518	331

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

4.2 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 winner 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.

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 1 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 1. 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.[17]

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.[18] The estimates are all similar.

4.3 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 in 2015, when Uttarakhand suffered poor rainfall, that there would be a lack of interest in NREGS labor. 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 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.[19] There is no evidence to suggest the president's own NREGS allocation

[17] 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).

[18] Since the number of votes is discrete a fractional bandwidth is effectively rounded down.

[19] 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.

varies by season.

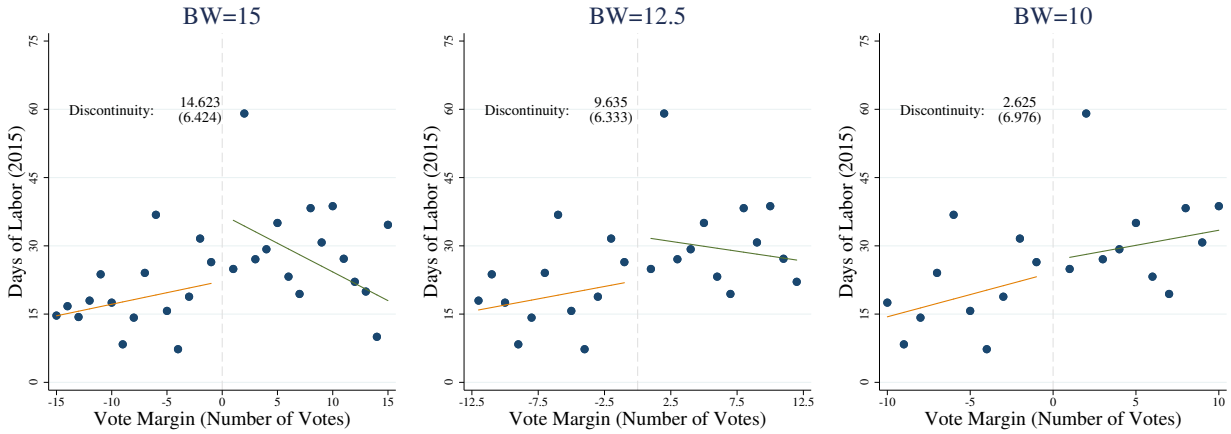
5 Implications for Political Selection

5.1 Greed or Patronage?

Our results so far show robust evidence that the returns from self-dealing of NREGS jobs are large. Though the main goal of this study is to measure the returns to self-dealing, here we return to our motivation by considering what the patterns in our data suggest these returns imply about political selection. We first assess whether council presidents use their power solely for personal enrichment, or if they also use it to reward allies and other enablers. The latter case might imply the political institutions attract leaders prone not only to greed but patronage, meaning they build a political faction by promising rewards to their supporters.

Figure 2

Extended Family Does Not Get Any Extra NREGS Labor



Note: Standard errors are clustered by panchayat.

Since political factions in India often coalesce around powerful families (George and Ponattu, 2019) the most natural test is whether presidents reward their extended family, which we define as households having the same father or husband as the head of the president's household. 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 2 is drawn analogously to Figure 1, but showing NREGS days allocated to extended

Table 3
Regression Specifications Shown in Figure 2

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

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

family. These estimates are more sensitive to the choice of bandwidth than our estimates from Figure 1, 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 3 shows the regression estimates of Figure 2—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.

5.2 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 leaders who extract fewer excess days of NREGS labor. We estimate

$$\begin{aligned}[Outcome]_{ip} = \pi_0^M + \pi_1^M [Margin]_{ip} + \pi_2^M [Margin]_{ip} \times [Win]_{ip} + \beta^M [Win]_{ip} \\ + [Het]_p \times \left(\pi_0^H + \pi_1^H [Margin]_{ip} + \pi_2^H [Margin]_{ip} \times [Win]_{ip} + \beta^H [Win]_{ip}\right) + \nu_{ip}\end{aligned} \tag{2}$$

where $[Het]_p$ is a dummy for whether the panchayat presidency is reserved for a woman or a disadvantaged caste (scheduled caste, scheduled tribe, or other backwards class). The coefficient β^H is a difference-in-discontinuities estimate for how many more days of NREGS labor are self-dealt by presidents elected in reserved panchayats. Table 4 shows that although this interaction term is negative for both forms of reservation, it is small and statistiaclly 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).

5.3 Is there Any Heterogeneity in Outside Payments?

The prior results suggest the president uses his power mainly to extract rents, and that the size of these rents does not vary much between reserved and unreserved panchayats. It is natural to ask whether there is any variation at all in the size of these rents. We measure heterogeneity

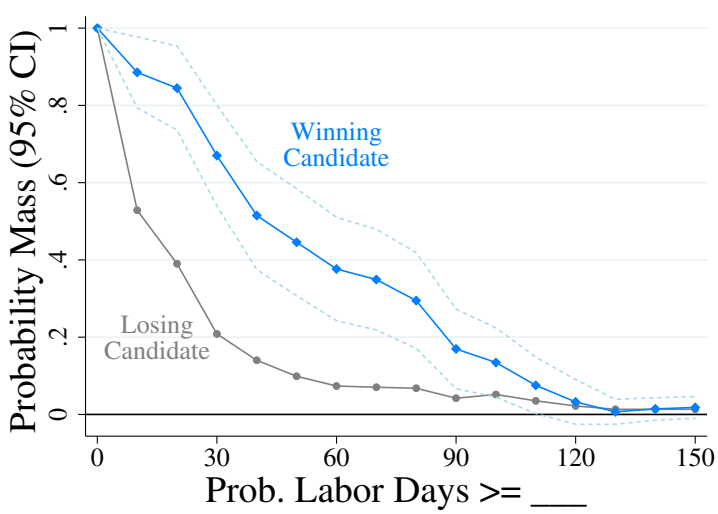
Table 4
Reservations and Outside Payments

	(1) Female Reservation	(2) Caste Reservation
RD Estimate	39.298***	37.412***
	(6.320)	(4.910)
RD Estimate (Interaction)	-4.342	-1.580
	(8.883)	(11.230)
Outcome at Disc.	19.726	20.010
Observations	1105	1105
Panchayats	757	757

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

by estimating Equation 1 on a sequence of dummy variables for whether the number of NREGS days exceeds 0 days, 10 days, 20 days, etc. Figure 3 plots the constant and the sum of the constant and the estimated discontinuity. These are essentially estimates of the inverse cumulative distribution function for a candidate who barely loses or barely wins.

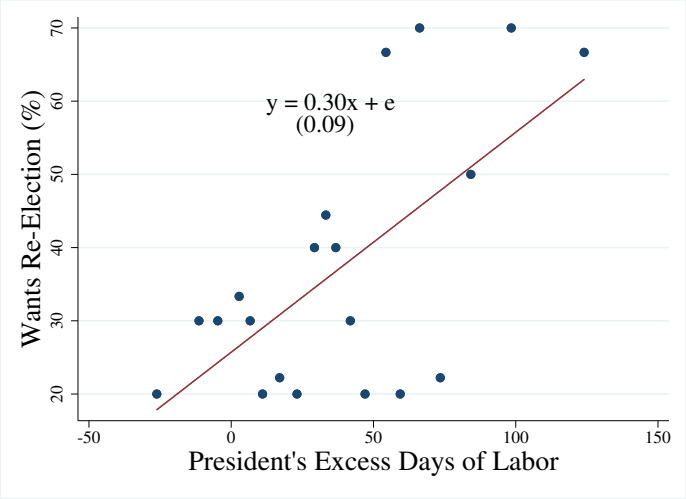
Figure 3
There is Heterogeneity in Self-Dealing



Note: We estimate Equation 1 on a sequence of indicator variables for whether the number of NREGS days exceeds 0,10,20,... We plot the constant (Losing Candidate) and the sum of the constant and the estimated discontinuity (Winning Candidate). Standard errors are clustered by panchayat.

Figure 3 shows that self-dealing varies drastically. While roughly 20% of presidents receive over 90 days of labor, a comparable fraction receives less than 20 days (the inverse-CDF is roughly 0.8 at 20). This variation could arise for two reasons. One is that political selection is not solely dependent on the returns from self-dealing, and despite the perverse incentives some relatively honest people run for office. But it may also be that some panchayats have stronger checks on self-dealing. We find in unreported results that self-dealing declines from 2015 to 2018 in panchayats close to the state capital or close to a cyber café, implying presidents may be less able to self-deal in places where state officials or voters can more easily monitor their behavior.[20]

Figure 4
Presidents Who Get the Most Excess Labor
Are Most Likely to Desire Re-Election



Note: Based on a regression using the responses of 194 council presidents who responded to our question about their plans. Presidents who said they were unsure whether they would seek re-election are coded as zeros. Excess NREGS labor is defined as NREGS days allocated to the president's household minus the average among other households in the village. Standard errors are robust to heteroskedasticity. Each dot shows the fraction responding yes within a bin of roughly 10 observations.

5.4 Is there Any Evidence of Political Selection?

Is there any evidence that variation in returns induces political selection? Figure 4 plots a correlation that is at least consistent with that notion. Among presidents that took our survey we

[20] This is consistent with recent research that institutions and changes in policy affect politicial selection, for example by requiring politicians to disclose financial information. See for example Becher and González (2019) and Fisman et al. (2019).

plot the fraction who say they intend to seek re-election against their excess NREGS labor (days earned net of the average among other households in the village). There is a statistically significant positive correlation that, though not necessarily causal, could be consistent with political selection. Relatively honest presidents could find the opportunity cost of office too high to seek re-election, or greedy presidents who are prevented from self-dealing by outside checks could leave office to seek better prospects. Since the variation in excess labor is not exogenous, however, the correlation can be taken as at best consistent with these explanations, not necessarily caused by them.

6 Conclusion

By directly measuring self-dealing in a public works program, we show that village council presidents in India earn excess returns equal to two thirds of their official salary. While high, this estimate is likely to be well-below what a local politician in a typical developing country can earn by self-dealing public benefits. Council presidents in Uttarakhand are constrained by institutional checks that are rare in developing countries, and their decisions about NREGS allocations face a level of trasparency that is almost unique. Local authorities elsewhere in India and in other developing countries are unlikely to face similar constraints.

We also find suggestive evidence that the large returns from self-dealing may induce negative political selection, since the politicians who self-deal the most are most likely to report that they plan to seek re-election. While not causal, this correlation has a worrying implication when viewed through a model of political selection (e.g. Besley, 2005). Our main results imply that a sizeble part of the returns to office comes from self-dealing benefits intended for the most vulnerable members of society. Such a system, according to a model of political selection, makes public service attractive to those most willing to subvert it for personal gain. Given the paltry size of the official salary — on average half of the median household income in rural Uttarakhand — public service may be too costly for those who refuse the unethical returns. If the model is correct the implications would be dire. As developing countries decentralize power over ever larger shares of their spending, the people entrusted with that power may be those most likely to abuse it.

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