De Jure versus *De Facto* Transparency: Corruption in Local Public Office in India

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Abstract

Governments and NGOs have invested heavily in fighting corruption by designing anti-poverty programs that maximize transparency and accountability. We analyze whether corruption is still widespread in the context of one such program, a massive make-work scheme in India where every job spell is posted publicly online. Linking millions of administrative job records to local election outcomes, we measure how many jobs local politicians self-deal. In the year after the election, winners of close elections receive 3 times as many workdays as losers and typical villagers. We find that corruption persists because of a gap between *de jure* and *de facto* transparency. Only when citizens have tools to access information in a timely manner does corruption eventually vanish.

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1 Introduction

Policymakers, non-profit organizations, and academics typically suggest that policies that foster transparency and accountability are the best solution for fighting 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). Citizens must have both information on where entrusted power is being misused, and ways to hold the corrupt accountable.

However, this seemingly straightforward solution implicitly assumes that citizens have the necessary tools to access the available information and have the desire to punish corruption, neither of which may be true.[1] For example, even if a government discloses public spending on its website, this information will be of little value if citizens do not have access to the internet and do not know when the information will be posted. This example highlights the difference between *de jure* transparency and *de facto* transparency, which arises only if the information is being accessed and understood by citizens. Moreover, having the information and the power to punish the corrupt does not always mean citizens *want to*. Citizens may be supportive of their leader's corruption if they believe that corruption is a good incentive for the leader's performance in serving their needs.

In this paper, we investigate these assumptions on transparency and accountability, and their role in reducing corruption. We study how local politicians in the Indian state of Uttarakhand implement the world's largest anti-poverty program, where considerable effort by governments, NGOs, and social activists has produced an administrative system designed to curb corruption. 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. One measure of corruption in this context is the number 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).

[1] See e.g. Kosack and Fung (2014) for a conceptual framework of the pathways to transparency and an overview of the literature. They find only mixed evidence that transparency interventions improve governance.

The program's unusual level of transparency allows us to create a unique dataset that directly captures presidents' self-dealing of NREGS benefits, as well as their performance in generating NREGS employment for the village. Each employment spell is published on a publicly available government website and identifies the recipient by name and location. We scrape millions of these reports, covering over 90 percent of rural households in Uttarakhand.[2] We match these NREGS reports to the election returns for candidates competing in thousands of village council elections. The merged 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 they remained out of office. We also observe the NREGS employment the winner subsequently creates for the entire village, a direct measure of program performance.

The very website used to construct our data also creates a level of *de jure* transparency that might be considered sufficient to reveal corruption. 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. All NREGS projects are hyperlinked. One click on the link will turn up more information about the project, including a list of all of the other workers assigned to the same project, which in turn links to their job card profiles. This means that even if workers start out as just being interested in accessing their own information, they can quickly compare their records to those of others with a few clicks. 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 generally aware that money for welfare benefits goes missing, voters in Uttarakhand can directly observe how much money flows into the president's own bank account.

The officials who administer these programs also face a level of local accountability that is

[2] Households can be registered for the program without ever having worked on it.

Figure 1
Comparing Close Winners and Losers on the Official NREGS Site

Won by 3 votes

Job card
MAHATMA GANDHI NATIONAL RURAL EMPLOYMENT GUARANTEE ACT

Job card No.: UT-[redacted] Family-Id: 82
Name of Head of Household: [redacted]
Name of Father/Husband: [redacted]
Category: OTH
Date of Registration: 1/8/2008
Address:
Villages: [redacted]
Panchayat: [redacted]
Block: BHAISIACHHANA
District: ALMORA(उत्तराखंड)
NO Family-Id: 82
Epic No.:

Details of the Applicants of the household willing to work

S.No	Name of Applicant	Gender	Age	Bank/Postoffice
1	[redacted]	Male	33	District Co-operative Bank
2	[redacted]	Female	30	UTTRANCHAL GRAMIN BANK

Signature/Thumb impression of Applicant
Seal & Signature of Registering Authority

Period and Work on which Employment Given

S.No	Name of Applicant	Month & Date from which employment requested	No of Days	Work Name	MSR No.	Total Amount of Work Done	Payment Due
1	[redacted]	21/02/2015	14	[redacted]bhetadangi se khan se mandir tak marg nirman 1.0	645	2184	0
	Sub Total FY 1415		14			2184	0
2	[redacted]	25/08/2015	14	[redacted]harju mandir se devithan tak marg nirman karya 1.0	178	2254	0
3	[redacted]	03/12/2015	14	Anganwadi/Other Rural Infrastructure,BHAISIACHHANA	404	2254	0
4	[redacted]	30/12/2015	14	Anganwadi/Other Rural Infrastructure,BHAISIACHHANA	496	2254	0
	Sub Total FY 1516		42			6762	0
5	[redacted]	03/08/2016	14	Anganwadi/Other Rural Infrastructure,BHAISIACHHANA	439	2436	0
6	[redacted]	03/08/2016	14	Anganwadi/Other Rural Infrastructure,BHAISIACHHANA	439	2436	0
7	[redacted]	23/08/2016	14	Anganwadi/Other Rural Infrastructure,BHAISIACHHANA	522	2436	0
8	[redacted]	23/08/2016	14	Anganwadi/Other Rural Infrastructure,BHAISIACHHANA	522	2436	0
	Sub Total		[redacted]			[redacted]	[redacted]

Lost by 3 votes

Job card
MAHATMA GANDHI NATIONAL RURAL EMPLOYMENT GUARANTEE ACT

Job card No.: UT-[redacted] Family-Id: 14
Name of Head of Household: [redacted]
Name of Father/Husband: [redacted]
Category: OTH
Date of Registration: 1/8/2008
Address:
Villages: [redacted]
Panchayat: [redacted]
Block: BHAISIACHHANA
District: ALMORA(उत्तराखंड)
NO Family-Id: 14
Epic No.:

Details of the Applicants of the household willing to work

S.No	Name of Applicant	Gender	Age	Bank/Postoffice
1	[redacted]	Male	35	
2	[redacted]	Female	30	

Signature/Thumb impression of Applicant
Seal & Signature of Registering Authority

Period and Work on which Employment Given

Note: From the publicly accessible official NREGS website at nrega.nic.in. Identifying information has been redacted for this image. The record of Employment Given is blank for the job card on the right because none was given.

unusual in a developing country. 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 has rendered the president unaccountable to voters. 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, unlike in larger polities, can make their complaints known in person. If villagers find presidents too odious to wait for the end of the term, they can recall them by petition. They can also exercise informal checks—confrontations, social ex-

clusion, and even (according to our own survey evidence) threats of violence. Given that the election rules in our context force most presidents to leave office after a single term, they know that their political authority cannot deter the displeasure of their fellow villagers for long.

Nevertheless we find a sizeable amount of corruption among local politicians. The winners of close elections receive nearly 3 times as many days of labor as losers in the year after the election. Individuals who visit the website would therefore see that the list of job spells received by the president is three times longer than that of a typical villager in the year after the election, an easily detectable difference even when just casually comparing job card records. This sizable list of jobs and wages may or may not represent actual work done by the president. The president or a member of the president's household might receive payments even without having to perform any manual labor if a worksite manager or technical assistant is complicit.[3]

What makes this result surprising is not the cost to the Indian government, which is small compared to the total NREGS budget, but its visibility.[4] Even relatively small levels of corruption in other contexts have triggered blaring headlines and resignations.[5] It would also be salient to these citizens that the labor allocated to the council president could instead have gone to them, given that there is a fixed quantity of NREGS jobs on any given project. The average size of the self-dealing amounts to as much as 50 percent of the yearly earnings of the poorest households seeking NREGS labor.[6] Why would they tolerate such visible self-dealing from an official who lives among them and is thus well within reach of their complaints?

To explore why corruption persists despite the transparency and accountability measures in place in our context, we scrutinize three assumptions of transparency and accountability, each

[3] See e.g. The Times of India, 'Sarpanch Gets NREGA Wages for Son', April 22, 2010, or The Times of India, 'Gujarat: Kin of Rich Sarpanch Too Work for MGNREGA!', September 9, 2020.

[4] Our back-of-the-envelope calculation implies this corruption amounts to roughly 1.4 percent of Uttarakhand's NREGS unskilled labor funds in 2015.

[5] For example, Germany's overseas aid minister had to resign under public pressure for not paying about 200 Euros in import duties for an Afghan rug (see https://www.reuters.com/article/oukoe-uk-germany-carpet/german-minister-chided-for-flying-carpet-from-kabul-idUKBRE8570QK20120608). Similarly, the alleged improper spending of Scott Pruitt, an official in the Trump Administration, generated months of headlines that ultimately drove his resignation. The total sum of potentially misreported or frivolous expenses amounted to less than 300 thousand dollars, an infinitesimal fraction of the 8 billion dollar budget of the agency he lead (see https://www.cnn.com/2018/04/06/politics/scott-pruitt-controversies-list/index.html).

[6] A survey in Uttarakhand's Chamoli district of 118 sampled NREGS beneficiaries found that more than half reported annual expenditures between 12,000 and 60,000 rupees in early 2017 (Chakraborty and Mukerji, 2017). These results are comparable to our own analysis based on NSS data from 2011-2012 for all of Uttarakhand.

of which could plausibly fail in this context. The first assumption, that citizens have access to infrastructure for information, may fail because internet access was far from standard in many rural areas in Uttarakhand during our study period. Even today most of rural Uttarakhand lacks reliable cellular data networks, forcing poorer households to rely on wired connections such as cyber cafes that may or may not be present in every village.

The second assumption, that information is available in a timely manner, was equally questionable at the beginning of our study period. Uttarakhand was lagging behind most other Indian states in using the full capacity of NREGS' online management system to generate information in close to real time. An audit by the central government found that information on employment spells was available online with an average delay of 30 days and that the typical wage payment delay was 74 days, but with considerable uncertainty about actual posting and payment dates (Comptroller and Auditor General of India, 2013).[7] Villagers who paid the cost of internet access (at a cyber cafe, for example) might discover they had wasted their money because the official record for a job spell had not been posted days or even weeks after the spell had concluded.

The third assumption, that citizens always want to punish corrupt officials, also may not hold in our context. In an original survey of roughly 200 village council presidents in Uttarakhand, we ask a number of questions about their work and how they run NREGS. 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 (despite being unaware that we are studying self-dealing or monitoring their labor allocation). The majority of village council presidents therefore rationalize self-dealing by claiming villagers accept corruption as compensation.

We test which of these assumptions might not have held in the aftermath of the election by exploiting three features of the context. First, there is substantial variation in the distance to the nearest cyber cafe, which we use as a proxy for access to infrastructure. Second, in mid-2016 the central government implemented an improved electronic wage payment system. The reform did not change the role or process for workers or local governments, but substantially improved the timeliness in posting job spell and payment-related information to the NREGS website. Third,

[7] Payment delays in Uttarakhand ranged from 1 to 669 days.

we construct a direct measure of the president's performance in managing NREGS, the average days of labor provided to households in the village. This measure lets us observe whether any decline in corruption caused by the cyber cafes and the 2016 regulation are followed by a decline in performance as predicted by the corruption-as-compensation hypothesis.

We estimate the difference in discontinuities in self-dealing and performance measures between areas close to and far from a cyber cafe over time. We find that the levels of self-dealing were similar for both types of villages before the 2016 regulation. After 2016, corruption declines substantially in villages close to cyber cafes, but there was no similar decline in performance. Our measure of job performance, the number of NREGS days generated for households, is similar across both types of villages both before and after the reform.

Our results suggest that the online system was initially unable to curb corruption because many villagers could not access it, and even those who could were unlikely to find timely information. Had access to technology been the only binding constraint, corruption would have been lower in areas near a cyber cafe from the very beginning. But since corruption only falls after the reform, it suggests that untimely information is an equally binding constraint at baseline, especially in our setting where the cost of obtaining information is high. By contrast, we find no evidence that villagers tolerate corruption as the price of competent leadership. Performance remains high even as corruption falls, suggesting there is no trade-off.

Our biggest contribution is to make the practical requirements for transparency explicit, and to demonstrate their importance for reducing corruption empirically. An extensive literature has documented that transparency and accountability measures can curb 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).[8] But the literature studying how transparency interventions improve government accountability has often found mixed results (Banerjee et al., 2008; Kosack and Fung, 2014; Lieberman et al., 2014).[9] Kosack and Fung (2014) suggest that the mixed results are due to specific contextual fac-

[8] For a literature overview, see Aidt (2003); Bardhan (1997); Olken and Pande (2012); Svensson (2005).

[9] An important exception is Banerjee et al. (2018), who find that mailing information cards to program beneficiaries in Indonesia reduces leakages while substantially increasing the benefits citizens derive from a large rice subsidy program. For a recent overview of the literature on government responsiveness, please also see Grossman and Slough (2022).

tors: Successful transparency interventions are especially difficult when politicians and service providers have no incentive to improve service delivery or collaborate with citizens. Our results provide some of the first empirical evidence for this hypothesis. The results are especially striking given that the "service provider" is a directly elected local politician, a position that ex ante might be assumed to have very strong incentives to address the concerns of citizens.

Although the transparency measures put in place by the Indian government far exceed those implemented in other developing countries, our findings therefore demonstrate that they alone are not sufficient. To curb corruption, governments and non-profit organizations should therefore not just consider ways of making information available, but should also work on coordinating when it is available and ensuring citizens have the means to access it. This may entail investing in technology and infrastructure as well as a careful tweaking of program design features to close the gap between *de jure* and *de facto* transparency.

Another strength of our context is that we observe that payments from NREGS land directly in the bank account of the politician, making it unusually clear where exactly the money goes.[10] Due to data limitations, the existing literature tends to focus on "leakages," where program funds go missing but it is unclear where exactly they disappear to. Measuring leakages can reveal the size of corruption, but not the source—for example whether bureaucrats or politicians take the biggest cut. In our context, 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 be unusually salient that the NREGS jobs that the president received could have gone to other villagers. These features allow us to contribute to the literature on unofficial returns to office in developing countries.[11]

[10] 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.

[11] 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; Baturo, 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 unethical 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).

Our results also contribute to the 'greasing the wheels' literature on corruption. 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).[12] 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.[13] Our paper is also novel in extending the hypothesis to interactions between citizens and local politicians, and its focus on service delivery rather than allocative efficiency.

Lastly, our results contribute to a better understanding of the remaining challenges to implementing NREGS. As one of the largest and most ambitious welfare programs worldwide, there is considerable policy interest in identifying the impact of its design features. While Muralidharan et al. (2016) have shown that the reforms that linked NREGS benefits to a biometric identity card have improved targeting significantly, we show that self-dealing drops substantially after a government reform leads to big improvements in the timeliness of available information online.[14]

2 Background

[12] 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.

[13] This makes our results more consistent with the macro literature, which overwhelmingly finds no support for the hypothesis.

[14] Consistent with an improvement in the implementation of NREGS, our estimates of self-dealing in a context with high levels of transparency and accountability are substantially lower than Niehaus and Sukhtankar (2013a)'s estimates from the early days of NREGS implementation when off-the-books corruption was much easier to pull off.

2.1 Elections and Accountability in Rural Uttarakhand

Village council presidents in Uttarakhand are directly elected every five years.[15] 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 arises in other parts of India from caste and religious conflict. The state only has 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.[16] The support of an ethnic community is thus less likely to insulate the president in Uttarakhand than elsewhere.[17] We focus on the 2014 election, the first after the NREGS online management system had substantially increased its capabilities, which we lay out in section 2.3 below.

Local elections in 2014 served a selective rather than disciplining role because the system of caste and gender reservations made it difficult or impossible for a president elected in 2014 to stand for re-election in 2019.[18] Presidents face several formal and informal checks on their power. They are 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 the politician. A district bureaucrat then convenes a village meeting where the president can be removed from office if a majority of villagers favors this.

[15] A directly elected president is the norm across most of northern and central India. Rajasthan, Uttar Pradesh, and Bihar are just a few other examples of states that have a non-partisan directly elected president. There are a few states, notably West Bengal and Kerala, that instead hold explicitly partisan indirect elections.

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

[17] 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. 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.

[18] We calculate that roughly 80% of our sample was legally barred from standing in the next election due to the rotation of reserved seats. Even after adjusting for the possibility that a spouse might stand in place of a candidate barred from running by gender reservation, we find that only 10 percent of presidents seek reelection.

The informal sanctions may be even more important. In a survey of council presidents described below, we asked elected presidents whether a typical president would be formally or informally sanctioned for making NREGS allocations that are unacceptable. The overwhelming majority said most presidents would be sanctioned. 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. Given that the current president is almost guaranteed to lose power in five years and is easily accessible in these small villages, they would have no protection against their disenchanted neighbors.[19]

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 labor at the minimum wage (Zimmermann, 2020). There are no further means tests (Dey et al., 2006; Government of India, 2018). Most projects are routine tasks that do not create substantial public investment, such as clearing bushes or digging holes.

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.[20] But in practice there is often a shortage of NREGS jobs. 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.[21]

[19] The 2014 Socioeconomic and Caste Census implies that the median panchayat in our sample contains just 100 households, and 90 percent of panchayats contain fewer than 250.

[20] See Berg et al. (2018), Imbert and Papp (2015) and Zimmermann (2020) for analyses of the economic impacts of the program.

[21] 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 implemenation 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.

The necessary rationing of employment due to excess demand gives village council presidents a key role in the allocation of jobs among households in the area they govern (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 president also effectively controls the allocation of jobs. A worker who wants NREGS labor must apply at the village council office. Though in theory a joint decision by the entire council, our survey of council presidents finds that they 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.

2.3 NREGS Performance and Transparency Measures as of the 2014 Election

Compared to other government schemes, both in India and elsewhere, NREGS includes unusually many design features intended to minimize corruption. They were added in response to pressure from NGOs and social activists, who were strongly interested in creating a welfare safety net for the rural poor, but worried about corruption.[22] The program contains safeguards such as social audits, which are supposed to be carried out regularly by villagers; a mandated 60:40 percent ratio of wage payments to materials; and a ban on employing contractors for NREGS projects. This is meant to maximize the jobs created for workers in simple manual tasks.

But the main transparency feature is the publicly available website, which we also use as our data.[23] From the beginning, the NREGS website was paired with the management information software 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). After a 2011 update, the software application allowed local government officials

[22] A first draft of an employment guarantee act, similar to the final provisions of NREGS and written by researchers and social activists Nikhil Dey and Jean Drèze, was circulated in the fall of 2004. As they note in their booklet 'Employment Guarantee Act – A Primer' from October 2004: 'Workers' organisations have been demanding a national Employment Guarantee Act (EGA) for many years. This 'primer' was prepared to facilitate public discussion of this issue at all levels – from remote villages to the national capital.' See http://www.sacw.net/Labour/EGAprimer.html

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

to update workers' bank account information in the system, meant to further speed up and digitize the process, but also included automatic flags for suspicious actions. Violations of the wage to material expenditure ratio are automatically monitored in the system, and there were expenditure caps on one-time payments for administrative costs and bills from vendors. Muster rolls from NREGS projects could also not be entered with a delay of more than two months.

Despite these provisions, a central government audit for the financial year 2012-13 documented that the state of Uttarakhand was one of the worst performers on using the entirety of NREGASoft's capabilities (Comptroller and Auditor General of India, 2013): Only 1607 of 7577 village councils implemented so-called e-MRs, entirely electronically generated muster rolls of workers at NREGS work sites. e-MRs make NREGS project and muster roll information publicly available in close to real time. Instead, muster rolls were typically filled out on paper at the work sites, and later manually entered into NREGASoft, generating a delay of information available on the public website of 30 days on average. Additionally, only one of Uttarakhand's 13 districts had implemented the e-FMS (electronic fund management system), which meant that most workers were not paid electronically through direct bank transfers from the central government to beneficiaries' bank accounts.

The state was told to quickly ramp up the real-time use of the electronic system and, along with all other Indian states, was required to make all payments for materials through e-FMS starting in January 2014.[24] This meant that central government funds would be directly transferred to vendors' bank accounts, making corruption in the materials budget much more challenging. Direct payments of NREGS wages into beneficiary bank accounts was still far from common, however: All over India, only 37 percent of wages were being paid directly electronically in the financial year 2013-14.[25]

Overall, by the time of the 2014 panchayat election, village council presidents in Uttarkhand were therefore subject to substantially more monitoring by the higher levels of the administration through NREGASoft than in the early days of the program, making off-the-books corruption more difficult.[26] But while NREGS job allocations and wage payments were eventually reflected

[24] See https://nrega.nic.in/netnrega/releases.htm

[25] Press Information Bureau, Ministry of Rural Development, Government of India. 'Direct Benefit Transfer under MGNREGS', available at https://pib.gov.in/newsite/PrintRelease.aspx?relid=181108

[26] See Afridi and Iversen (2014) and Niehaus and Sukhtankar (2013a,b) for earlier analyses.

on the official website, digitization delays led to uncertainty about the exact time at which the information was available online. In a survey of NREGS workers in Uttarakhand, 72 percent of respondents cited wage payment delays as a major problem, along with the concern that the available information on NREGS was incomplete. When asked about potential improvements, over 70 percent of workers favored increased transparency and accountability mechanisms (Singh et al., 2018).

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 all other adult 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.

We merge the NREGS data to publicly available information on local elections 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 profiles. 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, then matching manually by name.[27] We draw several variables from the Census, most importantly a categorical variable

[27] It is necessary to match by name because there is no local government code in either the NREGS or election

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. Other variables include the distance to the nearest post office, major road, bank and market.[28] 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.[29] 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).

data. We explain this process in detail in Appendix B.2. We lose panchayats during the matching because many names cannot be matched. These observations are excluded from the difference-in-discontinuity results.

[28] 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.

[29] 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.

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

	Winners and Runners-Up		Winners Only	
	Full Matched Sample	In Bandwidth	All	Surveyed
Winner	0.54	0.53	1.00	1.00
	(0.50)	(0.50)	(0.00)	(0.00)
Female	0.47	0.48	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	0.56	13.96	14.65
	(19.77)	(9.11)	(11.63)	(14.37)
In Bandwidth	0.67	1.00	0.66	0.65
	(0.47)	(0.00)	(0.47)	(0.48)
Surveyed	0.13	0.12	0.23	1.00
	(0.33)	(0.33)	(0.42)	(0.00)
Days of labor (2015)	40.10	40.30	57.19	57.40
	(40.66)	(40.08)	(42.15)	(39.00)
Days of labor (2013)	23.13	24.38	22.08	26.59
	(34.55)	(35.65)	(34.27)	(37.71)
Observations	1650	1105	887	207
Panchayats	1148	757	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.[30] 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:

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

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

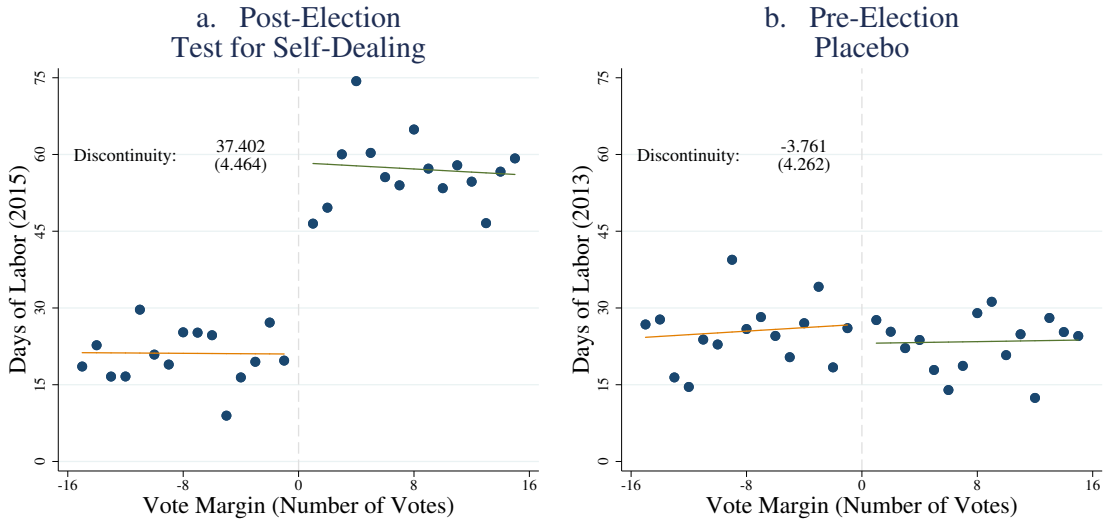
4.1 Winners of Close Elections Engage in Self-Dealing

We estimate Equation 1 on candidates whose vote margin is within a bandwidth of 15 votes.[31] As the election was in mid-2014 we initially 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 2 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. Winners receive an extra 37 days of labor—nearly 3 times as many as the losers—suggesting they heavily favor their own households over others. This does not necessarily mean

[30] 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.

[31] 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 2
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 presidents are literally working these jobs, only that they are being paid for the work. It is entirely possible that no member of the president's household ever sets foot on the work site. We have no means of directly assessing whether the president is actually providing any labor for the pay received.

Panel A of Table 3 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 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.[32] 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

[32] 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).

of council presidents and newspaper reports from Uttarakhand,[33] 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. This self-dealing amounts to as much as 50 percent of the yearly earnings of the poorest households seeking NREGS labor.[34]

These results are broadly consistent with those of Besley et al. (2012), who find that elected council presidents are more likely to receive antipoverty relief despite being wealthier than the average household. Our results bring a causal interpretation to the pattern of direct self-dealing by local politicians in India. But unlike Besley et al. (2012), 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.[35] We discuss these results next.

4.2 Does Reservation Affect Self-Dealing?

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. Table 2 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

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

[34] A survey in Uttarakhand's Chamoli district of 118 sampled NREGS beneficiaries found that more than half reported annual expenditures between 12,000 and 60,000 rupees in early 2017 (Chakraborty and Mukerji, 2017). These results are comparable to our own analysis based on NSS data from 2011-2012 for all of Uttarakhand.

[35] We also do not find evidence that presidents allocate excess NREGS benefits to extended family or other village council members (Appendix A.2.1).

women (Column 1), and 36 days under Caste Reservation versus 37.5 days in other panchayats (Column 2).

Table 2
Reservations and Self-Dealing

	(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). The "Interaction" estimate gives the difference in RD estimates between reserved panchayats and other panchayats. Standard errors are clustered by panchayat.
*p=0.10 **p=0.05 ***p=0.01

Afridi et al. (2017) find that female politicians in Andhra Pradesh initially have worse NREGS implementation quality and higher program leakages, but catch up to male politicians during their term. We show in Appendix A.2.2 that that pattern does not hold for self-dealing in our sample. Trends in corruption are statistically indistinguishable in reserved seats and unreserved seats.

4.3 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 that 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 3.A. But Column 4 shows that the estimate is close to zero, suggesting losers are treated no differently than the typical household.

Table 3
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)	(7)
	Days of Labor (2013)	SCT	Female	Age	Cand. Name Len	Rel. Name Len	Matched
RD Estimate	-3.761	0.012	0.010	0.498	-0.287	0.216	0.028
	(4.262)	(0.042)	(0.044)	(1.190)	(0.312)	(0.409)	(0.032)
Outcome at Disc.	26.85	0.18	0.47	35.52	10.46	8.61	0.32
Observations	1105	1105	1105	1040	1105	1105	2400
Panchayats	757	757	757	724	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

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 2 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 3.B report applying the same procedure to several other pre-determined outcomes. Column 1 is the same as Panel B of Figure 2. 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. Column 4 tests for whether winners are systematically older or younger than losers. 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 5 and 6 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 7 suggests there is no discontinuity.[36]

Finally, we verify that the results are not sensitive to the choice of bandwidth. Table 3.C

[36] 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).

estimates Equation 1 for bandwidths ranging from as wide as 25 votes to as narrow as 7.5.[37] The estimates are all similar.

4.4 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.[38] 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 (Appendix Figure A4).[39] There is no evidence to suggest the president's own NREGS allocation varies by season.

Finally, one might worry that the result is driven not by self-dealing but, in panchayats with multiple villages, by favoritism for the president's own village. Then it might appear that the president is working more jobs than the runner-up simply because all households in the presi-

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

[38] Uttarakhand experienced a 71 percent deficit of post-winter rainfall in 2015. See e.g. Hindustan Times, '9 Uttarakhand Districts Declared Drought Affected', April 15, 2016.

[39] 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.

dent's village work more jobs than households in other villages. We rule out this explanation by running an even more restrictive version of the Dual-Match specification in Column 2 of Table 3 where we use only observations where we match both winner and runner-up, and where both live in the same village within the panchayat. The size and significance of the result is identical.[40] More generally over 60 percent of panchayats comprise only a single village, leaving no room for geographic favoritism.

5 Potential Mechanisms

Self-dealing persists despite an online system that provides accurate information about how government funds are spent by accessible and accountable local officials. While in principle the system seems ideal, in practice it falls short. The *de jure* transparency of entering and posting information through NREGASoft, and the *de jure* accountability created by empowering local officials who can be confronted or formally recalled, cannot curb corruption if they are not actively used by local stakeholders. We consider three additional assumptions that may be necessary to turn *de jure* transparency and accountability into *de facto* transparency and accountability:

1. **(Convenient) access to technology**: Local stakeholders such as voters, NGOs, or the media have the ability to access the required infrastructure or technology to view the available information.

2. **Timeliness of information**: Information is posted not too long after the work is completed, the deadline for posting it is common knowledge, and the information posted by that deadline is final.

3. **Desire for accountability**: Stakeholders have the desire to act on the information to punish politicians for their self-dealing.

Only if all three assumptions hold will corruption be effectively reduced. In the following three sub-sections, we focus on each of the three assumptions in turn. We briefly lay out whether the assumptions could fail in our context and how we can test the importance of the different channels empirically.

[40] Results available on request.

5.1 (Convenient) Access to Technology? Variation in Internet Access

Local stakeholders cannot use a website to monitor their leader if they lack access to the internet or can only access it at a very high time or monetary cost. According to multiple large household surveys, internet access was low in Uttarakhand at baseline and during the time covered in our main analysis. The 2011/12 Indian Human Development Survey (IHDS) reveals that only 7% of Uttarakhandis used email or the internet on a computer. Almost no one had internet access on their mobile phone, either. Only 8% of Uttarakhandis report using internet or email on a mobile phone. The low internet penetration rates are similar when using more recent household data sets. According to the 2014 National Sample Survey (NSS Round 71) Social Consumption Survey, only 6% of households have a computer at home, while 15% of households reported having access to the internet.[41] Overall, this suggests that internet access may have been a substantial barrier for monitoring NREGS allocation in many parts of Uttarakhand.

The 2011/12 IHDS also asked individuals, who know how to use a computer, where they typically have access to one. Nearly 25% of respondents report visiting a cyber cafe, making this the second most popular option after having a computer at home.[42] While we have no village-level data on internet access, the 2011 Census provides information on the distance to the closest internet cafe. We use this variable as a proxy for access to internet under the assumption that being close to a cyber cafe allows at least some individuals to monitor NREGS allocations, who could then spread awareness of any suspicious allocations. This could include villagers themselves or other actors like local media, NGOs or the political opposition.

A potential concern with using 2011 Census information as a proxy for monitoring capacity is that internet access and the availability of smart phones could have expanded substantially by the end of our analysis period in 2018. Figure 3 plots the quality of 3G/4G phone data coverage in the state of Uttarakhand as of 2020, based on information from the company Airtel (the main cellular data provider in Uttarakhand).[43] The dashed line indicates the state border to the

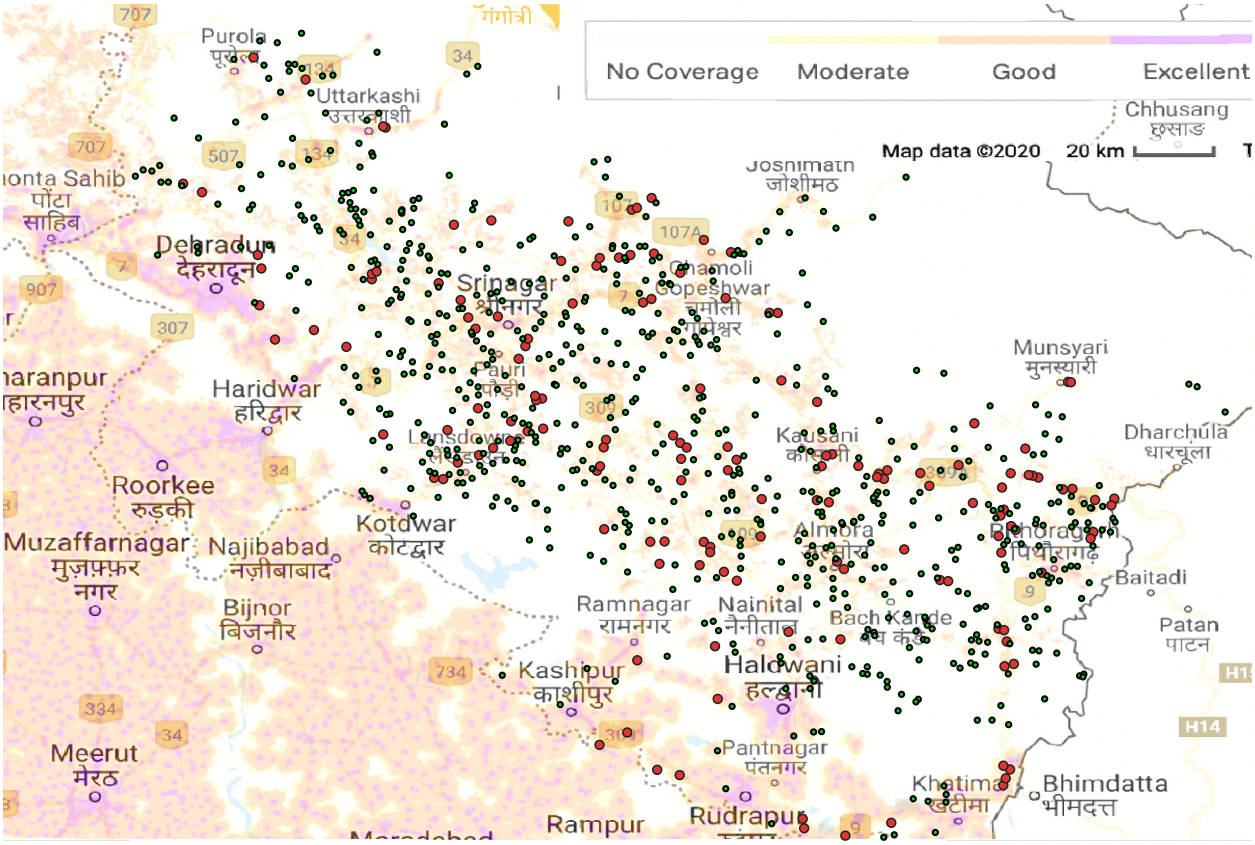
[41] This is consistent with the NSS 2015 Consumer Expenditure Survey where we find that only 7% of households in Uttarakhand pay for "internet, fax, e-mail" (not including mobile data), with a median expenditure of Rs100/month.

[42] Households who have a computer at home tend to have higher than average household income, making them less likely to seek NREGS jobs than our target population.

[43] Available from here https://www.airtel.in/opennetwork/.

Figure 3

Mobile Network Coverage in Uttarakhand in 2021



Note: Red dots indicate villages that are less than 5 kilometers from the nearest internet cafe, whereas green dots are villages more than 5 kilometers from a cyber cafe based on the 2011 Census.

neighboring state of Uttar Pradesh. As can be seen from the figure, network coverage is only excellent in Uttarakhand around the state capital of Dehradun and a few other cities close to the southwest border, a stark contrast to the much better network density in the neighboring state (Uttar Pradesh). Most rural areas have spotty network coverage, although there is substantial local variation. The map therefore shows that access to data and the internet was still far from universal in 2020, but with highly localized heterogeneity. We overlay this plot with information on the location of panchayats in our analysis sample. Red dots indicate villages that are less than 5 kilometers from the nearest internet cafe, whereas green dots are villages more than 5 kilometers from a cyber cafe. As the map shows, cellular access is spotty for most of the panchayats in our sample, making cyber cafes a potentially important source of access even as late as 2020.

Overall, the assumption of convenient access to technology may therefore fail in our context, but we can use substantial local variation in the plausible cost of accessing the internet, proxied by distance to the nearest cyber cafe, to test its importance for explaining the persistent corruption of local politicians.

5.2 Timeliness of Information and the 2016 Ne-FMS Reform

Having information posted soon after the work is completed ensures workers have fresh memories of who was present at the worksite. It also makes the website more useful for answering the question that is top-of-mind for any NREGS worker: whether their payment has been deposited. Getting the answer to this question is the most likely reason a worker would log onto the website, as the alternative—making repeated trips in-person to the bank—would be extremely costly.[44] A single worker who accesses the website could check the labor allocation for everyone on her project site. She could not fail to notice if the council president listed himself despite not being present.

But accessing the internet—even when a cyber cafe is nearby—is costly. Workers may not want to pay the cost if they are unsure whether NREGS payment updates are likely to be available

[44] In a survey of NREGS workers in Uttarakhand conducted in early 2017, Chakraborty and Mukerji (2017) find that the typical worker has a bank account for the sole purpose of receiving NREGS wages, but does not use banking services for other purposes. They note that information on when NREGS payments are available is important for workers. Almost all workers have to go to the bank in person to withdraw their NREGS payments, which requires travelling to the bank during business hours in a state where the bank branch network is still quite sparse.

on the NREGS website. Uncertainty about when the information will be posted might force them to incur large costs only to find out that information on employment spells and payments had not yet been posted. This lowers the incentive to use the website at all.

Finally, workers need to know that the information posted is final. Otherwise there is a risk that the initial beneficiary list is "updated" long after workers have stopped checking it. Workers need to know that the list of beneficiaries they find is the final list of beneficiaries authorized for payment, and that the council president cannot unilaterally alter it.

There is reason to believe none of these "timeliness" conditions were initially met in Uttarakhand. At baseline there was an average delay of 30 days of inputting records into the online management system, which in turn caused wage payment delays of 1 to 669 days with an average of 74 days (Comptroller and Auditor General of India, 2013). Though Uttarakhand was nudged to ramp up its use of the existing electronic system from 2014 to early 2016, this change had little success in reducing wage payment delays.[45] That may explain why we find high levels of self-dealing in 2015 and 2016.

To cut down on wage payment delays and make the process more transparent, the central government mandated a policy change, the adoption of the so-called National Electronic Fund Management System (Ne-FMS).[46] Importantly, the reform was directly integrated into NREGASoft and did not change the role or process for local governments. The reform focused on streamlining the transfer of funds by cutting down on the number of steps and players. There were now in effect exactly two reasons a worker's payment might be delayed: either the local government had not inputted the data and authorized the request, or the central government had not yet made the payment. A user of the NREGS website could clearly see who was to blame, putting immense pressure on local governments to complete their part within 15 days.[47]

[45] These results are consistent with new evidence by Goel et al. (2022), who use recent difference-in-difference methods for staggered treatment adoption to study the impact of NREGS digitization. Banerjee et al. (2020) study the implementation of a different NREGS digitization reform in Bihar, which was highly successful.

[46] For additional details, please see Appendix Section A.2.5.

[47] On paper, NREGS workers become eligible for additional compensation if wage payments are delayed by more than 15 days after the work is completed. In practice, however, the central government redefines a payment to be "on time" if a Fund Transfer Order has been generated and signed by local governments within 15 days, not if the money has actually been transferred into workers' bank accounts within 15 days. The central government sought to use this semantic shift to demonstrate an on-paper reduction in wage payments while insulating the central government from financial responsibility (Narayanan et al., 2019), but this would only work if the local governments did their part.

The Ne-FMS reform significantly increased the speed at which local governments entered the data and submitted payment requests. The share of payment requests made within 15 days hovered just under 40 percent in the three years before the reform, and remained roughly flat at 42 percent in the first year of the reform (2016-17). But by the 2017 fiscal year it had doubled to about 85 percent, and stood at 92 percent in 2018.[48] Thus timely information, available in less than half of panchayats in 2016, was abruptly available to the vast majority in 2017.

Aside from reducing the average time until the information was posted, the 15-day deadline almost eliminated the *uncertainty* about when the data would be visible on the website. After the reform workers could be almost certain that, if they checked the website 15 days after the end of a labor spell, they could see the muster roll and verify that the local government had authorized the payment. The reduced uncertainty made it less likely that paying the cost of accessing the site (say, by visiting a cyber cafe) would be a wasted investment, increasing the incentive to check the website. Finally, the requirement that the actual payment request (and not merely the muster roll) be posted within 15 days guaranteed that the information could not subsequently be altered.

5.3 Desire for Accountability? The Plausibility of Corruption as Compensation

The last assumption is that local stakeholders want to hold politicians accountable for their self-dealing. Otherwise the *de jure* accountability mechanisms and all of the available information would nevertheless be ineffective in curbing corruption. If voters believe, for example, that talented politicians are more likely to stand for office because they can compensate themselves through NREGS, then citizens may be willing to accept corruption to attract leaders with the administrative skill needed to create NREGS projects and jobs for the village. There is reason to believe that the desire-for-accountability assumption fails in our context. 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."

[48] See https://www.firstpost.com/india/digital-transaction-helped-nrega-workers-receiving-wages-on-time-claims-government-4049001.html and https://pib.gov.in/newsite/PrintRelease.aspx?relid=181108.

In our context, competent administrators could plausibly forego public office in the absence of such a cut. According to media reports and responses from our own survey, the median official full-time salary for village council presidents in Uttarakhand is 9000 rupees per year. This is less than 25 percent of the median income of rural households in Uttarakhand.[49] Well over 80 percent of council presidents in our survey report landholdings that are lower than or equal to most households in the village. Assuming this is true, it is unlikely anyone could support a household solely through the official salary. It is thus not surprising that nearly every president in our sample continues working in the private sector (typically farming for men and domestic work for women). Every hour spent on official duties is an hour taken away from private work, so the opportunity cost of carrying out their duties as village council president may be high. Nearly three-quarters of presidents claim that they would have more money if they had not become president. So while the payouts from self-dealing NREGS benefits are large relative to their official salary, they are too small to raise the typical president's total compensation to even half the median.

Knowing this, voters might be sympathetic to council presidents who self-deal NREGS benefits as long as they deliver good performance. But whether such a benign explanation holds is an empirical question.

6 Testing the Mechanisms

6.1 Specifying the Test of Each Set of Assumptions

If any of the three assumptions does not universally hold, it creates a binding constraint on *de facto* transparency. We exploit spatial variation created by distance to a cyber cafe, and temporal variation created by the post-2016 NREGASoft upgrade, to test for these constraints.

Access to technology is the binding constraint: There is corruption only in areas far from cyber cafes. There should be no evidence of corruption in areas near cyber cafes.

Timeliness of information is the binding constraint: Corruption declines everywhere in the years after 2016.

[49] We compute the median from the 2012 Integrated Household Development Survey.

Both access to technology and timeliness of information are binding constraints: Corruption declines after 2016, but only in areas near cyber cafes.

Desire for accountability is the binding constraint: Any decline in corruption (regardless of the cause) is accompanied by a decline in "performance," measured as the average number of NREGS jobs for households other than the president's.

We test between these scenarios by comparing regression discontinuity coefficients and average non-president NREGS jobs over time across areas near and far from a cyber cafe. We draw on the same sample of candidates who won or lost their elections by a margin of 15 or fewer votes. Suppose $NREGS_{ip,t}$ is the days of labor allocated to candidate i in panchayat p in year t. Assume $[Year]_{st}$ is a year dummy that equals 1 if the observation is from year s (that is, if $s = t$). Let c be an index that equals 1 if panchayat p is near a cyber cafe, and 0 otherwise. To cut down on notation, we define the local linear control function

$$\Omega^c(x) = \pi^c_{0,x} + \pi^c_{1,x}[Margin]_{ip} + \pi^c_{2,x}[Margin]_{ip} \times [Win]_{ip} \qquad \text{where c is either 0 or 1} \qquad (2)$$

where as before $[Margin]$ is the vote margin of the candidate and $[Win]$ is a dummy for whether they won the election. This is simply the function that estimates the linear part of the RDD—the lines on either side of the cutoff in Figure 2. It is indexed by c to allow the control function to differ among panchayats close to or far from a cyber cafe.

Then our first specification is a dynamic version of Equation 1 that estimates a separate RD coefficient in each year:

$$NREGS_{ip,t} = \Omega^c(0) + \pi^c_{3,0}[Win]_{ip} + \sum_{s \neq 2013} [Year]_{st} \times \left(\Omega^c(s) + \beta^c_s[Win]_{ip}\right) + \nu_{ipt} \qquad (3)$$

The coefficients $\{\beta^c_s\}$ give the size of the discontinuity between winners and losers in year s relative to its size in 2013, the year before the election. For example, β^c_{2015} would, for the subsample c, be the discontinuity in the left-hand panel of Figure 2 minus the discontinuity in the right-hand panel. We estimate (3) and a simple year-by-year average of performance separately in the subsample of panchayats near and far from a cyber cafe ($c = 1, 0$).[50]

[50] The estimates of performance come from a regression

$$PERF_{pt} = \sum_{s=2011}^{2018} \theta_s[Year]_{st} + \nu_{pt}.$$

Though comparing (3) for areas close and far from a cyber cafe will give visual evidence of a difference (or lack thereof), it cannot show whether the difference is significant. For that we estimate a dynamic difference-in-discontinuities:

$$NREGS_{ip,t} = \Omega^0(0) + \pi^0_{3,0}[Win]_{ip} + \sum_{s\neq 2013}[Year]_{st} \times \left(\Omega^0(s) + \pi^0_{4,s}[Win]_{ip}\right) + [Cafe]_p \times \left[\Omega^1(0) + \pi^1_{3,0}[Win]_{ip} + \sum_{s\neq 2013}[Year]_{st} \times \left(\Omega^1(s) + \zeta_s[Win]_{ip}\right)\right] + \nu_{ipt} \tag{4}$$

where $[Cafe]_p$ is a dummy for whether the panchayat is near a cafe. The dynamic difference-in-discontinuity ζ_s estimates the difference $\beta^1_s - \beta^0_s$.

We make a similar comparison for performance, as defined above. Since performance is estimated as a simple mean, we use a simple difference-in-differences to estimate the dynamic comparison of areas close to a cyber cafe with those that are far:

$$PERF_{p,t} = \pi^0_{0,0} + \sum_{s\neq 2013}\pi^0_{1,s}[Year]_{st} + [Cafe]_p \times \left[\pi^1_{0,0} + \sum_{s\neq 2013}\theta_s[Year]_{st}\right] + \nu_{ipt} \tag{5}$$

where the coefficients $\{\theta_s\}$ give the change in performance since 2013 in areas near a cafe relative to those far from a cafe.

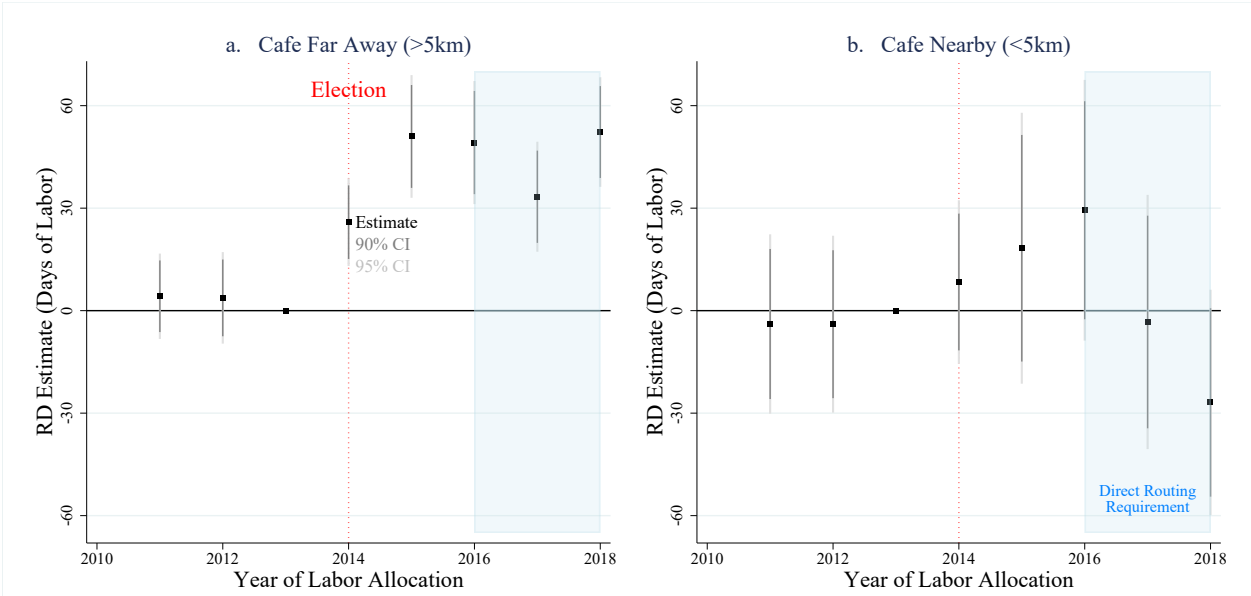
6.2 Results of the Tests: Access to Technology and Timeliness of Information Are Binding Constraints, but Desire for Accountability is Not

Panel a of Figure 4 plots estimates of Equation 3 in areas far from a cafe and near a cafe. The estimates make it clear that corruption takes an entirely different course in places near a cafe. While corruption remains positive and significant in areas far from a cafe, it is effectively gone in areas near a cafe by 2018. The year-by-year RD discontinuity graphs (Appendix Figures A1 and A2) confirm that these estimates are not a fluke. The visual pattern of the raw data implies the

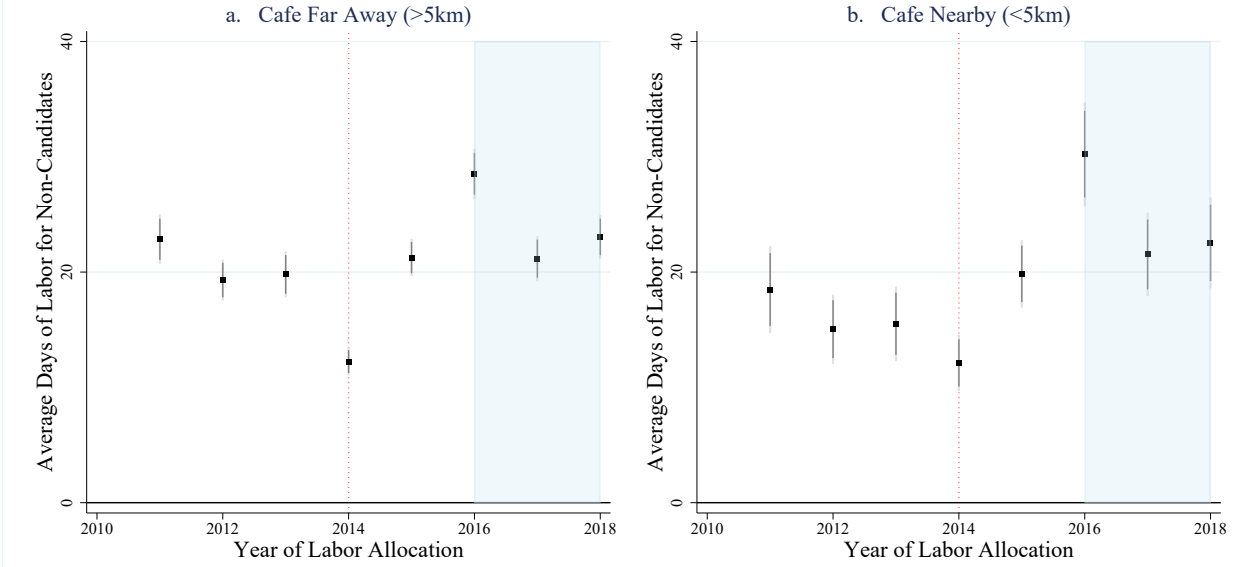
For all estimates of performance we restrict the sample to panchayats where we matched the president (rather than only the runner-up) because otherwise we cannot identify and exclude the president, whose allocation is large enough to actually skew the average.

Figure 4
Dynamic Effects by Internet Cafe Distance

a) Corruption Declines in Areas Near a Cyber Cafe After the Direct Routing Requirement



b.) There is no Commensurate Decline in Performance



Note: All standard errors are clustered by panchayat. **a.)** We estimate Equation 3 separately for areas far from and near a cyber cafe. The graphs plot estimates and confidence intervals the discontinuity between close election winners and losers relative to its size in 2013. **b.)** Graphs show simple means of NREGS days provided to non-candidate households.

discontinuity remains positive and largely unchanged among panchayats far from a cyber cafe, but closes among those close to one.

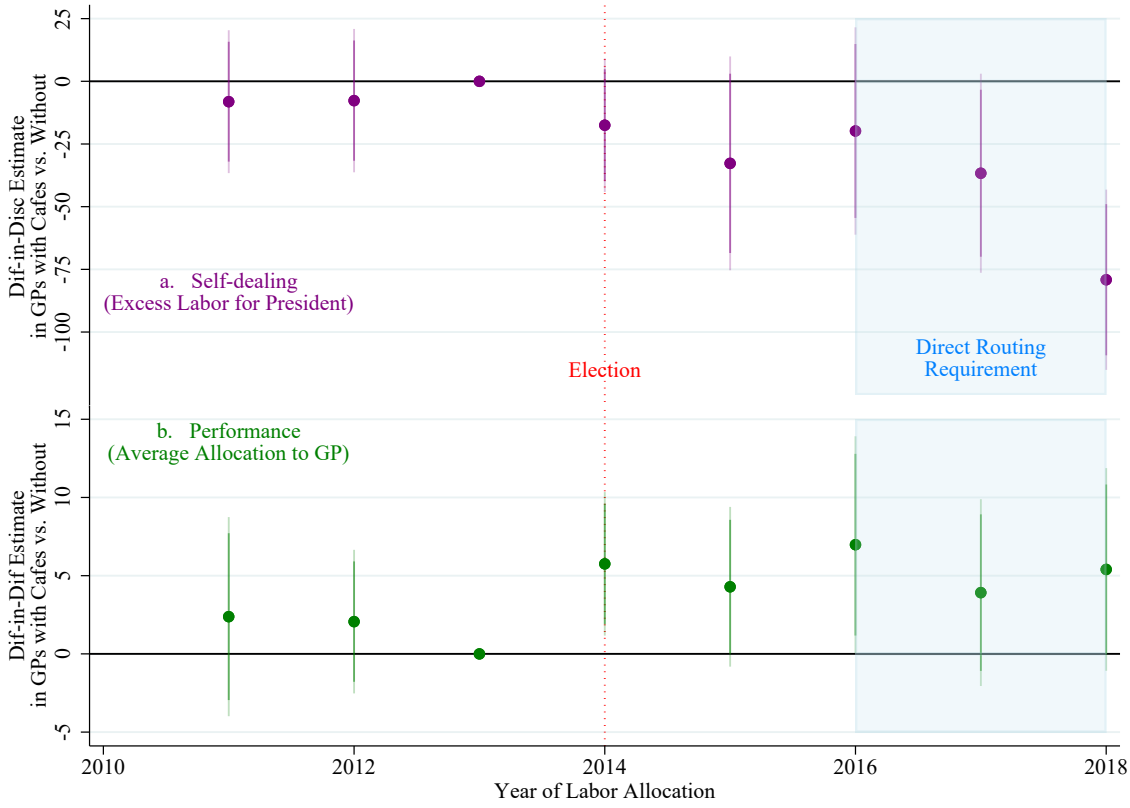
Even though the point estimates in areas near a cafe are smaller, they are positive and actually increasing until after the post-2016 upgrade. That suggests that *Access to Technology* cannot be the only binding constraint. Likewise, the fact that the post-2016 decline in corruption happens only in areas near a cafe suggests *Timeliness of Information* is likewise not the only binding constraint. Both constraints need to be alleviated simultaneously, as was the case post-2016 in areas near cyber cafes, for self-dealing to decline.

What about the *Desire for Accountability* assumption? Panel b of Figure 4 plots our measure of performance, the average NREGS benefits among non-candidates. The averages are similar in villages close to and far from a cyber cafe even after the post-2016 NREGASoft upgrade. The pattern suggests there is no trade-off between corruption and performance, as would have been implied if *Desire for Accountability* were a binding constraint.

We test whether the difference in corruption and performance between areas with and without cafes is statistically significant by estimating Equations 4 and 5. Figure 5 plots both sets of estimates with 90 and 95 percent confidence intervals. The upper part of the figure shows that although the point estimates are negative after the election—meaning areas near a cafe have lower corruption—they do not become statistically significant until after 2016 when the Ne-FMS requirement begins. The reduction in corruption becomes marginally significant in 2017 and highly significant by 2018. The lower half of the figure confirms there is no similar decline in performance. If anything, average allocations for non-candidate households are actually higher in areas near a cyber cafe, and the trends are unchanged under the central government policy change.

Overall, our results provide an interesting empirical pattern. Implementation quality as measured by the average days of NREGS labor citizens receive is unaffected by the distance to the nearest cyber cafe and is about the same in 2015 and 2018, i.e. before and after the NREGASoft reform. Self-dealing, on the other hand, is lower in villages close to a cyber cafe, with village council presidents allocating themselves fewer days of NREGS labor by 2018 than the typical villager receives. In contrast, the reform appears to not have any lasting effect in villages far from a cyber cafe. These results are not consistent with a corruption-as-compensation explanation,

Figure 5
Difference in Estimates: Near Cafe versus Others



Note: All standard errors are clustered by panchayat. **a.)** The dots and lines show estimates and confidence intervals from Equation 4. These difference-in-discontinuity estimates are essentially the difference between the estimates in the right-hand versus left-hand panel of Figure 4.a. These estimates use the same bandwidth as the main specification in Section 4.1, but Appendix A.4 shows that the results are not sensitive to bandwidth. **b.)** Graphs show estimates of Equation 5, which are difference-in-differences estimates of the average allocations to non-candidate households in areas near versus not near a cafe. It is essentially the difference between the estimates in the right-hand versus left-hand panel of Figure 4.b.

since there is no reason why changes in the timeliness of information on the NREGS website should lower self-dealing if it is accepted by villagers in return for more generated NREGS jobs. We also do not find a similar decline in performance, so presidents are not just lowering their own compensation because they provide less NREGS employment than before.

Instead, the results are consistent with imperfect monitoring being the main obstacle. In areas far from cyber cafes, technical difficulties in accessing the NREGS website lower monitoring capacity, and the 2016 reform to the website therefore has little effect on self-dealing or program performance. In areas that are closer to an internet cafe, the increased incentive to regularly visit the NREGS website appears to be effective for eradicating self-dealing within two years without reducing the NREGS labor for other households.

6.3 Addressing Lingering Concerns about the Mechanisms

6.3.1 Is There Evidence that Villagers Use the NREGS Website?

There is ample anecdotal evidence that use of the NREGS website has been increasing, especially after electronic payments began. A recent report by a civil society group notes that

> Over the years... the availability of information on the online MIS has become the only channel of information for the field functionaries and workers alike. Based on our continued engagement in Jharkhand and Rajasthan, the field functionaries have said that if the worker had any questions about work or wage payments, they would ask the computer operator in the block or inquire at the bank ...

In our own survey of council presidents in our sample, we find that over two-thirds believe most of the villagers in their panchayat know about the NREGS website. Over 80 percent of pradhans say that at least a few of their constituents have visited the site. Since it takes only a single worker to notice an anomaly and alert the village, a few regular visitors in each village may be sufficient.

The popular press has reported on several incidents where a villager or local activist has detected corruption on the website, often while checking for their own NREGS payment. One tells the story of a young middle school dropout from Muzaffarpur, Bihar, who visited a cyber cafe to investigate complaints from fellow villagers that they never got work under NREGS. He printed out job card records (the same sort used to construct our dataset) to take home, using them to prove to his neighbors that the village president had embezzled most of the NREGS budget.[51]

[51] https://www.civilsocietyonline.com/mega-hall-of-fame/mgnrega-hero-sanjay-sahni/

In Uttarakhand itself a farmer from Pithoragarh district discovered a massive embezzlement scheme masterminded by the president and her husband. He is quoted by the *Times of India* saying

> On the website (http://nrega.nic.in), around 80 different works had been shown as done under the scheme in the gram panchayat. The truth was that none of these had actually been done at all.[52]

In these anecdotes the online sleuths then confront their officials directly. Council presidents that we surveyed seemed to expect villagers would confront and punish rogue presidents. Roughly 75 percent said that most presidents who make unpopular NREGS allocations would be sanctioned by villagers. Angry confrontations with villagers were the most commonly mentioned type of informal sanctions, followed by exclusion from social events and threats of violence. These responses are consistent with surveys of citizens. In the 2006-2008 Rural Economic Development Survey (REDS), nearly 75 percent of respondents said they believed that it is not hard to hold their local officials accountable.

6.3.2 Are Cyber Cafes Just Proxying for Something Else?

One natural concern about the results is that the presence of a cyber cafe is likely correlated with the presence of any number of other facilities. Though it is not possible to control for every potential confounder, we can run a simple "horse race" specification that modifies Equation 4 to measure only how the RD estimate changes from 2015 to 2018, but allowing us to "net out" the effect of being near other infrastructure.

Let $z = 0, 1$ index whether the panchayat is near some "control" piece of infrastructure other than a cyber cafe, and let all other terms be as defined in Equation 4. We estimate

$$\begin{aligned} &NREGS_{ip,2018} - NREGS_{ip,2015} \\ &= \Omega^0(0) + \beta^m[Win]_{ip} + [Cafe]_p \times \left[\Omega^c(0) + \beta^c[Win]_{ip}\right] + [Near\ \ z]_p \times \left[\Omega^z(0) + \beta^z[Win]_{ip}\right] + \nu_{ip} \end{aligned} \tag{6}$$

where Ω is as defined in Equation 2. This specification tests for how the *change* in NREGS allocations from 2015 to 2018 is different for winners versus losers. As before, $[Cafe]_p$ is a dummy for

[52] https://timesofindia.indiatimes.com/city/dehradun/farmer-exposes-scam-in-rural-jobs-scheme-threatened/articleshow/47166893.cms

Table 4

Difference-in-Discontinuity Estimates, Part 1: Change in Self-Dealing from 2015 to 2018

	Change in NREGS Allocation, 2015 to 2018							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Est.	-13.138**	-8.275	1.296	5.451	8.352	-0.076	-0.924	9.742
	(5.745)	(7.041)	(7.568)	(8.907)	(10.812)	(7.510)	(8.544)	(11.908)
RD Est. X Near Cyber Cafe			-46.417**	-39.636**	-44.017**	-57.198**	-50.408**	-54.338**
			(18.191)	(18.379)	(18.869)	(22.903)	(20.168)	(22.468)
RD Est. X Near Post Office				-14.761				-19.434
				(14.449)				(15.380)
RD Est. X Near Major Road					-14.522			-16.126
					(14.634)			(14.653)
RD Est. X Near Bank						16.889		21.019
						(24.370)		(26.550)
RD Est. X Near Market							11.321	15.291
							(15.831)	(16.218)
Observations	1071	663	663	663	663	663	663	663
Panchayats	742	460	460	460	460	460	460	460

Note: We present difference-in-discontinuity estimates (Equation 6). Standard errors are clustered by panchayat. Each estimate gives the size of the discontinuity within the subset of panchayats near the specified facility (relative to panchayats not near it). Column 2 runs the same specification as Column 1 but after restricting the sample to observations that we can match to the 2011 Census.

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

whether the panchayat is near a cafe. We analogously define $[Near\ \ z]_p$ as a dummy for whether the panchayat is near the "control" infrastructure. These two dummies are not mututally exclusive.

The coefficient β^c tests whether the reduction for winners is greater in panchayats near a cyber cafe, and β^z does likewise for panchayats near some other facility (e.g. a bank or a post office). Estimating these discontinuities simultaneously lets us test whether the differential impact of being near a cyber cafe remains even after controlling for these other factors. Meanwhile, β^m tests for whether there is a reduction in corruption among panchayats far from all of these facilities.

Table 4 estimates Equation 6 using several different facilities for the "control" infrastructure. As above, we mark a panchayat as "close" to a cafe or control infrastructure if any of the constituent villages lies within 5 kilometers (which is the same definition as Figure 4). Table A6 in the appendix confirms that the results are robust to using 10km as the cutoff.

Column 1 shows the pooled regression discontinuity estimate across all panchayats. The

Table 5

Difference-in-Discontinuity Estimates, Part 2: Change in Self-Dealing from 2015 to 2018

	Change in NREGS Allocation, 2015 to 2018						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD Est.	3.073	-1.506	8.903	5.656	2.492	7.576	8.648
	(8.187)	(10.744)	(14.808)	(9.656)	(13.808)	(13.395)	(23.354)
RD Est. X Near Cyber Cafe	-44.763**	-46.161**	-47.761***	-48.310***	-47.740**	-47.890***	-48.599**
	(18.124)	(18.348)	(18.310)	(18.462)	(18.465)	(18.121)	(19.305)
RD Est. X Dist. to town		0.082					0.193
		(0.218)					(0.267)
RD Est. X Dist. to HQ			-0.113				-0.153
			(0.172)				(0.210)
RD Est. X SC Frac.				-17.057			-1.802
				(28.780)			(33.327)
RD Est. X Tot. Pop					0.001		0.007
					(0.022)		(0.030)
RD Est. X Primary Schools						-3.454	-2.162
						(6.719)	(9.613)
Observations	658	663	663	663	663	663	658
Clusters	455	460	460	460	460	460	455
Block FEs	X						X

Note: We present difference-in-discontinuity estimates (Equation 6). Standard errors are clustered by panchayat. Each estimate is gives the size of the discontinuity within the subset of panchayats near the specified facility (relative to panchayats not near it). *p=0.10 **p=0.05 ***p=0.01

point estimate is negative, suggesting that on average there is a reduction in self-dealing across all panchayats. The estimate remains negative in Column 2, which restricts to the subsample of panchayats successfully matched to the 2011 Census, though the estimate loses statistical significance. Column 3 simultaneously estimates the differential size of the discontinuity among panchayats near cyber cafes. The estimates in Panel A suggest that panchayats near a cyber cafe see a highly significant reduction in self-dealing of 45 days, while there actually an *increase* in self-dealing of 1 day among those away from a cyber cafe (though the increase is not statistically significant).[53]

Columns 4—7 show how the impact of being near a cyber cafe changes when we "net out" the effect of also being near a post office, major roadway, bank, or market. The way to interpret the "RD Est. $\times$ Near Cyber Cafe" estimates in these columns is as the size of the discontinuity among panchayats that are near a cyber cafe but not near a post office, etc. The impact of being near

[53] The reduction for panchayats near a cyber cafe is calculated as the difference-in-discontinuity coefficient -46.417 plus the main discontinuity 1.296.

a cyber cafe remains significant and lies within one standard error of the estimate in Column 3. Column 8 simultaneously estimates all of the discontinuities together to test whether proximity to a cyber cafe is proxying for some combination of the other facilities. The estimate is largely unchanged, though the sheer number of coefficients makes the estimates slightly noisier.

Table 5 presents additional tests based on 2011 census characteristics other than local facilities. Column 1 adds block fixed-effects, and Columns 2—6 test for whether the estimates hold after partialling out the difference-in-discontinuities from being close to a town, to the district headquarters, being in a panchayat with higher fraction of scheduled caste people or a higher population, or a higher number of primary schools. Column 7 controls for all simultaneously. Again, the cyber cafe difference-in-discontinuities remains largely unchanged.

Finally, although for consistency the estimates in Tables 4 and 5 use the same bandwidth as the main specification in Section 4.1, Appendix A.4 shows that the results are essentially unchanged when we calculate the Calonico et al. (2014) optimal bandwidth.

Tables 4 and 5 imply that cyber cafes are not merely proxies for being near a transportation or commercial center. We cannot rule out that being near a cyber cafe is a proxy for some less easily observed institution—for example, the presence of journalists and local activists who can monitor a president's behavior after the reform and publicize their findings among the village population and beyond. An alternative mechanism like this would nevertheless be consistent the idea that certain institutions are needed to convert *de jure* transparency into actual transparency.

6.3.3 Is it Petty Self-Dealing, or a System to Maintain Power through Patronage?

Another concern is that the excess allocations to the president are not individual self-dealing but a systematic campaign of patronage by a political machine. A political machine might be able to sustain corruption in the face of scrutiny because it can undermine checks by voters or independent institutions. But as described in Appendix A.2.1, the most obvious tests are inconsistent with patronage. We show that members of the president's extended family (job card holders with the same father or closest male relative) do not receive excess jobs. We also collected the NREGS allocations of the most senior members of the panchayat council other than the presi-

dent. We find that these senior members—who in theory should play a key role in checking the president—are no more likely to receive large NREGS allocations than a typical villager.[54] Finally, we find no evidence that members of the president's caste category (scheduled caste, scheduled tribe, or other caste) receive favorable treatment.

6.3.4 Could the Results Be Driven by Social Audits?

One might be concerned that the policy change that we exploit happens to coincide with other changes to the implementation of NREGS. The other big change to NREGS in the same year was an attempt by the central government to improve the effectiveness of social audits in Indian states.[55] Concerned with the low number of social audits of NREGS happening in most Indian states, the central government mandated a more top-down organization of NREGS audits. If social audits of NREGS increased massively in the following years, or if village council presidents reduced their self-dealing in anticipation of an audit, this could be driving our results.

In practice, we find no evidence that this is the case. Uttarakhand had not carried out any social audits of NREGS before the mandate and was slow to get started afterwards. We scrape the entire social audit calendar for Uttarakhand from the NREGS website, which contains information on the exact dates a village carried out a social audit. Matching this to our main dataset, we find that audits in Uttarakhand ramped up mostly after the end of our analysis period. In Appendix Figure A5, we repeat our analysis from Figure 5 but control for whether the panchayat was audited in the year of observation and its interaction with the indicator variable for winning the presidency. Our results are almost unchanged. In Appendix Figure A6, we instead drop all panchayats that were ever audited during our sample period or that were audited in the year after our analysis period ends, which tests whether anticipation of a future audit might be leading to reductions in corruption. Again, our results are qualitatively similar to Figure 5, suggesting

[54] These results are in contrast to those of Shenoy and Zimmermann (2021), who find evidence in West Bengal of coordinated corruption by a political machine. The difference in results likely arises from a difference in context. West Bengal runs explicitly partisan elections, where most candidates are backed by a political party. Council presidents in West Bengal are also indirectly elected by the rest of the council, making their power contingent on the support of their party. By contrast, presidents in Uttarakhand are directly elected and may not declare a party affiliation. Given that panchayats in Uttarakhand are an order of magnitude smaller than those in West Bengal, it less likely they need the help of a political organization to contest the election. Indeed, our survey of presidents in Uttarakhand shows that the most popular method of campaigning is to invite voters for dinner.

[55] For research focusing on the impact of NREGS social audits see e.g. Afridi and Iversen (2014).

that social audits are not driving our results.

6.3.5 Why Does the Reform Affect Self-Dealing More Heavily than Program Performance?

Interestingly, the reform affects self-dealing more heavily than program performance. Why might this be the case? 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 presidents do not actually work the jobs they are paid for and hide or manipulate the physical muster rolls. Then it would be hard to detect self-dealing without accessing the NREGS website.

While the NREGASoft reform should increase any risk of being reprimanded for self-dealing by the higher administration equally in both types of villages, it plausibly provides citizens in areas with easier internet access an increased incentive to check the website for real-time information about NREGS allocations and the status of wage payments. From there, it is only one click to learn more about who else is working on a particular project, where self-dealing will become more easily apparent. While this was possible before, the 30 day delay between the date when work was carried out on a project and when it was reflected in the online system required much better recollection of the people assigned to a project than checking the NREGS allocations in real time as workers have just completed their employment spell and are very likely to remember who was present at the worksite. Similarly, other stakeholders like NGOs, the political opposition or the media are able to monitor politicians' self-dealing in close to real time after the reform. The capacity to monitor politician behavior via the public website in conjunction with improvements to the provided value of visiting the website regularly is therefore most consistent with the empirical results.

6.3.6 Why is Self-Dealing Not Higher in Villages with Worse Internet Access?

We find that an average president's own job allocation is approximately 58 days, while a typical villager is allocated 21 days. In areas with lower monitoring capacity, presidents allocate up

to 70-80 days to their own household. It may be surprising that self-dealing is not higher than it actually is, given the annual legal limit of 100 days. One might expect that presidents would maintain a good enough performance of NREGS implementation for their villagers while self-dealing as much as possible. The automatic checks for suspicious activity built into NREGASoft could provide a potential explanation for this. 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 household to the muster rolls, but they can only add their household once, especially in the post-2016 world where payments are directly linked to a beneficiary's bank account. The detection probability may then increase rapidly when one worker, the president, is allocated more NREGS days than everyone else assigned to the same project, and the expenditure management software automatically flags unusually large one-time expenditures or households exceeding the annual NREGS quota of 100 days. 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 far away from cyber cafes, institutional constraints could make it too costly for presidents to be able to create more self-dealing opportunities for themselves in the process.

6.3.7 Could the Results be Driven by Top-Down Rather than Bottom-Up Monitoring?

One last concern is that the reform may have helped the higher-level governments better monitor council presidents, and it was this top-down monitoring that reduced corruption. In its most basic form this hypothesis cannot explain why corruption declined only in areas near cyber cafes. As noted in Section 5.2 compliance with the reform was almost universal by 2017. And as shown in Section 6.3.2, there is no evidence that areas closer to the district headquarters saw any differential reduction in corruption.

It is also unlikely that cyber cafes enhanced compliance with the reform or the scope for monitoring. All panchayats already had some form of access to the internet, as they must enter muster rolls and submit fund transfer orders through NREGASoft. It is thus unlikely that a council president would need a cyber cafe to learn about the reform, or that the block program officer would be unable to communicate with council presidents in areas that have no cyber cafe.

That said, we cannot rule out that citizens use cyber cafes to directly complain to higher-level government officials. Likewise, we cannot rule out that presidents quietly reign in their own corruption in areas near cyber cafes because they anticipate this risk. We view either possibility as a different sort of bottom-up monitoring rather than top-down monitoring.

7 Conclusion

Using a unique dataset, our paper tests whether local politicians allocate themselves excess welfare benefits and explores potential explanations for such self-dealing. 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 combined with a reform that increased the timeliness of available information appears to lead to the eradication of self-dealing. In contrast, self-dealing remains high in areas where monitoring is plausibly more difficult. Our results are most consistent with the explanation that the built-in institutional constraints imposed on village council presidents by the fund management software limit corruption, but that self-dealing can only be completely eradicated if villagers or other stakeholders have the capacity and incentive to monitor presidents' behavior. This is in sharp contrast to the explanation provided by the politicians themselves, who claim that their self-dealing is acceptable compensation for putting in the effort of implementing the program.

Our results highlight discrepancy between *de jure* and *de facto* transparency and identify potential barriers. Completely eliminating corruption may require additional investments 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 those in other 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 and the ability to hold a politician accountable alone do not seem to be enough to eradicate corruption. Citizens, or other

players like the media or local NGOs, may also have to be better enabled to pro-actively monitor program implementation.

One caveat to our results is that even though we do not see a relationship between performance and corruption at the village level for the average household, we cannot rule out that the corruption-as-compensation mechanism might be at play at the electoral support group level. In Uttarakhand, this concern is low because less than 10 percent of council presidents in our survey report having relied on the support of a caste, religion, or political party in their election campaign. That said, there might be other non-ethnicity/caste-based support groups pradhans may reward in exchange for corruption.

Finally, our findings and interpretations are based on using self-dealing as a proxy for corruption. However, corruption takes many forms and it is a limitation of our study that our results cannot speak to the changes in overall corruption of pradhans. Indeed, since *de facto* transparency and accountability increased for the allocation of NREGS labor days, this would create an incentive for pradhans to seek opportunities for corruption that are less visible to workers (e.g., NREGS material expenditures) or even beyond NREGS (e.g., other rural social protection programs).[56] In those instances, the improved NREGS transparency and accountability system likely pushes pradhans into areas where corruption is costlier, because politicians would have already been exploiting them otherwise. We leave a comprehensive study of substitution across different forms of corruption to future research.

[56] As noted in Section 2.3, extensive transparency safeguards for NREGS materials expenditures, such as electronic payments directly into vendors' bank accounts and flags for suspicious payments, have been in place since well before the 2016 policy change. The 2016 policy could have made corruption on materials more attractive relative to self-dealing on the labor budget.

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A Empirical Appendix (For Online Publication)

A.1 Additional Graphs and Tables

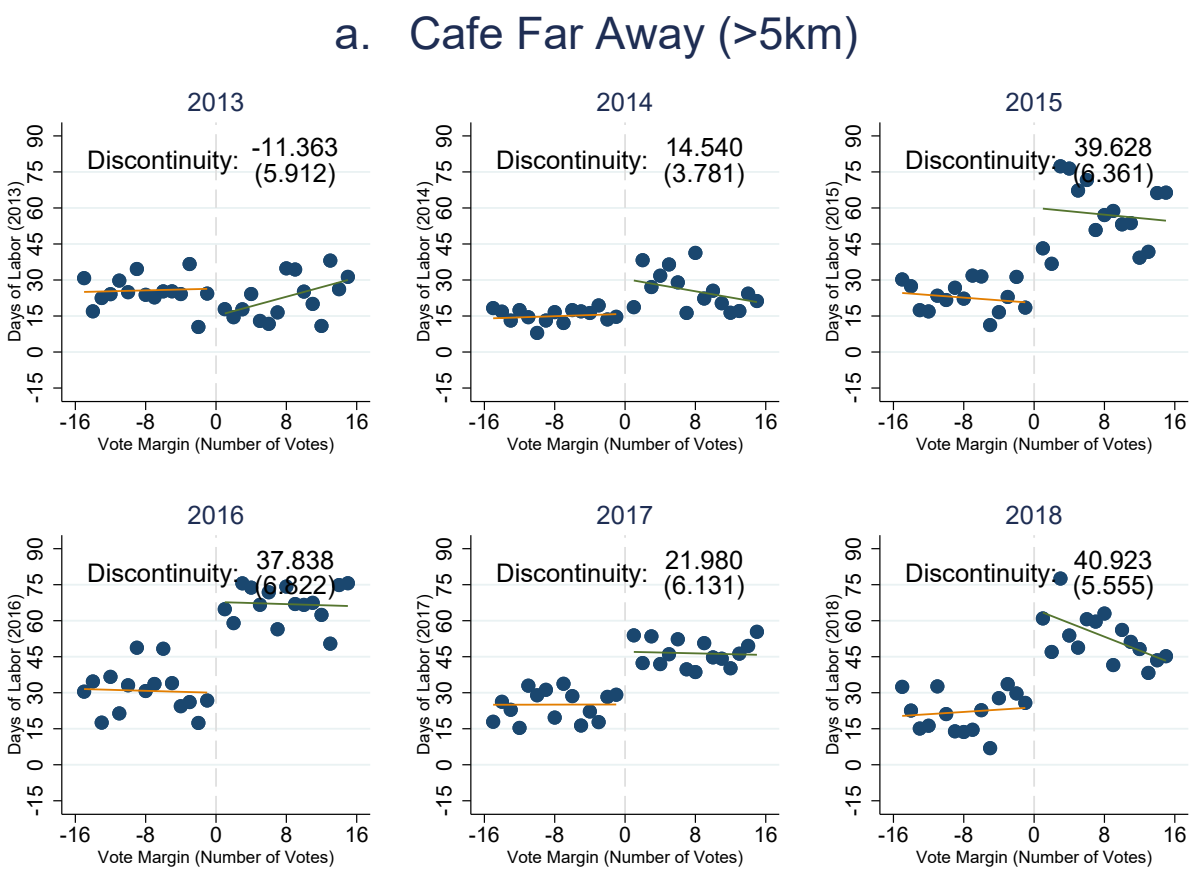
The results in this sub-section provide additional results or robustness checks for the results presented in the main paper.

Table A1
Robustness of Internet Cafe Results

	(1)	(2)	(3)	(4)	(5)
	Days of Labor for President				
Near Cafe	-9.747**	-8.795**	-7.833*	-8.433*	-11.532**
	(4.203)	(4.270)	(4.312)	(4.401)	(4.657)
Avg. Days	1.199***	1.175***	1.188***	1.192***	1.457***
	(0.110)	(0.111)	(0.112)	(0.110)	(0.139)
2011 Night Lights		-0.672	-0.539	-0.540	-1.102*
		(0.409)	(0.429)	(0.467)	(0.642)
Distance to Border			-0.089	-0.099	-0.324
			(0.092)	(0.094)	(0.418)
Distance to Dehradun			0.104***	0.110***	0.278
			(0.039)	(0.041)	(0.377)
Distance to Sub-Dist HQ			0.108	0.103	0.199
			(0.097)	(0.097)	(0.129)
Distance to District HQ			-0.013	-0.021	-0.187*
			(0.050)	(0.052)	(0.109)
Literacy				5.642	11.268
				(29.014)	(35.173)
SCT Fraction				-12.179	-4.814
				(10.405)	(10.058)
Constant	25.218***	27.722***	17.598**	17.260	11.147
	(2.933)	(3.343)	(8.911)	(22.129)	(51.200)
Observations	340	340	340	340	340
Block FEs					X

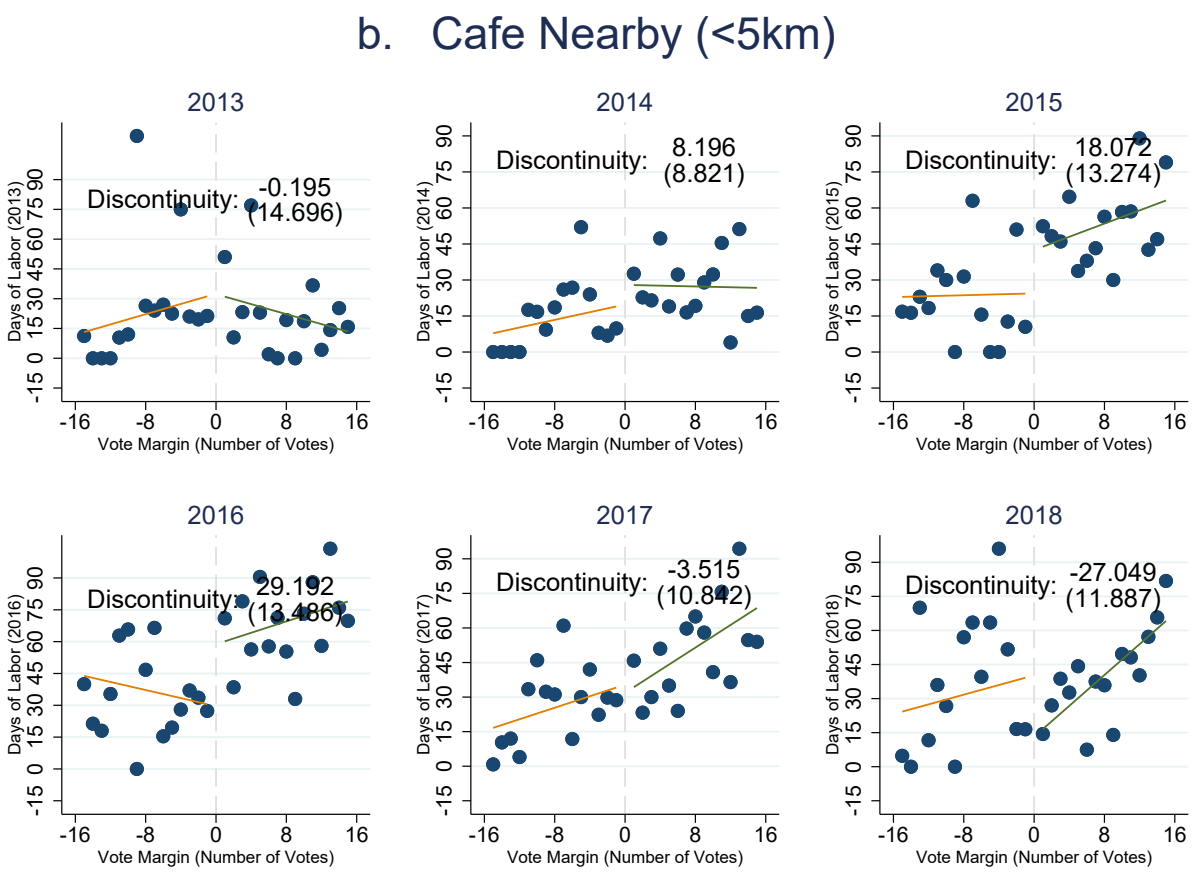
Note: Cafe refers to the nearest internet cafe according to Census data. Standard errors are robust to heteroskedasticity.
*p=0.10 **p=0.05 ***p=0.01

Figure A1
RD Results for Long Internet Cafe Distance



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



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

A.1.1 Differences Between Areas Near and Far from a Cyber Cafe

Table A2

Differences Between Areas Near and Far from a Cyber Cafe

	Far	Near	Diff	P_val
Near Post Office	0.26	0.67	0.41	0.00
Near Major Road	0.52	0.80	0.28	0.00
Near Bank	0.15	0.62	0.48	0.00
Near Market	0.21	0.61	0.40	0.00
Dist. to town	33.44	29.99	-3.46	0.21
Dist. to HQ	72.47	62.06	-10.42	0.01
SC Frac.	0.22	0.23	0.01	0.71
Frac. Literate	0.67	0.70	0.03	0.00
Tot. Pop	613.48	845.30	231.82	0.01
Primary Schools	1.60	1.58	-0.02	0.83
Fraction of Votes Rejected	0.04	0.04	0.00	0.05
Number of Candidates	3.61	3.91	0.29	0.09

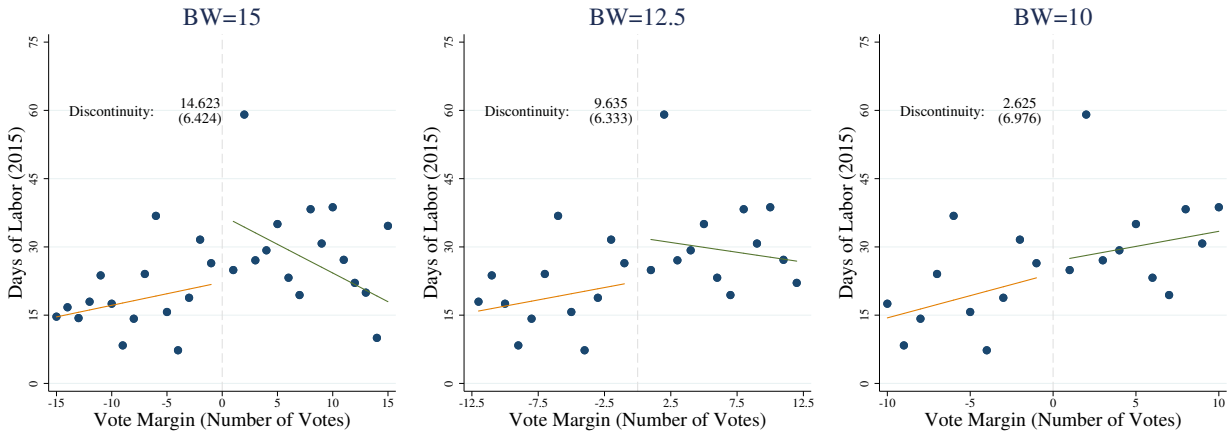
Note: The columns “Far” and “Near” give the mean of the variable for panchayats more than or less than 5 km distant from a cyber cafe. The next two columns calculate the difference and the p-value of the difference.

A.2 Extensions of the Analysis

A.2.1 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



Note: Standard errors are clustered by panchayat.

Figure A3 is drawn analogously to Figure 2, but showing NREGS days allocated to extended family. These estimates are more sensitive to the choice of bandwidth than our estimates from Figure 2, 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 A3
Regression Specifications Shown in Figure A3

	(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

Table A3 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 3). 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.

Finally, one could imagine patronage based on caste rather than family or political power. We do not observe the jati of any candidate (and few rural Uttarakhandis have informative last names that could be used to infer jati). But we can test whether members of the president's caste group (scheduled caste, scheduled tribe, other caste) receive favorable treatment. We calculate

the average allocation within the panchayat among job cards identified as being in the same caste group as the president. Table A4 shows that there is no evidence of any such favoritism.

Table A4
Tests for Caste Group Favoritism

	(1) Self (2015)	(2) Caste Group (2015)	(3) Self (2016)	(4) Caste Group (2016)
RD Estimate	37.40***	-0.65	34.24***	-0.65
	(4.46)	(1.08)	(4.93)	(1.08)
Observations	1105	1103	1105	1103
Clusters	757	755	757	755

Note: "Self" refers to payments to the candidate's own job card. "Caste Group" refers to the within-panchayat average of members of the president's broad caste group (SC, ST, other).
*p=0.10 **p=0.05 ***p=0.01

A.2.2 Dynamic Corruption Results are Similar for Reserved Constituencies

The dynamics of corruption are no different in reserved seats. Table A5 shows difference-in-discontinuity estimates similar to those estimated for cyber cafes in Table 4. There is no evidence that the reduction in self-dealing is any bigger or smaller in female-reserved or caste-reserved seats. This result differs from Afridi et al. (2017), who find that female politicians in Andhra Pradesh initially have worse NREGS implementation quality and higher program leakages, but catch up to male politicians during their term.

Table A5
Reservation and the Change in Self-Dealing from 2015 to 2018

	Change in NREGS Allocation, 2015 to 2018		
	(1)	(2)	(3)
RD Est.	-13.138**	-15.078*	-12.628**
	(5.745)	(8.287)	(6.380)
RD Est. X Female Reservation		4.563	
		(11.366)	
RD Est. X Caste Reservation			-0.033
			(14.227)
Observations	1071	1071	1071
Panchayats	742	742	742

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

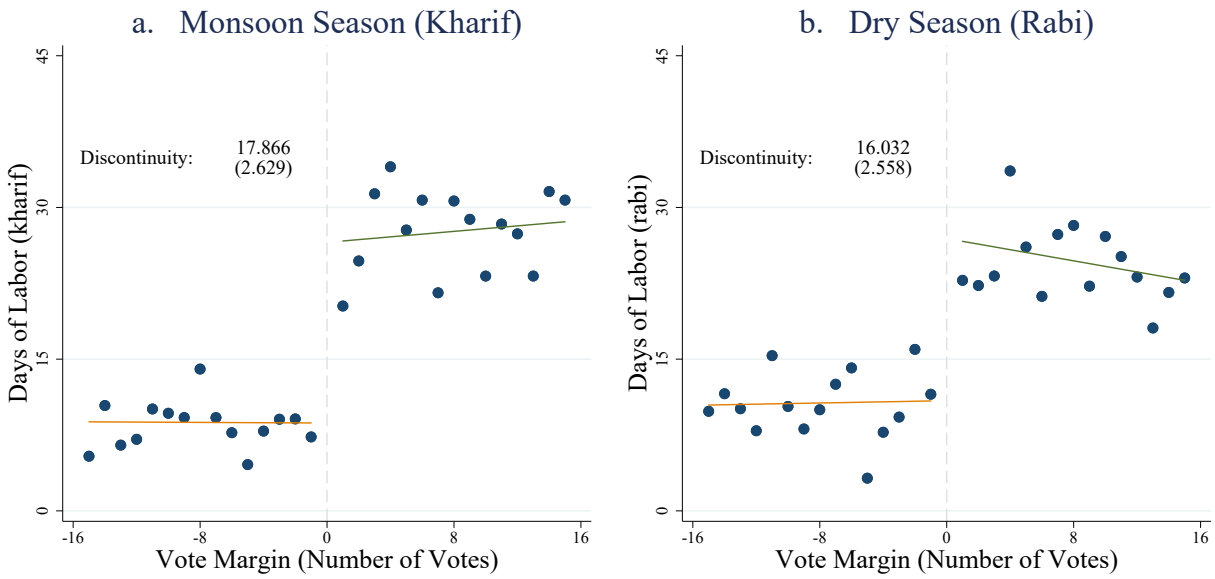
A.2.3 Does Self-Dealing Vary with Season?

We interpret excess jobs allocated to the president as self-dealing, but a more charitable interpretation is that presidents are working to keep projects running during times when regular households do not need NREGS labor. If that interpretation were valid, we would expect most of the excess labor allocated to the president's household to come during the monsoon season (kharif) when most households are busy with farm work. We would likewise expect little or no excess during the dry season (rabi) when most farmers cannot grow crops and need government support.

Figure A4 shows no evidence of such a pattern. Both the level of the president's labor allocation and the gap with the runner-up is largely unchanged between the two seasons. This pattern is inconsistent with the idea that presidents take jobs only when no one else wants them.

Figure A4

Self-Dealing is Similar During Kharif (Monsoon Season) and Rabi (Dry Season)



Note: Standard errors are clustered by panchayat.

A.2.4 Definition of "Near"

Table A6
Results of Table 4 are Robust to Using 10km
as the Cutoff for Being "Near" a Cafe or Other Piece of Infrastructure

	Change in NREGS Allocation, 2015 to 2018							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Est.	-13.138**	-8.275	8.090	10.298	18.669	8.164	10.019	19.625
	(5.745)	(7.041)	(8.686)	(11.577)	(14.583)	(9.160)	(10.581)	(17.359)
RD Est. X Near Cyber Cafe			-40.984***	-36.144**	-34.846**	-41.560***	-39.276**	-33.220**
			(14.176)	(14.681)	(15.849)	(15.652)	(15.922)	(16.884)
RD Est. X Near Post Office				-8.208				-6.088
				(14.721)				(14.990)
RD Est. X Near Major Road					-19.732			-19.911
					(18.151)			(18.515)
RD Est. X Near Bank						0.618		5.147
						(15.460)		(19.169)
RD Est. X Near Market							-5.265	-1.856
							(15.260)	(17.126)
Observations	1071	663	663	663	663	663	663	663
Panchayats	742	460	460	460	460	460	460	460

Note: We present difference-in-discontinuity estimates (Equation 6). Standard errors are clustered by panchayat. Each estimate is gives the size of the discontinuity within the subset of panchayats near the specified facility (relative to panchayats not near it).
*p=0.10 **p=0.05 ***p=0.01

Table A7

Results of Table 5 are Robust to Using 10km
as the Cutoff for Being "Near" a Cafe or Other Piece of Infrastructure

	Change in NREGS Allocation, 2015 to 2018						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD Est.	10.771	5.486	14.982	11.973	11.223	13.175	16.412
	(9.316)	(10.998)	(14.456)	(10.895)	(15.327)	(14.043)	(23.166)
RD Est. X Near Cyber Cafe	-42.799***	-40.749***	-41.100***	-42.240***	-42.876***	-41.851***	-44.774***
	(14.651)	(14.198)	(14.192)	(14.377)	(14.772)	(14.179)	(15.876)
RD Est. X Dist. to town		0.075					0.172
		(0.213)					(0.267)
RD Est. X Dist. to HQ			-0.105				-0.149
			(0.163)				(0.203)
RD Est. X SC Frac.				-14.429			-0.128
				(27.591)			(32.090)
RD Est. X Tot. Pop					-0.002		0.000
					(0.024)		(0.031)
RD Est. X Primary Schools						-2.622	0.192
						(6.869)	(9.808)
Observations	658	663	663	663	663	663	658
Clusters	455	460	460	460	460	460	455
Block FEs	X						X

Note: We present difference-in-discontinuity estimates (Equation 6). Standard errors are clustered by panchayat. Each estimate is gives the size of the discontinuity within the subset of panchayats near the specified facility (relative to panchayats not near it). *p=0.10 **p=0.05 ***p=0.01

A.2.5 More Details on the Ne-FMS Reform

In late May of 2016, the central government notified 10 states, including Uttarakhand, that local governments would now be required to use the improved National Electronic Fund Management System (Ne-FMS) for all NREGS wage payments. The explicit goal of the reform was to lower corruption by making the transfer of funds more transparent while simultaneously cutting down on wage payment delays by making the electronic payment system more efficient. The process works as follows: Once the work on a NREGS project is completed, the muster roll is closed and any remaining information (e.g. measurements of work completed) is entered into the online system. The panchayat then creates an electronic Fund Transfer Order (FTO) for each worker for the wage amount. The FTO is electronically signed by two panchayat or block level officials. Once the signatures are on the FTO, the payment process is out of the hands of local governments. This process is supposed to be completed within 8 days from the last day of NREGS work (Ministry of Rural Development, 2016).

The FTO is now automatically electronically transferred to the Public Fund Management System (PFMS) run by the Ministry of Finance of the central government. The central government's bank releases the necessary funds, and the wage payments are transferred into workers' bank accounts.[57] This process is supposed to be completed within 48 hours after receiving the FTO. Information about the transactions is automatically pushed to NREGASoft (Ministry of Rural Development, 2016).

From the perspective of the worker, the NREGS website makes these steps transparent. Once the FTO is generated, this is automatically noted as the 'payment date' in the system. The actual date when the money has been transferred into the beneficiary's bank account is noted as the 'credited date'. At any point, a visitor to the website can therefore check whether the NREGS wages have been paid and which steps have been completed and in what time since the completion of the project. This makes transparent whether delays are the 'fault' of the local or central government and when payment is expected. If all actors adhere to the official timeline, workers

[57] In practice, the money passes through the state government's bank NREGS bank account, but in contrast to the earlier e-FMS process this is just an automatic pass through that requires no action from the state government. If the transaction fails, for example due to incorrect bank account information or an inactive bank account, the problem needs to be resolved and a new FTO is generated.

should expect to get paid within 10 days after their NREGS work ends.

How did the Ne-FMS process differ from the earlier system? It improved the timeliness of wage payments and reduced the number of actors involved in the payment process, thereby increasing transparency and accountability. Importantly, the reform did not introduce FTOs or direct transfers into workers' bank accounts, nor did it change the role or process of local governments. All of these elements were already in place before the reform. Between 2014 and early 2016, all districts in Uttarakhand had started implementing the so-called e-FMS system which included all of these features, as requested by Comptroller and Auditor General of India (2013). Instead, the Ne-FMS reform improved the efficiency of the process once local governments had completed their part of the process. Before the automated Ne-FMS process under which the central government transfers payments directly to beneficiaries, state governments were responsible for the wage transfers. This made the payment system more complex, as the central government notes: 'The internal studies conducted on the reasons for the delayed payments [under the pre-Ne-FMS system] pointed out that the delays in release of funds by the Central Government [to the states], multi-level release system, continued parking of funds at various levels and the inability of the implementation agencies to get the funds in time for payment - were the main contributory causes for the increased delays. This calls for further steps to improve the system and to assure timely availability of funds as per demand.'

From the worker's perspective, the 2016 reform changed the situation in two main ways. First, it improved the availability of information on the NREGS website shortly after the completion of a NREGS work project. An FTO can only be generated if all the muster roll information has been entered and updated on NREGASoft. Only when this information has been digitized is it possible to track further changes made in the system, like the FTO creation date and the actual payment date. Second, the reform improved the timeliness of payments and increased transparency of where payment delays happen. A NREGS worker going to the bank to withdraw their wages can now check online why the wages are not yet in their bank account. This also directly allows a worker to check whether the responsibility lies with their pradhan (because the FTO has not been generated or signed) or with the central government. This makes it easier for workers or other stakeholders to hold the correct actor accountable and to double-check claims by the panchayat that they have completed their part of the process on time. Under the Ne-FMS

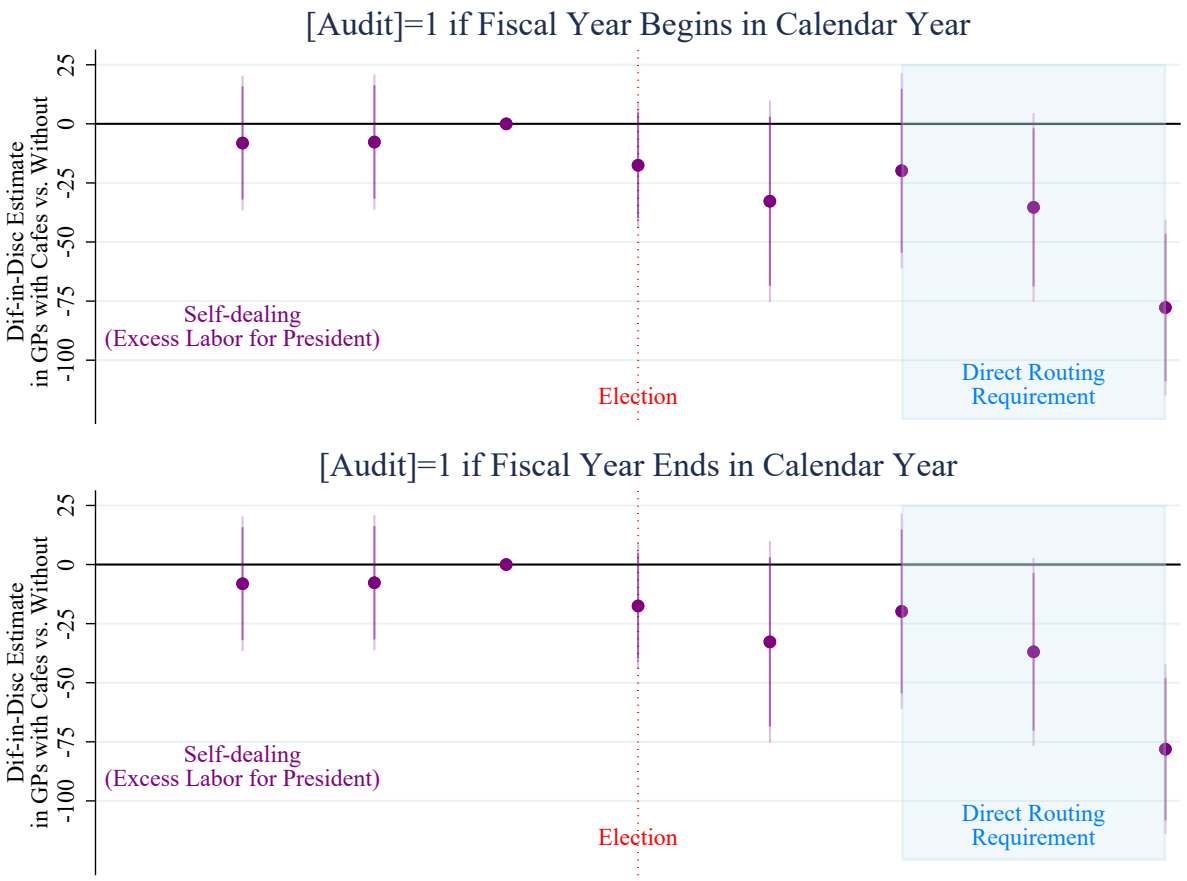
system, pradhans will typically be unable to answer voter questions about when transfers will be made once the FTO has been generated, and since the ability to directly question central government officials is limited, the website becomes the main source of information. Especially if a trip to the bank during work hours has high opportunity costs, checking the website or getting information from someone else that payments have been made could also be attractive.[58]

[58] In a survey of NREGS workers in Uttarakhand conducted in February to March 2017, Chakraborty and Mukerji (2017) find that the typical worker has a bank account for the sole purpose of receiving NREGS wages, but does not use banking services for other purposes. Almost all workers have to go to the bank in person to withdraw their NREGS payments, which requires travelling to the bank during business hours in a state where the bank branch network is still quite sparse.

A.2.6 The Results Are Not Driven by Social Audits

Figure A5

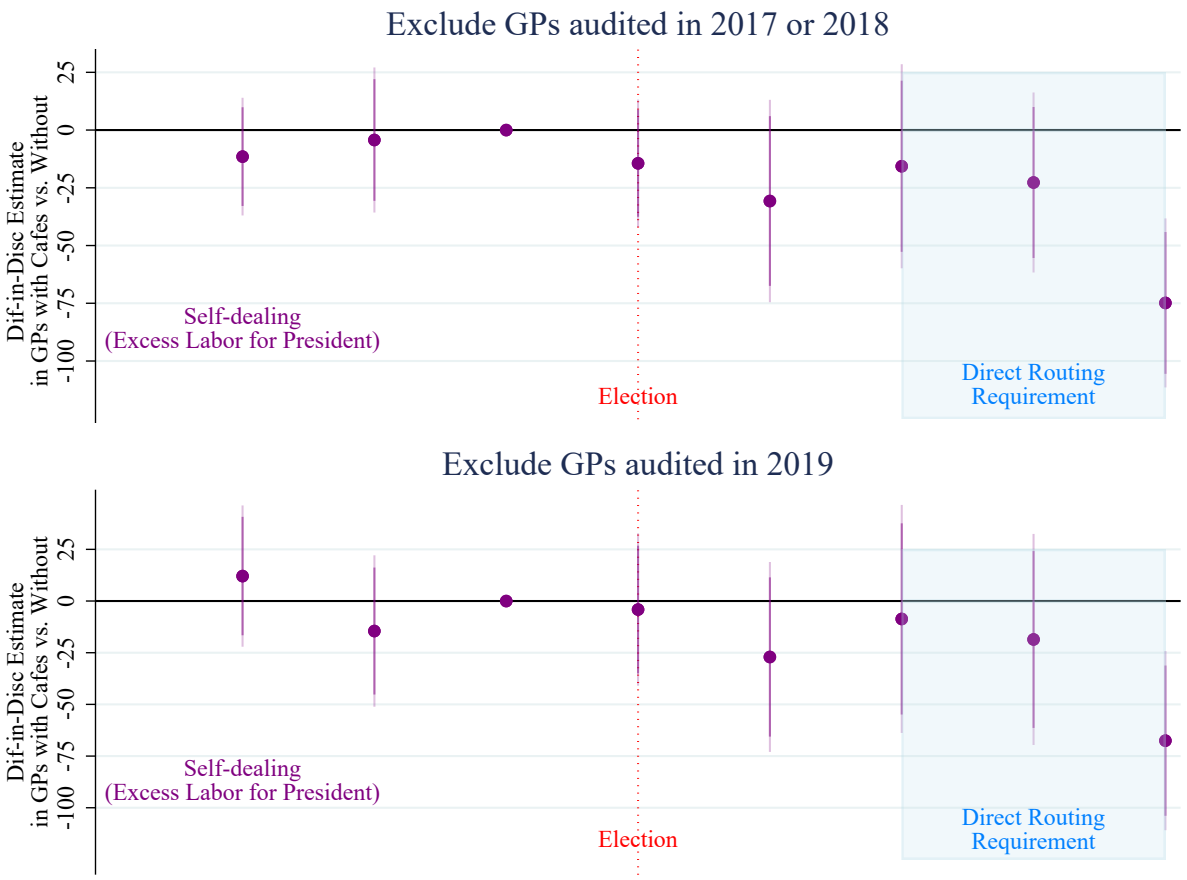
Difference-in-Discontinuity Results Are Unchanged When We Control for [Winner] X [Audit]



Note: This figure is identical to Figure 5a except that we control for the dummy for whether the panchayat was audited in the year of the observation, as well as its interaction with the $[Win]_{ip}$ dummy for whether the candidate won the presidency. Audit periods are based on fiscal years rather than calendar years. We allow for either disciplining or anticipatory effects by defining our audit dummy in based on either the beginning or end of the fiscal year.

Figure A6

Difference-in-Discontinuity Results Are Unchanged When We Drop GPs That Were or Would Be Audited

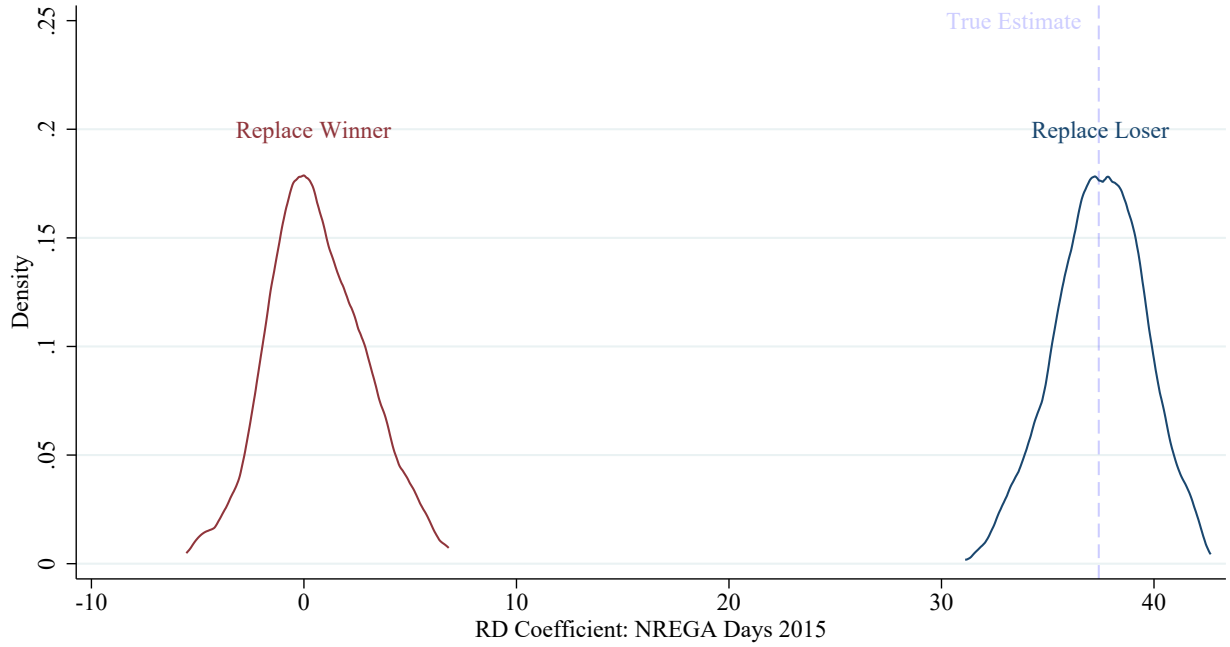


Note: This figure is identical to Figure 5a except that we drop panchayats that were audited either during our sample period (in fiscal year 2017 or 2018), or just after (fiscal year 2019). The latter ensures our results are not driven by anticipation of a future audit.

A.2.7 Permutation Tests

We extend the SUTVA analysis in Table 3 by assigning the winner and loser the days of labor in 2015 of a random non-candidate villager within their panchayat. We draw with replacement a sample of 300 "permutation" villagers. For our first set of tests, we swap the winner's labor allocation, keeping the loser's allocation unchanged. We re-estimate the RD estimates for each of the 300 permuted values. We then repeat the exercise in reverse, replacing the loser's days of labor while keeping the winner's unchanged. Figure A7 shows that when we replace the winner's allocation the estimates are clustered around zero. When we replace the loser's allocation the estimates are clustered around our main estimate (Column 1 of Table 3). The two distributions have no common support, validating that 1) our results are not driven by chance; 2) the winner's allocation is larger than that of nearly every other individual villager; and 3) there is no evidence that the loser is being punished relative to other villagers.

Figure A7
Random Permutation of Winner and Loser Confirms Results

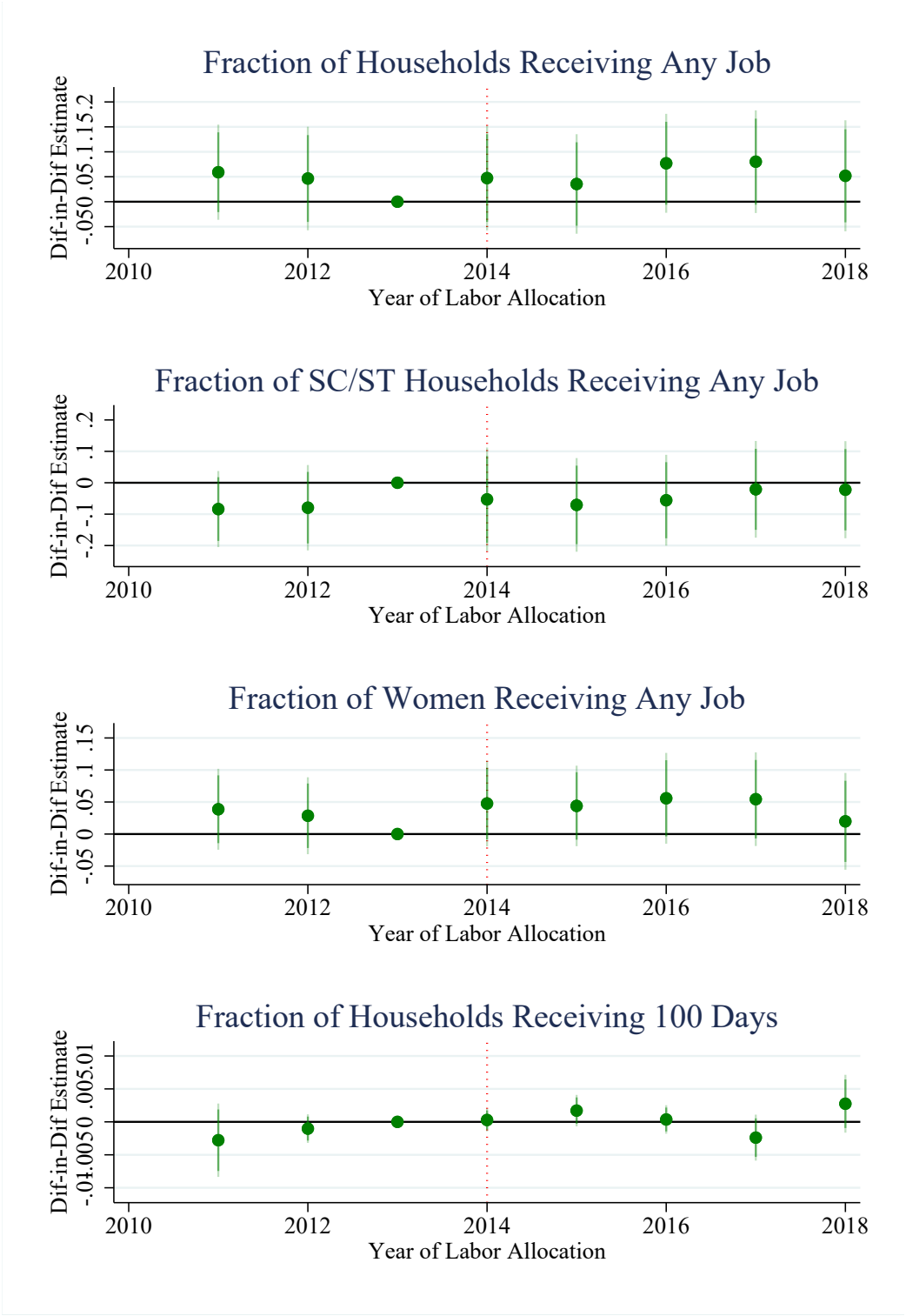


A.2.8 Additional Measures of Performance

Figure 5 shows that performance along the most obvious measure—the average days of labor created for non-candidate households—does not decline in areas near cyber cafes. This appendix tests for declines in four other measures of performance: the fraction of households who receive a job, the fraction of Scheduled Caste or Scheduled Tribe (SC/ST) households who receive a job, the fraction of women who receive a job, and the fraction of households who receive the full 100 days of labor they are entitled under law. Figure A8 shows no statistically significant evidence of a decline that coincides with the reduction in self-deal shown in the top panel of Figure 5.

Figure A8

No Evidence of a Decline in Other Forms of Performance in Panchayats Near a Cyber Cafe

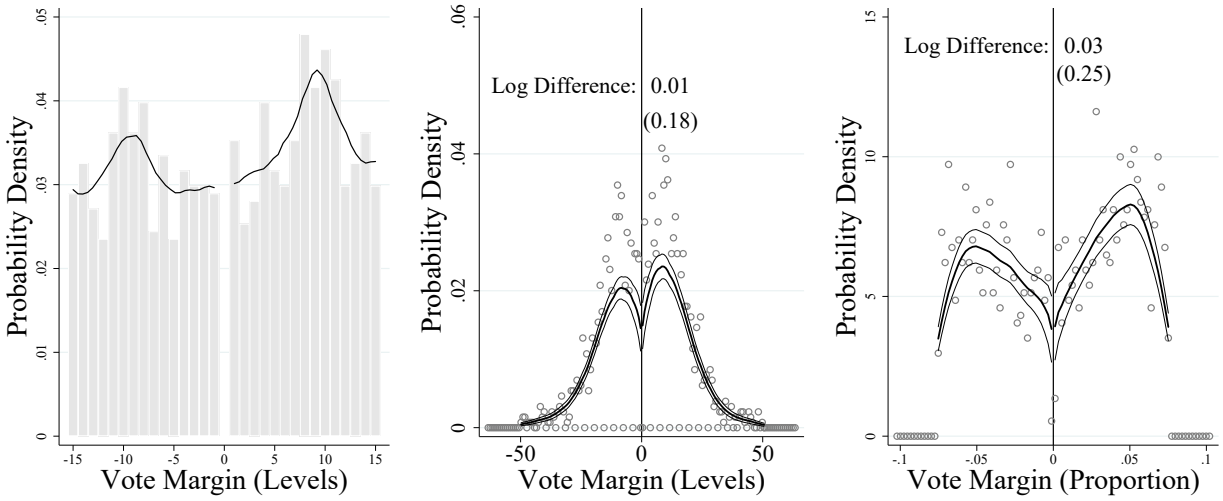


Note: We present difference-in-difference results (Equation 5) using the same specification as the bottom panel of Figure 5, but using different outcomes.

A.3 Density Tests

As noted in Section 4.3, the standard density tests are less useful for our running variable because the vote margin of the winner equals the margin of the loser with the sign flipped. In the full set of winners and runners-up, the density of the running variable is symmetric in the neighborhood of the cutoff. In practice any asymmetry or discontinuity can only arise in our final sample if we are systematically more likely to match winning (or losing) candidates to their job card records. Column 6 of Table 3.B tests this directly and finds no evidence of a discontinuity in the match rate.

Figure A9
No Evidence of a Discontinuity in the Density



Nevertheless we present density tests in Figure A9. The first panel shows a simple histogram where each bin shows the fraction of observations with a vote margin equal to the discrete number of votes on the horizontal axis. As explained in the main text (Footnote 30) there is no mass at zero because election officials are instructed to break ties randomly and add one to the winners official vote total (State Election Commission of Uttarakhand, 2008). To be precise the presiding officer will write each candidate's name on a slip of paper and draw one at random in the presence of the candidates, recording the drawn slip as though it were a vote.[59] There is no evidence

[59] There are a handful of cases where an officer nevertheless recorded a tie. These observations are removed before any further analysis.

of higher mass on either side of the cutoff, as confirmed by local polynomial fits represented as dark curves overlaying the histogram.

The second panel shows a McCrary (2008) density test on the margin of votes (again in levels), and the last panel runs the same test on the margin as a proportion of total votes. In neither case is there evidence of a discontinuity.

A.4 Optimal Bandwidth for the Difference-in-Discontinuity Results

The main text uses the same bandwidth for the difference-in-discontinuity estimates of Section 6.3 as the basic regression discontinuity in Section 4. That choice is mainly made for the sake of consistency. This appendix shows that the results are not sensitive to the choice of bandwidth.

To our knowledge there is no consensus on how to choose the optimal bandwidth of the difference-in-discontinuity specifications of Table 4 (Columns 3—8) and Table 5 (Columns 2—7). But we can apply the method of Calonico et al. (2014) to calculate the optimal bandwidth of

$$NREGS_{ip,2018} - NREGS_{ip,2015}$$

and use that bandwidth for the other specifications. Tables A8 and A9 use this bandwidth, which in practice is almost identical to the bandwidth used in the main text (14 versus 15). The results are largely unchanged.

In the case of the year-by-year difference-in-discontinuity results shown in Figure 5 there is no obvious way to calculate the optimal bandwidth, but Figure A10 shows that the results are similar for a range of bandwidths as wide as 22 and as narrow as 8.

Table A8

The Results of Table 4 are Robust to Using the Calonico et al. (2014) Optimal Bandwidth

Panel A: Dummies for Facility within 5 Kilometers of Any Village in the Panchayat

	Change in NREGS Allocation, 2015 to 2018						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD Est.	-13.438**	-9.324	-0.833	3.223	6.616	-2.117	-3.432
	(5.999)	(7.304)	(7.858)	(9.301)	(11.313)	(7.797)	(8.912)
RD Est. X Near Cyber Cafe			-40.886**	-34.087*	-38.161*	-50.928**	-44.858**
			(18.845)	(19.063)	(19.520)	(24.255)	(20.979)
RD Est. X Near Post Office				-14.684			
				(14.800)			
RD Est. X Near Major Road					-15.053		
					(15.246)		
RD Est. X Near Bank						15.176	
						(25.655)	
RD Est. X Near Market							12.021
							(16.282)
Observations	1008	627	627	627	627	627	627
Panchayats	703	439	439	439	439	439	439

Panel B: Dummies for Facility within 10 Kilometers of Any Village in the Panchayat

	Change in NREGS Allocation, 2015 to 2018						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD Est.	-13.438**	-9.324	6.181	7.128	16.716	5.934	7.078
	(5.999)	(7.304)	(8.985)	(12.066)	(15.193)	(9.430)	(10.948)
RD Est. X Near Cyber Cafe			-39.166***	-35.650**	-33.716**	-40.478**	-38.266**
			(14.719)	(15.043)	(16.669)	(16.276)	(16.527)
RD Est. X Near Post Office				-5.069			
				(15.089)			
RD Est. X Near Major Road					-19.351		
					(19.150)		
RD Est. X Near Bank						1.960	
						(16.090)	
RD Est. X Near Market							-2.402
							(15.772)
Observations	1008	627	627	627	627	627	627
Panchayats	703	439	439	439	439	439	439

Note: This table is identical to Table 4 except that this table uses the optimal bandwidth for the change in the labor allocation from 2015 to 2018 (as opposed to the level of the 2015 allocation).
*p=0.10 **p=0.05 ***p=0.01

Table A9

The Results of Table 4 are Robust to Using the Calonico et al. (2014) Optimal Bandwidth

Panel A: Dummies for Facility within 5 Kilometers of Any Village in the Panchayat

	Change in NREGS Allocation, 2015 to 2018						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD Est.	0.794	-2.924	8.388	3.336	5.120	5.113	9.549
	(8.536)	(11.189)	(15.479)	(10.204)	(14.221)	(14.081)	(24.386)
RD Est. X Near Cyber Cafe	-37.104**	-40.335**	-42.059**	-43.000**	-43.771**	-42.326**	-42.446**
	(18.652)	(19.030)	(19.000)	(19.215)	(19.023)	(18.792)	(19.656)
RD Est. X Dist. to town		0.064					0.160
		(0.222)					(0.271)
RD Est. X Dist. to HQ			-0.133				-0.165
			(0.177)				(0.218)
RD Est. X SC Frac.				-16.249			8.756
				(30.486)			(34.927)
RD Est. X Tot. Pop					-0.008		-0.015
					(0.023)		(0.032)
RD Est. X Primary Schools						-2.907	3.080
						(6.923)	(9.853)
Observations	621	627	627	627	627	627	621
Clusters	433	439	439	439	439	439	433
Block FEs	X						X

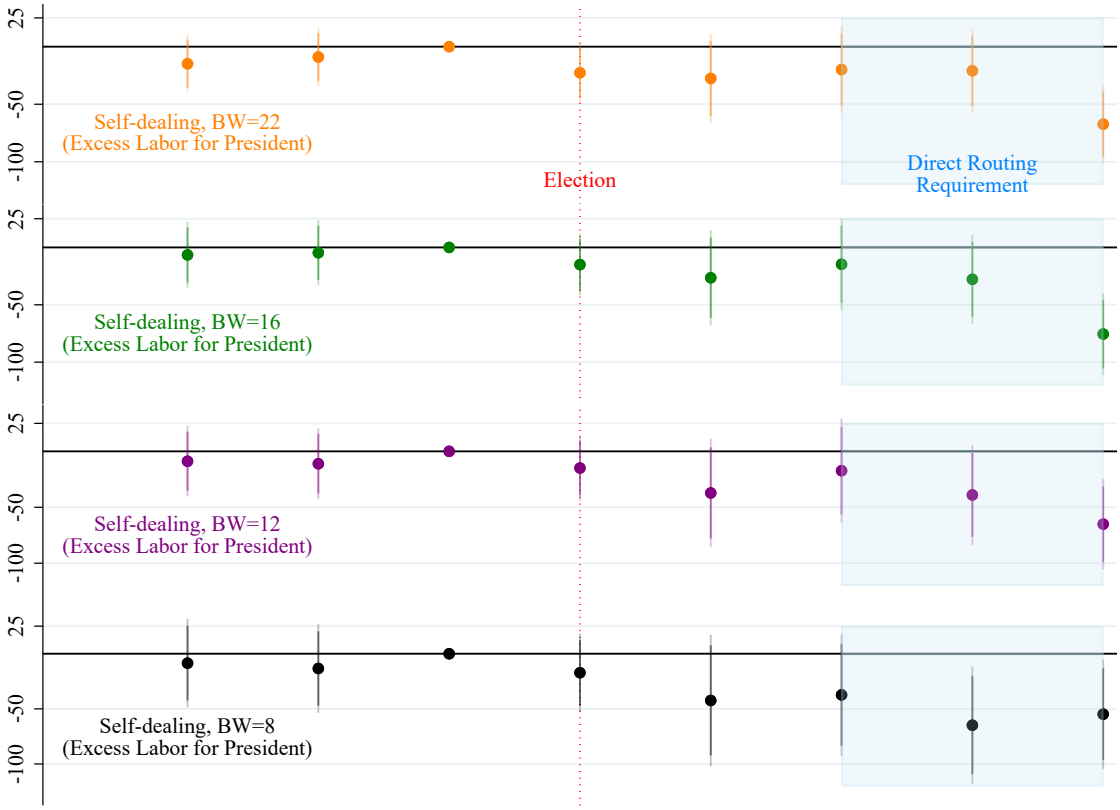
Panel B: Dummies for Facility within 10 Kilometers of Any Village in the Panchayat

	Change in NREGS Allocation, 2015 to 2018						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD Est.	9.089	3.956	14.683	10.242	14.029	11.308	19.929
	(9.661)	(11.356)	(15.105)	(11.423)	(15.825)	(14.845)	(24.037)
RD Est. X Near Cyber Cafe	-40.679***	-38.654***	-39.236***	-40.355***	-41.822***	-40.393***	-44.672***
	(15.337)	(14.817)	(14.753)	(14.951)	(15.355)	(14.744)	(16.493)
RD Est. X Dist. to town		0.065					0.143
		(0.217)					(0.271)
RD Est. X Dist. to HQ			-0.125				-0.171
			(0.168)				(0.210)
RD Est. X SC Frac.				-15.537			8.099
				(29.102)			(33.440)
RD Est. X Tot. Pop					-0.011		-0.023
					(0.024)		(0.032)
RD Est. X Primary Schools						-2.217	5.413
						(7.068)	(9.990)
Observations	621	627	627	627	627	627	621
Clusters	433	439	439	439	439	439	433
Block FEs	X						X

Note: This table is identical to Table 5 except that this table uses the optimal bandwidth for the change in the labor allocation from 2015 to 2018 (as opposed to the level of the 2015 allocation).
*p=0.10 **p=0.05 ***p=0.01

Figure A10

The Year-by-Year Difference-in-Discontinuity Results are Robust to the Choice of Bandwidth



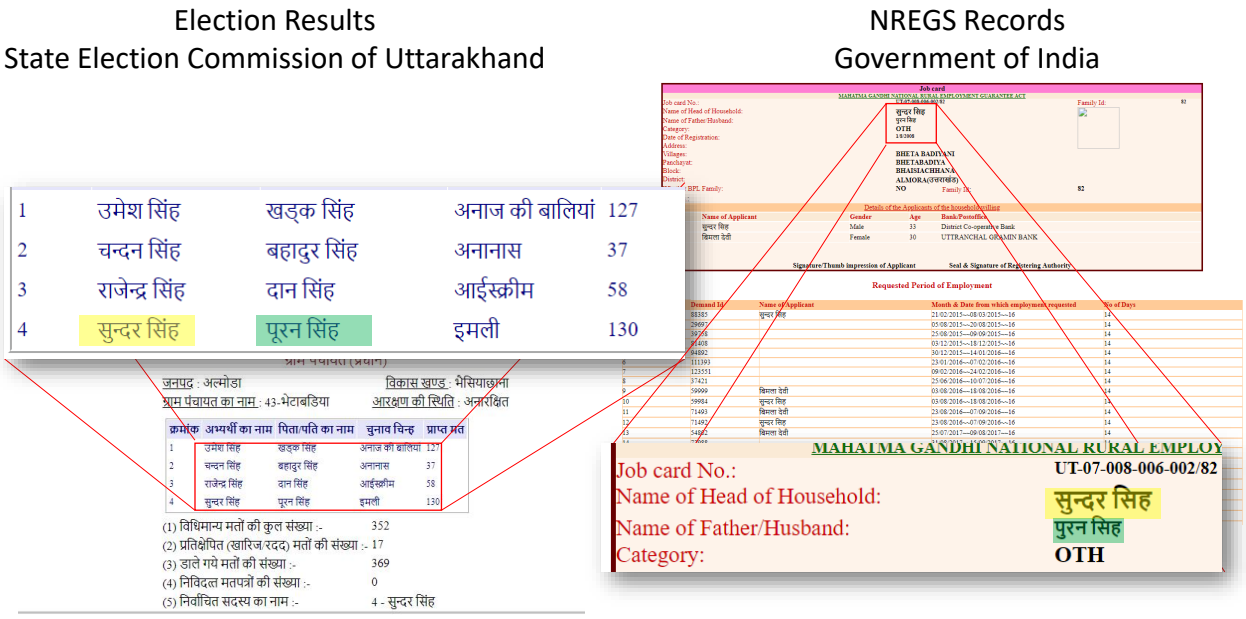
Note: Each regression retains observations such that the vote margin is less than or equal to "BW." All standard errors are clustered by panchayat.

B Data Appendix (For Online Publication)

B.1 Matching Candidates to NREGS Job Card Records

Our two key sources of data are the 2014 gram panchayat election results (scraped from the website of the State Election Commission of Uttarakhand) and the NREGS records for Uttarakhand (scraped from the central government's public NREGS portal). The election results record the name of each candidate and the name of their closest male relative (husband for married women, and father otherwise). The NREGS job card record contains the name of the head of household as well as the names of all other members of the household. The name of the closest male relative is directly recorded for the head of household. For all other members we infer that the head of household (if male) is their closest male relative.

Figure A11
Candidates Are Matched to Job Card Records Using
Own Name and Name of Closest Male Relative



As Figure A11 suggests, we restrict the set of potential matches to those that live within the same panchayat and match candidates to their job card records if and only if both pieces of data match. We restrict attention to the winner and runner-up in each race (discarding uncontested

races) and focus on the set of panchayats where the margin between them was less than 7.5 percent of the total votes cast. As nearly all of the records in both election and job card data are in Devanagari script, we hired native Hindi speakers to manually match records. For each panchayat we hired two people to independently identify matches and investigated any cases of disagreement. We considered a match to be valid only in cases where a candidate was matched to a unique household within the panchayat.

B.2 Matching Panchayats to Census Data

We draw information on panchayat facilities (cyber cafes, banks, etc.) from the 2011 Census Village Directory. To investigate job allocations by village characteristics, we match the 2011 Census and the job card data at the panchayat-village level. In most cases, one data source was in Hindi while the other data source was phonetically spelled in English. We hired two native Hindi speakers to independently match Devanagari script and the Latin alphabet. Some matchings of panchayats-villages did not agree between the two people, and we hired a third person to reconcile those unmatched results for further reconciliation.

Over 60 percent of panchayats contain only a single village. For the rest we aggregated village-level outcomes to the panchayat. Most aggregate to the panchayat naturally (e.g. populations are summed). When calculating distances, we assumed that the distance of a facility (cyber cafe or school) to the gram panchayat was the distance to its closest constituent part (in other words, the minimum among its villages).

B.3 Surveying Council Presidents

Out of 887 presidents (winning candidates) within the bandwidth of our regression discontinuity analysis, 89 percent of them (n=790) had phone numbers from the public website of the State Election Commission of Uttarakhand. Enumerators called all 790 presidents as many as five times unless the numbers were wrong or invalid (n=76) or presidents refused to participate in the survey (n=11). We hired a professional English-Hindi translator to create scripts for the enumerators. All calls were recorded to verify that the enumerator's survey entries were based on the actual interview.