Complaint Resolution Systems: Experimental Evidence from Rural India *

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Abstract

We study whether access to complaint resolution systems can resolve hold-up problems in the implementation of public good projects. We run a field experiment involving 1629 low-caste local representatives who were unable to start public goods projects in their constituencies due to bureaucratic hurdles. We randomize offers to file complaints regarding public good project initiation on their behalf and track its effects. Our treatment leads to a 40 percentage points jump in complaint filing rate and is effective in improving project implementation: treated constituencies see a 26% rise in public good projects. We also find that the treatment increases project initiation in neighboring constituencies by 23%. Our analysis suggests that the mere threat of a formal complaint technology could cause project initiation in neighboring wards. However, when multiple complaints are filed against the same higher bureaucrat, resolution rates go down. Surprisingly, treated representatives do not gain any electoral returns in the local elections that were held two years after the treatment.

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1 Introduction

Complaint resolution systems have become an integral part of states and private corporations.[1] Despite their ubiquity across the world, we know very little about their effectiveness. Complaint resolution systems, in theory, can improve organizational effectiveness by allowing for bottom-up accountability: complaints from lower strata (lower-tiered workers/officials or citizens) can deter higher officials from misusing power. Top leadership can also use complaints as a signal for malfeasance to reallocate monitoring efforts (McCubbins and Schwartz, 1984).

One potential problem with these institutions, however, is that they are prone to elite capture. First, elites use these systems more because they are more aware, have the technical human capital on how to complain, and have the resources to bear the cost of complaining and attending hearings. (Kruks-Wisner, 2021). Second, the quality of complaint resolution tends to be biased toward elites which can further alienate disadvantaged groups.[2] Thus, it's possible that these institutions, rather than promoting social justice, end up becoming tools for perpetuating group-based inequality.

As the effects of complaint resolution systems are theoretically ambiguous, it's important to empirically investigate whether they can be an effective bottom-up monitoring tool in practice. In particular, we need to test whether it can protect the interests of minority groups. Yet, estimating the causal effects of complaint resolution systems is challenging as finding a setting with a functional complaint resolution system and exogenous variation in

[1] More than 200 American cities have designed portals where citizens can log in and file complaints. The central government of India has an online platform—Centralised Public Grievance Redress and Monitoring System (CPGRAMS)—which allows one to file complaints against any department of the federal or state government. The literature documents these systems in other parts of the world too, for example, in China (Göbel and Li, 2021), Africa (Raffler, 2020), and Latin America (Trucco, 2017)

[2] Our analysis of the universe of complaints data from the state of Bihar shows that complaints from low socioeconomic caste groups are more likely to be reported as 'unresolved'. Furthermore, a primary survey of 200 households, shows that the priors of low-caste respondents about complaint resolution rate are 20% lower compared to high-caste respondents

its access or use is difficult.

We set up a field experiment in Bihar, India, where a formal complaint resolution system – Bihar Public Grievance Redressal Act (BPGRA) – was introduced in 2016. It gave every citizen and their local representatives a right to resolution of a wide range of complaints against the state in a time-bound manner. We identified a set of lower-caste local representatives who were unable to start public goods projects in their constituencies due to bureaucratic hurdles. We then randomly offered to file complaints on their behalf. Using this experimental variation in the likelihood of complaint filing, we demonstrate that complaint filing significantly improves the speed of project implementation with big positive spillovers on neighboring constituencies.

The administrative bodies responsible for implementing public goods projects comprise over 534 Blocks and 8400 Gram Panchayats (GPs – "village councils"). The GPs are further divided into wards (13.6 per GP, on average). Both Gram Panchayats (GPs) and wards are represented by directly elected politicians: GPs are headed by GP heads(Mukhiya) and wards are represented by ward members.[3] We refer to the block officials and GP heads as "upper-tiered" officials and ward members as "lower-tiered" officials.

We focus on a set of key water and sanitation (WAS) public goods that were created under a new government program. Under this program, every ward was supposed to receive funds to construct WAS projects within a three-year period (2017-20). This was a major decentralization policy change. For the first time, ward members were given the main responsibility for implementing public good projects in their constituencies.[4] Despite the massive decentralization push, upper-tiered politicians (GP heads) and bureaucrats (block officials) wielded significant power as the funds for these projects were routed through them. This

[3]These elections are non-partisan by law.

[4]The upper-tiered politicians were not happy with this decentralization move and challenged the government's policy in the high court. The high court eventually issued a judgment in favor of the devolution of funds to ward members for implementing WAS projects.

allowed upper-tiered state officials to delay the release of funds and create other hurdles, possibly to extract a bribe or due to prejudice against low-caste, low-tiered representatives.[5] Furthermore, given the history of discrimination against low-caste (SC) groups in Bihar, the state government issued an order stating that wards run by low-caste (SC) members should be the first ones on the priority list for funds allocation. However, two years after this program started, 30% of the low-caste wards had not been able to start projects in their constituencies. We specifically collaborated with these low-caste, ward leaders for our experiment.

This paper examines three main research questions. First, can filing complaints against upper-tiered state officials improve implementation of public goods projects by minority leaders? Second, what are the spillover effects of formal complaints technology on complaint filing and project implementation in neighboring constituencies? Third, what are the net electoral returns from complaint filing in the longer run? More specifically, does the act of filing complaints improve project implementation and hence reelection chances? Or does it invite backlash from superiors who use their political power to punish the politicians for complaining?

To answer these questions, we recruited 1,629 low-caste ward leaders who reported facing difficulties in implementing public goods projects in their constituencies and randomly assigned them to either a control group or a complaint-filing assistance group. In the complaint filing assistance group, we provided both information regarding the formal complaints technology and offered to file complaints regarding WAS project initiation on their behalf. In

[5]There could be legitimate reasons for not releasing funds. For instance, it is possible that the quality of the proposals submitted by ward members is low or the budgets are inflated. However, upper-tiered officials are supposed to provide feedback and seek a revised proposal to ensure that the projects are completed on time. The demand for bribery came up frequently during our qualitative interviews with the ward members. We do not have hard evidence on this as ward members were not willing to report it during formal surveys. We also interviewed senior bureaucrats of the Government of Bihar who confirmed that they had allocated sufficient funds to the blocks under this program and could not think of any legitimate reasons for such inordinate delays.

order to understand barriers to greater adoption of the new formal complaints mechanism, we also conducted a smaller experiment with 271 low-caste representatives, 50% of whom we treat with information only, but do not offer to file complaints. Using the experimental variation in the likelihood of complaint filing, we track its short-run (3 months) and long-run (3 years) effects.

Our findings are as follows. First, in the short-run, as a first-stage, we find that our treatment—complaint filing assistance—results in a big jump in the actual complaints filed as per the administrative data: a 40 percentage points (p.p.) increase compared to pure control. In contrast, the information-only treatment causes a much smaller increase (7 p.p.). This suggests that the technical human capital needed for complaint filing and other transaction costs are bigger barriers to the adoption of the new formal complaints technology than information.

We also find that the formal complaints technology significantly improves the speed of WAS public good projects implementation. Our endline survey shows an additional 6.9 p.p (26%) increase in WAS projects being undertaken in treated wards. Treated representatives are also more likely to report that the main problem preventing projects from being undertaken has been resolved.

Furthermore, we find that the treatment has positive spillovers on complaint filing and project initiation in neighboring jurisdictions. Our endline survey of 945 neighboring wards where projects had not been undertaken indicates a 7 p.p. (23%) increase in project initiation for neighbors of treated wards when compared to neighbors of control wards. Only 2.5 p.p of these representatives actually file complaints. The discrepancy between complaints filed and the project initiation in neighboring wards suggests that the mere threat of a formal complaints technology could cause project initiation. Heterogeneity analysis shows that the spillover effects on project initiation are mainly driven by neighboring wards that (like the experimental sample) are low-caste wards. They are 49 p.p. more likely to report project

initiation, as opposed to only 1.7 p.p. increase for higher-caste wards.

While the complaint filing seems to reduce the hold-up in implementing public good projects in the short-run, its appeal to ward leaders may depend on the net electoral returns over the long-run. We find that treated representatives do not gain any electoral returns in the local elections that were held 2 years after the treatment. If anything, we find evidence that our treatment had negative electoral consequences: treated representatives are 3.9 p.p. (13%, p=0.17) less likely (statistically not significant) to be reelected in their next elections. This reduction in reelection probability seems to be partly driven by the fact that treated wards are 4.8 p.p.(6%, p=0.09) less likely to run compared to the control wards.[6]This reduction in reelection chances is possibly due to backlash from superior politicians in response to their complaints.

Our paper makes several contributions to the literature. This is the first paper to provide experimental evidence on the effectiveness of intra-government complaint resolution systems in improving the functioning of the state. The only other experimental work around this institution is Trucco (2017), which shows that exogenous improvements in state responsiveness to citizens' complaints result in greater citizens' participation. Her findings complement ours and suggest a possibility of a virtuous cycle: exogenous increases in complaint filing results in a more responsive state which in turn can lead to greater citizens' participation.

Our paper also adds to a new strand of empirical work on state effectiveness. A vast majority of literature on state effectiveness has focused on testing new mechanisms to select and monitor front-line workers that improves their performance (Dal Bó et al. (2013), Duflo et al. (2012), Khan et al. (2019)). But mid-level officials play an equally important role in the delivery of public goods and services and misaligned incentives across tiers of government

[6]This could also be due to the fact that treated low-tiered representatives start running for higher posts. We formally test this hypothesis using nominations data and find very few cases of ward members running for higher posts and cannot reject the null that the likelihood of running for higher posts is the same. These results are presented in Table A4

can be a major source of inefficacy (de la Sierra et al., 2022). This paper demonstrates that empowering lower-tiered officials with tools to hold mid-level managers accountable can improve organizational efficiency.

Our paper is also related to a nascent empirical literature on information flow and monitoring in multi-tiered organizations (Dodge et al. (2018), Dal Bó et al. (2018), Callen et al. (2018),Banerjee et al. (2020)). This literature has looked at information flows within government and evaluate various mechanisms through which higher bureaucrats can use information to fix incentives of lower bureaucrats. We show that complaints from local elected officials can be used as a signal to monitor the functioning of other elected and non-elected state officials.

This paper also contributes to the literature on effectiveness of minority leaders. While one strand of this literature has looked at how the "selection" of minority leaders affects outcomes (Pande (2003)) the other one has focused on identifying factors that could undermine the performance of minority leaders in organizations: lack of cooperation from subordinates(Ayalew et al. (2021)), discrimination from colleagues/co-workers (Gagliarducci and Paserman (2012)) or due to discrimination from top (Casas-Arce and Saiz (2015)). Little work exists on what institutional mechanisms can be put in place to make minority leaders' work more effective. Our paper takes this literature forward by showing that access to institutions such as complaints resolution systems can improve the bargaining power of minority leaders and help them perform better.

Finally, our paper shows that exercising a voice in the form of complaint filing may invite backlash from superiors. The fact that registering a formal complaint or 'whistle-blowing' can make the complainants targets of retribution is an important feature of many theoretical models (Chassang and Miquel (2019), Heyes and Kapur (2009), Bac (2009)). However, there is very little empirical evidence on the extent of actual retribution and the forms it can take in practice. Results from our paper provide some empirical evidence on the unintended

consequences of reporting malfeasance.

2 Background and Context

2.1 Local Administrative Structure

Bihar's over 100 million strong rural population live in villages that are grouped into administrative units called Gram Panchayats (GP). There are over 8400 GPs in Bihar. Each GP is headed by an elected representative called the "Mukhiya". In this paper, we will refer to the Mukhiya as the upper-tiered representative. Each GP is divided into wards. Each ward is headed by a ward member. We will refer to the ward member as the lower-tiered representative. There are over 114,000 wards in Bihar. The elections for both the upper-tiered and the lower-tiered representative posts were held simultaneously in May 2016.

An upper-tiered politician represents, on average, a population of 13300 persons; on the other hand, the lower-tiered representative is elected from a population of approximately 1000[7]. Local bodies are responsible for, among other things, the implementation of a wide array of development projects and representing their constituents' issues at higher levels. Within a GP, nearly all of this has been traditionally done by the upper-tiered representative (Rider et al. (2011), Gupta (2002)).

While the GPs are elected bodies and have considerable decision-making authority, they rely heavily on upper-tiered state bureaucracy for funding and support. The bureaucrats at block level, in particular, directly monitor, supervise and support implementation of public programs by GPs. There are 534 blocks in Bihar and each block oversees program implementation by 16 GPs on average. Figure 1 depicts the state administration structure

[7]These are back-of-the-envelope extrapolations. The last estimates of GP populations are from 2010:10953 persons per GP. Since there exist 13.5 wards per GP. the average ward population for 2010 can be esitmated to be 806 persons. The figures of 13,300 and 1000 are arrived at by assuming population growth for the decade to be 22%

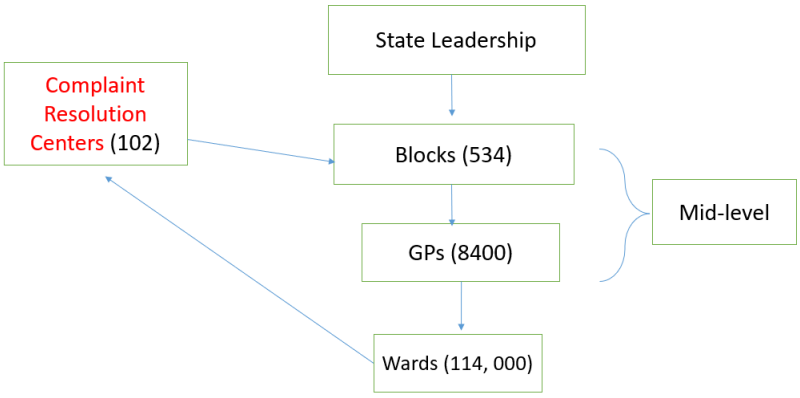

Figure 1: State Administration Structure
This figure displays how different layers of the state are connected to each other. Here state leadership consists of state officials at the top: state capital and district. The next layer is a set of bureaucrats at block level who oversees implementation of projects on the ground. The blocks are further divided into GPs. GPs are elected bodies headed by a GP council head (Mukhiya). Each GP is further divided into multiple smaller wards that are represented by Ward Members. Complaint resolution Centers are independent bodies set up by the state that can call block officials for hearing conditional on receiving complaints from below

and shows where Blocks, GPs, Wards are placed in the overall structure of the state.

2.2 Implementation of Water and Sanitation (WAS) Schemes

In late 2016, the state government of Bihar devolved implementation of two major water and sanitation schemes to the lower-tiered representative. The two schemes, called "Nal Jal" [piped water for every household] and "Nali Gali" [construction of village roads and drains] formed key planks of the incumbent government's "seven-resolves"[8] to development. An estimated sum of 4 billion dollars have been allocated to the implementation of these schemes. Over 93% of lower-tiered representatives surveyed report that these two schemes prove extremely beneficial to households in their jurisdictions.

The decision to transfer implementation powers to the lower-tiered representatives con-

[8]The seven resolves - "7-Nishchay" - include: skill development programs for youth, reservation for women in government jobs, electricity in every house, piped water to households, local drains, construction of toilets and improving higher education

stituted an important decentralization move. In one stroke, the implementing authority was brought significantly closer to citizens, by a factor of 13.5. For the first time in Bihar's history, lower-tiered representatives had a direct say in the spending of any state funds. Each lower-tiered representative was responsible for spending an average sum of $20,000 over three years.

Wards were selected for WAS asset construction as per rules set up by officials at the higher state (non-local). The state government issued an order that stated that every year, the list of wards where projects needed to be implemented be drawn up by the upper-tiered bureaucrats(block officials). Often, in practice, this was done together with the GP heads. Money for WAS schemes was transferred from the state to the GP account. The GP head then transferred the amount to the ward account. The ward leaders then identified where the asset had to be created, find a suitable contractor, and liaised with the relevant department to organize the construction and monitor the implementation of WAS assets.

The block officials played one additional role in WAS projects' implementation: they reviewed and approved the financial estimates at the initial stage and again at the completion of the project. The funds could not be released to the wards without their approval[9]. Thus, we can see that the block officials and GP heads continued to hold significant power in the implementation of WAS projects by the ward leaders. The main way in which the upper-tiered bureaucrats and representatives interfered with WAS projects was by withholding funds for implementation (funding).

Our baseline survey of ward members provides empirical evidence on the reasons for not being able to start WAS projects. A large majority of ward members (55%) report that upper-tiered politicians and bureaucrats are not releasing funds.[10] Another 23% ward members

[9]We should clarify that for a third of the wards, the piped water scheme is being implemented by the Public Health Engineering Department (PHED). This is because these wards are seen to have problems with ground-water quality. There was, however, some confusion over PHED's role for much of 2017-18 and some parts of 2018-19

[10]We can further decompose this figure: 34% ward members report that it's the upper-tiered politician

mention a variety of procedural reasons such as ward not on the list of selected wards for a given financial year, can't implement multiple WAS projects simultaneously, implementation to be done directly by public health department. While some of the procedural reasons could be valid but a part of this might be deliberate hurdles by upper-tiered officials.

2.2.1 Complaints Resolution Mechanism Prior to Our Intervention

It is important to understand how the lower-tiered leaders tried to resolve these problems prior to our intervention. Our baseline data provides useful insights. 25% of our sample reported they had not done anything about their problems. the Rest of them (75%), however, tried reaching out to higher state officials to air their grievances. On investigating whom they approached with their complaints, we discovered that they approached mostly upper-tiered politicians and bureaucrats. 49% ward members approached GP heads (Mukhiyas) and another 44% contacted the block level officials (Block Development Officers). Only 6% of the sample try reaching out to state officials based at district headquarters or the state capital. We can see that most of the lower-tiered officials were not able to reach out to senior politicians (above GP level) or bureaucrats (above block level) or any other independent authorities set up by the state.

2.3 Caste Divisions

For over two millennia, much of Indian society has been divided along caste lines. Caste is defined at birth and is usually based on the caste of the father. A defining feature of caste is the presence of strict hierarchies: the castes at the very top of the ladder have historically enjoyed (and indeed, continue to do so) great privileges in society, while those at the bottom are discriminated against, both socially and economically. Much of the laws that defined the nature of caste-based society for the Indian subcontinent were laid down in the Manusmriti

(GP head) and 21% blame the BDOs (Block Development Officer)

(or the "Laws of Manu") - a text written around the dawn of the common era. The laws prescribed forbade lower castes and upper-castes from mixing in society.

Those individuals belonging to sub-castes that fell outside the caste system altogether were the untouchables, which are now grouped into a heterogeneous whole referred to as the Scheduled Castes. A term that is increasingly commonly used for this grouping is "Dalits" (literally - "the oppressed"). Historically these groups could not own land, conduct trade or business, receive education, or buy or sell in markets. Though the Indian state abolished untouchability in 1950, SCs lag severely on several socioeconomic indicators even today (Banerjee and Somanathan (2007) Deshpande (2011)).

3 Experimental Design

3.1 Sample Selection

Our sampling frame comprises all wards that, according to official government data in May 2019 (1) had not implemented at least one of the two WAS projects,(2) had a representative who belonged to a scheduled caste, and (3) who could be contacted and agreed to participate. [11] We could not reach 15% of the sample over the phone. The main reason for our inability to get through to more representatives was because phone numbers were switched off or not reachable. Table 1 compares the population with our sample on observables. While wards in the experimental sample are broadly comparable to all wards, contacted lower-tiered SC representatives are likelier to be from somewhat wealthier GPs, and are marginally closer to the district headquarters.

[11] On piloting, we discovered that the official data reports WAS construction with a lag. Hence, we have a series of screening questions to screen out wards where WAS projects have been completed.

Table 1: Comparison of Sample Wards to All Wards

Variable	(1) Population	(2) Sample	(3) Difference
Mean SC Wealth Score (in GP)	-0.424	-0.362	0.062
	(0.611)	(0.597)	(0.000)
Mean non-SC Wealth Score (in GP)	0.327	0.384	0.057
	(0.764)	(0.757)	(0.005)
Proportion of SCs (Census 2011)	0.202	0.198	-0.004
	(0.092)	(0.086)	(0.120)
Distance to District Headquarters (Census 2011)	32.954	31.751	-1.203
	(18.501)	(17.656)	(0.012)
Total GP Area (Census 2011)	1,174.446	1,113.245	-61.201
	(882.700)	(733.147)	(0.004)
Total Population of GP (Census 2011)	11,843.700	11,872.551	28.851
	(4,491.801)	(4,710.335)	(0.814)
Percentage of SCs in Main SC Village (Census 2011)	0.557	0.548	-0.008
	(0.257)	(0.253)	(0.220)
Percentage of all SCs in Main SC Village	0.328	0.325	-0.003
	(0.205)	(0.204)	(0.612)
GP Head Reserved for OBC in 2016	0.169	0.169	0.000
	(0.375)	(0.375)	(0.986)
Margin of Victory of Upper-Tiered Representative (Votes)	167.262	173.472	6.210
	(167.416)	(175.299)	(0.178)
Lower-Tiered Representative's Age	39.857	38.886	-0.971
	(11.884)	(10.987)	(0.001)
Lower-Tiered Representative's Gender	0.445	0.467	0.022
	(0.497)	(0.499)	(0.098)
Lower-Tiered Leader Has Five Years of Education	0.262	0.356	0.094
	(0.440)	(0.479)	(0.000)
Observations	3,070	2,628	5,698

Tables present category-wise averages and t-tests of difference in means. Standard errors are reported in parentheses except for column 3, where p values are reported in parentheses. We also conduct F-test for the null that the coefficients for all covariates are jointly zero: F(13, 5345) = 6.31 and Prob F= 0.00

3.2 Intervention

3.2.1 Formal Complaints System

In 2016, the government of Bihar successfully passed the Bihar Right to Public Grievance Redressal Act (BPGRA) that gave every citizen the right to "redressal" (resolution) of any "grievance" (complaint) filed across 44 different departments of the state. Crucially, the Act

mandated the creation of 102 Public Grievance Redressal Officers (PGRO) which were setup at sub-district level. A sub-district is above block level but below the district administration, as depicted in Figure 1. Each district, on average, had about 2.5 officers who were tasked with the duty of hearing and resolving citizens' grievances. In these hearings, the complainant presented their grievance in the presence of the concerned departmental bureaucrat. The officer's job was to determine the validity of the grievance and, once determined as permissible to be acted upon under the law, ensure the grievance is disposed off within 60 days.

Filing and following up on complaints is not costless. Over three-quarters of complaints are filed in person at the PGRO's office. Subsequently, the process of redressal involves making multiple trips to the PGRO's office to attend hearings. There is one PGRO for every 5.23 Blocks, 84.6 GPs and 1120 wards. Thus, the average complainant has to travel a considerable distance (12 km on average) to ensure their cases are heard. Our survey evidence suggests that travel and food alone cost INR 140 per hearing. There are, on average, 2.5 hearings per complaint. In addition to this, there are opportunity costs of attending hearings. Complainants we spoke to say that attending hearings takes up a whole day.

In the first three years of its functioning, over 500,000 grievances were filed. PGROs are empowered to punish errant departmental bureaucrats with fines upto INR 5000 ($70). A study conducted by the IDFC Foundation in collaboration with the government of Bihar finds that, on average, a third of the grievances are redressed. The government's own estimates are, however, close to 90%. In either case, complainants report high satisfaction rates, at nearly 75%.

3.2.2 Experimental Arms

All treatments are administered over the phone in our setting. The experiment included three experimental arms:

1. **Complaint Filing Arm**: In this treatment arm, we called randomly-sampled lower-

tiered SC representatives where, per official records, no WAS project had been undertaken and provided them information about the formal complaints technology and offered to file grievances on the representatives' behalf. We filed complaints for those who took up our offer. After a complaint was successfully filed, we sent a follow-up reminder call to the representative on the day of the first hearing of the complaint. Our main objective here is to measure the impact of complaint filing on WAS public good provision in the short-run and electoral returns in the long-run.

2. **Information Only Arm**: We called randomly sampled lower-tiered SC politicians and only provided information. The key difference from the complaint filing assistance treatment arm is that we did not offer to file complaints. Our main objective here is to see if information alone suffices to increase the number of complaints filed.

3. **Control Arm**: Control group members were also provided information about key government schemes introduced by the government. But these schemes were unrelated to water and sanitation.

3.3 Randomization

Once we ascertained that at least one of the two WAS projects had not been undertaken—based on the ward representatives' testimony during the call—then they were randomly assigned to one of the experimental groups described earlier. Randomization occurred in real-time on the survey app the enumerators used. Initially, we ran the intervention such that two arms, complaint filing assistance treatment, and control occurred with equal probability. Subsequently, the third arm of the experiment—the information treatment—was added and all three arms were to occur with equal probability. In the end, we had 727 filing assistance treatment wards and 130 information treatment wards. The unit of randomization was at the ward level without any stratification.

We use the baseline survey data to check if the randomization achieved balance. Table 2 presents the results of balance checks for our main treatment –complaint filing assistance.[12] It shows that the treatment and control groups are balanced across most covariates.

Table 2: Test of Randomization Balance for Complaint Filing Assistance Treatment

Variable	(1) Control	(2) Treatment	(3) Difference
Mean SC Wealth Score (in GP)	-0.386	-0.362	0.024
	(0.588)	(0.615)	(0.032)
Mean non-SC Wealth Score (in GP)	0.359	0.352	-0.007
	(0.780)	(0.756)	(0.040)
Proportion of SCs (Census 2011)	0.205	0.199	-0.006
	(0.096)	(0.088)	(0.005)
Distance to District Headquarters (Census 2011)	31.847	31.647	-0.200
	(17.993)	(18.332)	(0.945)
Total GP Area (Census 2011)	1,176.579	1,116.427	-60.152
	(943.318)	(663.041)	(42.138)
Total Population of GP (Census 2011)	11,971.220	11,776.543	-194.676
	(4,991.410)	(4,199.561)	(238.833)
Percentage of SCs in Main SC Village (Census 2011)	0.553	0.555	0.002
	(0.252)	(0.254)	(0.013)
Percentage of all SCs in Main SC Village	0.323	0.323	0.001
	(0.197)	(0.197)	(0.010)
Margin of Victory of Upper-Tiered Representative (Votes)	168.831	171.073	2.242
	(167.245)	(172.665)	(8.906)
Lower-Tiered Representative's Age	39.185	38.685	-0.501
	(11.175)	(10.836)	(0.572)
Lower-Tiered Representative's Gender	0.452	0.466	0.014
	(0.498)	(0.499)	(0.026)
Lower Tiered Representative is Illiterate	0.112	0.101	-0.011
	(0.316)	(0.301)	(0.016)
Lower Tiered Representative is Literate	0.542	0.543	0.001
	(0.499)	(0.499)	(0.026)
Observations	760	727	1,487

Table presents category-wise averages and t-tests of difference in means. Standard errors are reported in parentheses except for column 3, where p values are reported in parentheses. We also conduct F-test for the null that the coefficients for all covariates are jointly zero: F(13, 948) = 0.49 and Prob F= 0.9289

[12]balance checks for the information only treatment arm is shown in Appendix Table A2

4 Data Sources

This project brings together multiple data sources, both primary and secondary in nature. All our secondary data sources, except for data from two rounds of the decennial census of India, are obtained from different administrative departments of the Government of Bihar. Our primary data sources are obtained via surveys of various local actors in the administrative machinery.

4.1 Secondary Data Sources

4.1.1 BPGRA Grievances Data

We have official government data on the universe of over 500,000 complaints filed under the BPGRA between June 2016 and August 2019. Our data contains personal information including the name and address of complainants. Furthermore, we have phone numbers for 82% of complainants. We also have data detailing complaints including the date filed, the exact text of the complaint, the number of hearings held, the date of redressal and whether appeals were filed.

4.1.2 WAS Scheme Data

This includes official government data regarding every single WAS asset constructed across Bihar's 114000 wards. This dataset is the source of our WAS-related outcome variables such as the status of WAS project construction in a given ward. We use this information to arrive at the sampling frame.

4.1.3 Local Representatives Data

We have official government data on both upperand lower-tiered representatives for 94% of the upper-tiered representatives and 81% of the lower-tiered representatives. We also have

data on individuals who contested these elections at both tiers. In all, we have a dataset of over 350,000 local politicians. For each of these, we have personal characteristics including the name, age, education, gender, caste category of these representatives. We also have data on reservation status of various posts and electoral data of on the number of votes won in the 2016 and 2021 elections.

4.2 Primary Data

All our primary data is collected via phone-based interviews of representatives and mainly of three types:

4.2.1 Baseline

We collected data on the status of WAS project, self-reported impediments to effective functioning of the lower-tiered representative and knowledge about the formal complaints technology.

4.2.2 Endline

Three months after the treatment, we carried out the endline survey. We collected information on whether the problem reported at the time of baseline was resolved, if they were able to start implementing WAS projects, and whether they were approached by any officials to discuss WAS project implementation.

4.2.3 Spillover Survey

In the endline survey, we also surveyed one randomly sampled neighboring lower-tiered representative in the GP in whose wards projects were not yet undertaken. We conducted the spillover survey only for GPs with only one experimental ward.

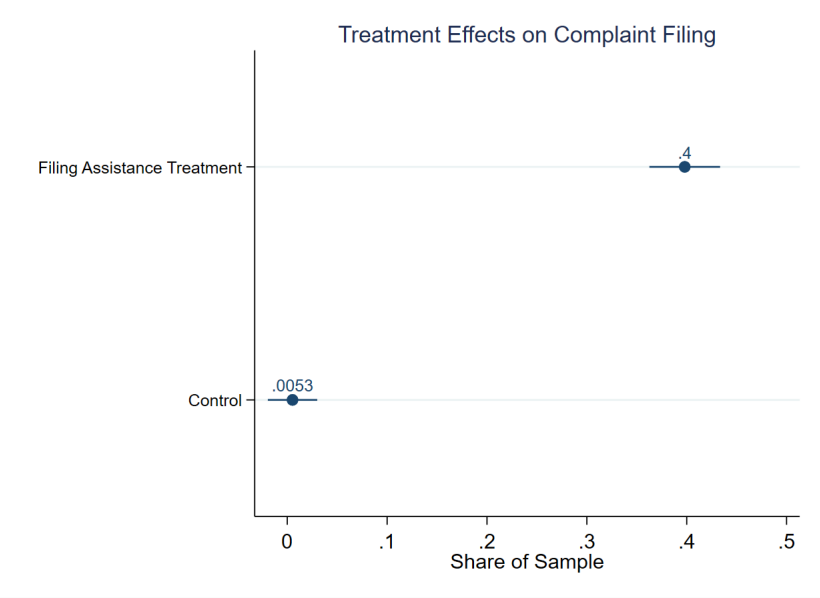

Figure 2: Impact on Complaint Filing Rates
This figure displays the impact of our main intervention arm – a complaint filing assistance treatment –on whether complaints are filed. This is the "first stage" of the experiment). Outcomes are measured as per administrative data on complaints.

5 Estimation Strategy and Results

5.1 Short-Run (3-Month)

5.1.1 Impact of Complaint Filing Assistance Treatment

We begin by estimating the causal effects of complaint filing assistance treatment on complaint filing and project implementation. The ITT effects of the treatment can be estimated using the following

$$Y_{ig} = \beta_0 + \beta_1 * T_{ig} + X_g'\gamma + S_b + \eta_{ig}$$

here, Y_{ig} could include whether a project was initiated (as per official data or endline survey) and whether a complaint was filed in ward i of GP g. X_g is a vector of controls at the GP and ward-level.S indicates block fixed effects. T_{ig} is a dummy that takes the value

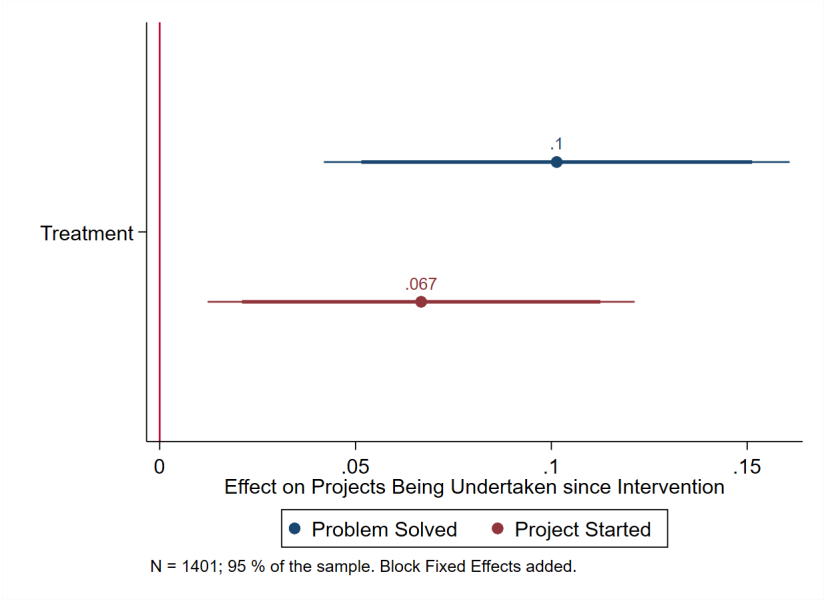

Figure 3: Impact of Complaint Filing on Project Implementation
This figure shows the impact of the filing assistance treatment on our two main outcome variables: (a) whether the problem preventing projects from starting was resolved and (b) whether projects were actually initiated. Outcomes are measured via the endline survey.

of 1 if the lower-tiered representative i is treated with complaint filing assistance treatment.

We pre-registered this specification along with the experimental design and primary outcome variables we focus on in this experiment.[13]

We first start by looking at the impact of our treatment on levels of complaint filing by lower-tiered leaders. Our complaints filing assistance treatment significantly improved the likelihood of lower-tiered representatives filing complaints. The difference in complaint filing between treated and control representatives is 41 percentage points (Figure 2) as per administrative data. Thus, our treatment results in a strong first stage which should allow us to detect effects on downstream outcomes if they exist.

We now turn to impacts on projects being undertaken. We focus on two outcome variables from our 3-month endline survey[14] : (i) whether the problem preventing projects from

[13]The unique identifying number for the AEA registry is: AEARCTR-0004308. Here is the link for our pre-registration: https://www.socialscienceregistry.org/trials/4308

[14]Outcomes were pre-registered

starting had been resolved and (ii) whether projects had, consequently, started.

Figure 3 plots treatment effects for our main estimating equation.[15] The complaints filing assistance treatment had strong positive effects on the overall project implementation. First, treated lower-tiered leaders are more likely to report that the main impediment to project starting was resolved. 51 per cent of treated leaders report 'problem resolved' compared to control mean of of 41 p.p.: this amounts to a 24% increase in respondents reporting that the main impediment to project starting was solved (Table 3, col (1)). Second, we find that our treatment improves project initiation by 7 p.p over a control mean of 27 p.p (Table 3, col (2)). This translated to a 26% increase in the likelihood of project initiation.

The direct effects of complaint filing assistance treatment on project initiation seem robust to changing the level of fixed effects and clustering errors at different levels as shown in Table A5 (cols (2)-(4)). As the unit of randomization is at ward level, we cannot rule out potential within-GP spillovers affecting our estimates. However, we can test for the extent and direction of bias due to spillover concerns by restricting our sample to those GPs that have only one treated or control wards. We do not find much evidence for spillovers across wards biasing our results as shown in Table A5.

Since the treatments follow 'encouragement design' approach, we also look at the ToT effects using the following specification:

$$C_{ig} = \beta_0 + \beta_1 * T_{ig} + X_g'\gamma + S_b + \eta_{ig}$$

$$Y_{ig} = \alpha_0 + \alpha_1 * C_{ig} + X_g'\gamma + S_b + \epsilon_{ig}$$

Here, we assume that the impacts on project initiation come only from the individuals that filed complaints. As we can see in Table 4, the ToT effects are much bigger: our

[15] This specification - with block fixed effects - is our pre-registered specification

Table 3: ITT Effects of Complaint Filing on WAS Projects

	(1)	(2)
	Problem Solved	**Project Initiated**
Filing Assistance Treatment	0.105***	0.069**
	(0.030)	(0.028)
Observations	1332	1332
Control Mean	.41	.27
Fixed Effects	Block	Block
Pre-specified	YES	YES

Table delineates the ITT impact of the complaint filing assistance treatment on our two main outcome variables.In column 1, we measure whether the problem preventing the ward members from initiating projects was resolved. In column 2, we test whether a project was initiated in the post intervention period. The regression specification across both columns is our pre-specified estimating equation. All regressions contain GP-level controls. Standard errors are reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

treatment results in a 17 p.p. (65%) increase in the likelihood of initiating projects. However, the exclusion restriction may not hold in this context. For instance, it is possible that the threat of filing a complaint from non-compliers group was enough to ensure projects were initiated.

Table 4: TOT Effects of Complaint Filing on WAS Projects

	(1)	(2)
	Problem Solved	**Project Initiated**
Complaint Filed	0.266***	0.177***
	(0.071)	(0.065)
Observations	1330	1330
Control Mean	.41	.27
Fixed Effects	Block	Block
Pre-specified	YES	YES

Table delineates the TOT impact of the complaint filing assistance treatment on our two main outcome variables. We have used complaint filing assistance treatment as instrument for whether complaint was actually filed and the table reports the results of the second stage. 'Problem solved' measures whether the problem preventing the SC ward members from initiating projects was resolved. 'Project initiated' measures whether the project was, subsequently, initiated. All regressions contain block fixed effects and GP-level controls. Standard errors are reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

5.2 Spillover Effects

The effects of our complaint filing treatment can spill on to neighboring wards through various channels. First, a 'social interaction' channel: treated ward members can help other ward members in their network with complaint filing. If true, we can expect positive spillover effects on complaint filing. This increase in complaint filing, in turn, could impact project implementation. Second, spillover effects can arise through an 'administrative response' channel. As implementation of WAS projects are jointly monitored by GP heads and block officials, having a treated ward in a GP (Block) could impact project implementation outcomes of other wards because the GP head (Block officials) change their response not only to the treated ward but also to other wards that fall under their jurisdiction. We could find negative spillover due to multitasking concerns: upper-tiered officials start paying greater attention to the treated wards at the cost of others wards. The spillover effects are likely to be positive if upper-tiered officials start supporting project implementation in other wards in order to avoid future complaints (deterrence effects). Positive spillovers can also arise if upper-tiered officials face a fixed cost of approving projects in bulk: they might want to approve all stuck projects in their jurisdiction together. Thus, the spillover effects on project implementation are theoretically ambiguous and depend on which channel dominates in practice.

We test for within-GP and within-Block spillovers separately as our empirical strategy for measuring spillovers is different for each group. At the block level, the main source of variation we exploit is the intensity of the treatment level: the number of treated wards in a block. GPs are smaller administrative units, so we do not have much variation in treatment intensity at the GP level. Most GPs in our sample have either only one treated or a control ward, so we measure spillovers by comparing untreated wards of GPs with one treated ward with those that have one control ward.

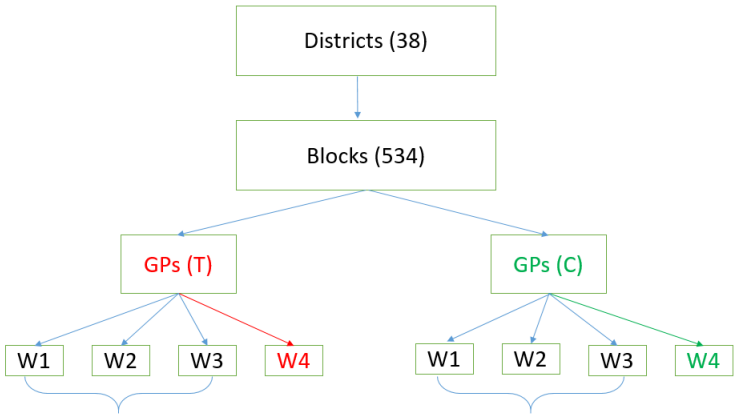

Figure 4: Within-GP SPillovers Estimation
This figure depicts our within-GP spillover estimation strategy. We restrict our analysis to GPs with either one treated or control ward. We compare untreated wards of GPs with one treated ward (marked in red) with GPs that have one control ward (in green).

5.2.1 Within-GP Spillovers

To test for spillovers, we restrict our attention to GPs that have only one experimental ward. This excludes 25% of GPs from our sample. We then test the impact of having either one treated or one control ward in the GP on outcomes in non-experimental wards from that GP. Despite dropping 25% of the observations for spillover analysis, our new sample remains largely balanced (see Table A1).

To measure within-GP spillovers, We estimate the following:

$$N_{ig} = \beta_0 + \beta_1 * T_{ig} + X_g'\gamma + S_b + \eta_{ig}$$

Here, N_{ig} could include (a) WAS projects have been undertaken or (b) complaints are being filed by representatives. X_g is a vector of controls at the GP level. S_b indicates block fixed effects. T_{ig} is a dummy which takes the value of 1 if the ward i in a given GP g has a treated ward member.

We start with looking at the spillover effects of our treatment on a set of complaint filing

outcomes. Effects on complaint filing can spillover across many dimensions. First, neighboring ward members in a given GP might file complaints regarding non-implementation of WAS projects. This is because ward members within a GP know each other as they are part of the GP council. Second, neighboring ward members can file complaints regarding non-provision of other public goods or any other benefits they are entitled to receive from the state. Third, citizens in treated wards can also learn about it from their representatives and start filing complaints. Data limitations do not allow us to test for all three types of spillover effects but we are able to test the first two.

Table 5 presents the results of spillover effects on complaint filing. Using the administrative data on the universe of complaints filed during the post-intervention period, we look at three outcome variables. First, whether neighboring ward members file complaints regarding WAS projects(column 1). Second, the likelihood of complaint filing by neighboring ward members about any public goods in their constituencies including WAS projects (column 2). Third, whether neighboring ward members file a complaint about any private dispute or benefits/services they are not receiving from the state. We can see that spillover effects are positive but relatively small in magnitude: 0.2 p.p. increase compared to control where no one complains. The effects are also positive for private complaints but much smaller in magnitude.

Spillover Effects on Project Initiation

Unlike complaint filing, we do not have access to real-time admin data on status of WAS projects. Thus, in order to test for spillover effects on project implementation, we surveyed one randomly sampled neighboring ward representative in whose wards projects had not yet been undertaken. We were able to contact one such representative in over 96% of these GPs.

Table 6 presents the results using data from this spillover survey. Neighboring wards report more projects being undertaken in the post-intervention period. In particular, wards neighboring treated wards are 7.6 p.p (25%) more likely to report that any project had been

Table 5: Spillover Effects on Complaint Filing

	Complaints Filed		
	(1)	(2)	(3)
	WAS	**Public Goods**	**Private**
Treated Neighbor	0.002**	0.002*	0.001***
	(0.001)	(0.001)	(0.000)
Observations	10744	10744	10744
Control Mean	0	0	0
Fixed Effects	Block	Block	Block
Pre-specified	YES	YES	YES

Table delineates the impact of the complaint filing assistance treatment on our two main spillover outcomes.Outcomes are collected via an endline survey of one randomly selected representative from a ward neighboring a representative who was part of the experimental sample. Each column lists a different outcome.In column 1, we measure whether a complaint was filed by the representative of the neighboring ward. In column 2, we focus on whether a project was initiated in the post intervention period. The regression specification across both columns is our pre-specified estimating equation. All regressions contain GP-level controls. Standard errors are reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

undertaken in the post-experimental period. Thus, we can see that the spillover effects on project initiation seem as large as the main treatment effects reported in the previous section.

What explains such a large spillover effects on project initiation? As discussed earlier, positive spillovers on project implementation can arise from three different channels: direct effects of complaint filing through 'social interaction' channel, deterrence effect, and fixed admin cost channel.

While we do not have experimental variation to formally test the relative importance of these three channels, we can provide some suggestive evidence to assess which channel is most likely to dominate in this setting. First, if the direct effects of complaints through 'social interaction' channel is the main driver, we should expect a big increase in complaint filing the neighboring ward. However, spillover effects of our treatment on rates of complaint filing is relatively small: neighboring wards are only 2.6 p.p more likely to file a complaint (Table 6, Column 1). Therefore, positive spillovers on complaint filing is unlikely to explain such a large improvement in project initiation.

We now consider whether the improvements in project initiation are due to the presence of the 'fixed administrative cost' of approving projects which encourages GP heads to resolve problems for all wards together as opposed to doing it individually. If this motive plays a key role, we should not expect much heterogeneity in spillover effects depending on ward characteristics. We test this prediction by exploiting the variation in caste identity of ward members (lower-tiered representatives) of the spillovers survey.

Table 7 presents the spillover effects of our treatment on low-caste and high-caste ward members separately.[16] It shows that improvements in project initiation is mainly concentrated amongst the low-caste neighboring ward members: they are 49 p.p more likely report project initiation as opposed to only 1.7 p.p. increase for their high-caste counterparts. This result is not consistent with the 'fixed admin cost' motive but provides more support for 'deterrence effect' of our treatment. Having a treated neighbor seems to serve as a 'credible' threat to GP heads. Perhaps, GP heads perceive higher threats from lower-caste wards as they are the ones who receive complaint filing assistance in this experiment and likely to have closer ties with other lower-caste wards in the GP.

5.2.2 Block Level Spillovers

We exploit natural variation in the intensity of treatment at the block level to estimate spillover effects. We measure the intensity of treatment by calculating the total number of treated wards in a given block. We look at the marginal effects of block-level treatment intensity on the control wards. We restrict our sample to blocks that have at least one treated ward (we are able to use 91% of the observations).

We estimate block level spillovers using the following:

[16] We did not pre-specified this heterogeneity analysis. But given social networks are segregated along caste lines, we believe it'd be useful to conduct this analysis. We cannot do a similar heterogeneity analysis for our main results since there is no variation in the caste identity of ward members: they are all low-caste (SCs).

Table 6: Spillover Effects of Complaint Filing Assistance Treatment

	(1) **Complaint Filed**	(2) **Project Initiated**
Treated Neighbor	0.026***	0.076*
	(0.010)	(0.041)
Observations	833	833
Control Mean	0	.3
Fixed Effects	Block	Block
Pre-specified	YES	YES

Table delineates the impact of the complaint filing assistance treatment on our two main spillover outcomes.Outcomes are collected via an endline survey of one randomly selected representative from a ward neighboring a representative who was part of the experimental sample. Each column lists a different outcome.In column 1, we measure whether a complaint was filed by the representative of the neighboring ward. In column 2, we focus on whether a project was initiated in the post intervention period. The regression specification across both columns is our pre-specified estimating equation. All regressions contain GP-level controls. Standard errors are reported in parentheses $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

$$Y_{igb} = \beta_0 + \beta_1 * T_b + X_b'\gamma + G_g'\theta + \delta_d + \eta_{igb}$$

Here, Y_{ig} includes whether WAS projects have been undertaken. T_b measures the intensity of treatment at block level in terms total number of treated wards in the block, X_b is a vector of controls at the block level, G_are is a vector of controls at the GP leve. δ_d indicates district fixed effects. We cluster standard errors at block level.

Since we did not randomize the intensity of treatment at block level, we cannot give these results a causal interpretation. However, we take several steps to minimize the effects of potential confounds. First, the number of treated wards in a block is a function of total number of WAS projects that were stuck in the block at the time of sampling –we directly control for it by calculating the total number of stuck projects at baseline. Second, there could still be other block level characteristics correlated with treatment intensity. Therefore we add a wide range of block level controls including size of the block in terms of area,

Table 7: Heterogeneity in Spillover Effects by Ward Leaders' Caste

	PANEL A: Ward Member is Low-Caste	
	(1)	(2)
	Complaint Filed	**Project Initiated**
Complaint Filing Assistance	-0.021	0.494**
	(0.066)	(0.230)
Observations	139	139
Control Mean	.01	.24
	PANEL B: Ward Member is High-Caste	
	(1)	(2)
	Complaint Filed	**Project Initiated**
Complaint Filing Assistance	0.021**	0.017
	(0.009)	(0.046)
Observations	694	694
Control Mean	0	.32
Fixed Effects	Block	Block

Table delineates the impact of the complaint filing assistance treatment on our two main outcome variables for two different subgroups. Each panel presents results for different subgroup of the sample. In Panel A, we focus on lower-caste (SC) ward members. In Panel B, we present results for higher-caste (Non-SC) ward members. In column 1, we measure whether the ward member filed a complaint. In column 2, we test whether a project was initiated to in the post intervention period. All regressions contain GP-level controls and block fixed effects.Standard errors are not clustered and reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

population, number of villages, and relative share of different caste groups.

Table 8 shows the results. We find that an increase in block-level treatment intensity does not have any impact on control wards: the coefficient is positive but extremely small not statistically significant(column1). However, the effects are negative for treated wards: one additional treated ward at the block level is associated with a reduction in the likelihood of project initiation by 3.4 p.p (Column 2). Negative spillover effects on the treated wards could arise due to two possible reasons. First, as the treatment intensity increases, the average time spent on resolving complaints filed by each treated ward is likely to decrease which might reduce the overall effectiveness of complaints resolution technology at the block level. Alternatively, an increase in the number of complaints at the block level could antagonize

the block officials who may become less responsive.

It's hard to disentangle these two effects as both predict that complaints are likely to become less effective with an increase in treatment intensity. As the linear specification might mask heterogeneity in treatment effects, we look at the effectiveness of our treatment at various levels of block-level treatment intensity separately. Figure 5) shows these results. We compare the project initiation rate of wards from blocks with only one treated ward with wards from blocks with four different levels of block-level treatment intensity (2, 3, 4, 5 and above). The estimates are a bit noisy due to the reduction in sample size. however, we find different patterns for control and treated wards. For the treated wards, the effects are positive when block level treatment intensity increases from 1 to 2 but turn negative as the treatment intensity increases further. No such pattern exists for control wards.

Table 8: Spillovers at Block Level

	Control Wards	Treated Wards	All Wards
	(1)	(2)	(3)
	Project Initiated	**Project Initiated**	**Project Initiated**
Total Treated Wards	0.001	-0.034***	-0.011
	(0.009)	(0.012)	(0.008)
Stuck Projects Baseline	-0.002	0.005*	0.001
	(0.002)	(0.003)	(0.002)
Observations	1280	649	1929
Sample Mean	.29	.29	.29
Fixed Effects	District	District	District
Cluster SE	Block	Block	Block
Treatment Intensity	1 and Above	1 and Above	1 and Above

This table delineates the impact of block level treatment intensity on project initiation across different types of wards. Column 1 provides results for the control wards, column 2 for the treated wards, and column 3 for all wards together. The sample for this analysis is restricted to all blocks with at least one treated ward. Outcome variable 'Project initiated' measures whether the project was initiated. All regressions contain district fixed effects and block and GP-level controls. Standard errors are clustered at block level and reported in parentheses $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

block-levelenotes

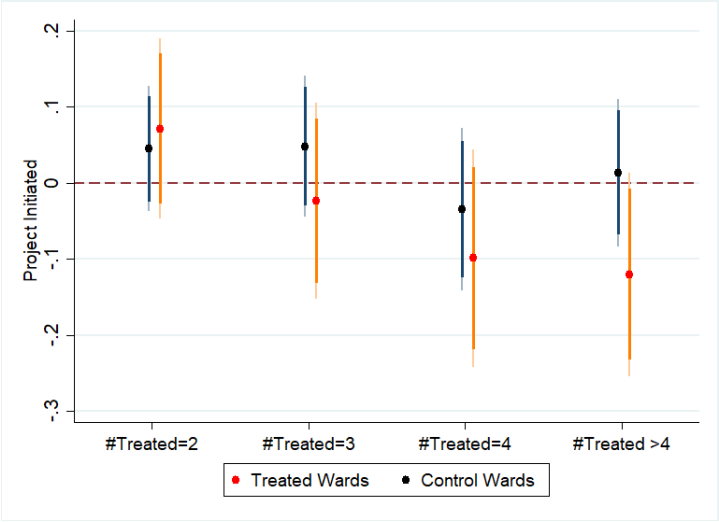

Figure 5: Impact of Block-Level Treatment Intensity on Treated Wards
This figure displays how the effects of block-level treatment intensity on treated wards vary across various levels of treatment intensity. The control group comprises of wards from blocks with only one treated ward. Y axis measures the likelihood of project initiation. All regressions have block and GP level controls and district-fixed effects. We cluster the standard errors at the block level.

5.3 Backlash Effects

One potential problem with formal complaints systems is that complaints may invite backlash from superior officials for whom complaints might impose some cost. In this case, the complaints seem to be effective in removing the impediments in the short-run, it's possible that superior officials try to punish the complainants through other channels. In order to test for it, we collected data on direct measures of backlash in the 3-month endline survey. We measured if someone from the administration approached our respondent after our intervention. However, state officials can also approach our respondents to resolve their problems. Therefore, we also collected data on the nature of conversation with the state officials and classified it into two binary categories: friendly conversation that may or may not be helpful; unfriendly conversation that includes receiving a threat or demand for a commission.

The results are shown in table 9. We can see that our treatment doesn't have much

impact on the likelihood of being approached by state officials. But treated leaders are 3 p.p. more likely to receive a threat or a demand for a commission from the upper-tiered state officials. This suggests that complaint filing can result in inviting backlash and it could be a valid concern in this setting.

Table 9: Backlash Effects of Complaint Filing in the Short-Run

	ITT Effects		**TOT Effects**	
	(1) Officials Approached	(2) Unfriendly Conversation	(3) Officials Approached	(4) Unfriendly Conversation
Complaint Filing Assistance	0.021	0.030***		
	(0.032)	(0.011)		
Complaint Filed			0.050	0.077***
			(0.074)	(0.026)
Observations	1329	1329	1327	1327
Control Mean	.48	.02	.48	.02
Fixed Effects	Block	Block	Block	Block

Table delineates the impact of complaint filing assistance treatment on two possible self-reported measures of backlash. Each column considers a different regression specification. Column 1 presents the ITT effects of complaint filing assistance treatment on whether ward members were approached by state officials. Column 2 presents the ITT effects of our treatment on whether the naure of conversation with offcials was unfriendly: this includes a threat or demand for a commission. Columns 3 and 4 present the TOT effects where the treatment status of ward members serves as an instrument for the actual complaint filing rate. The regression specification across both panels is our pre-specified estimating equation. All regressions contain GP-level controls. Standard errors are reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$T

It is possible that backlash effects matter only for a small subset of leaders, there are several reasons this may be an underestimate. First, backlash effects can take many forms and come from a variety of sources. We could capture only a few possible measures. For instance, upper-tiered state officials could block access to public services and benefits ward members are entitled to receive. Second, many respondents may not want to report threats from state officials. Third, we only measure threats in the first 3 months. It is possible that backlash effects are subdued in the short-run but likely to become stronger with time.

5.4 Electoral Consequences of Complaint Filing

It is important to look at electoral consequences of complaints filing for at least two reasons. First, continued use of a formal complaints technology by politicians is likely to be largely determined by whether it hurts or improves reelection chances. Second, the net electoral effects of complaints technology is theoretically ambiguous. As complaint filing results in reducing the delays in project implementation, it might help improve electoral returns. But, complaints could also invite backlash from upper-tiered politicians which might hurt them electorally.

Table 10 shows the impact of complaint filing assistance treatment on reelection probability in 2021 local elections. Our treatment results in a 4 p.p (14%) reduction in the likelihood of reelection but it's not statistically significant. The TOT effects are much bigger: they suggest a 10 p.p (30%) reduction the likelihood of reelection. We need to be cautious while interpreting the results as the estimates are imprecise.

Table 10: Effects of Complaint Filing on Reelection

	ITT Effects		**TOT Effects**	
	(1)	(2)	(3)	(4)
	Reelected	Run	Reelected	Run
Complaint Filing Assistance	-0.042	-0.047*		
	(0.029)	(0.027)		
Complaint Filed			-0.108	-0.123*
			(0.067)	(0.064)
Observations	1236	1222	1236	1222
Control Mean	.27	.79	.27	.79
Fixed Effects	Block	Block	Block	Block

Table delineates the impact of complaint filing assistance treatment on reelection probability and likelihood of running in 2021 local elections. Each column considers a different regression specification. Column 1 presents the ITT effects of of complaint filing assistance treatment on whether ward members are reelected. Column 2 present the ITT effects of our treatment on whether ward members run in 2021 elections. Column 3 and Column 4 present the TOT effects where treatment status of ward members serves as an instrument for the actual complaint filing rate. The regression specification across both panels is our pre-specified estimating equation. All regressions contain GP-level controls. Standard errors are reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 11: Effects of Complaint Filing on Electoral Outcomes

	(1) **Win**	(2) **Vote Share**	(3) **Total Candidate**
Complaint Filing Assistance	-0.036	-0.004	-0.181
	(0.037)	(0.014)	(0.121)
Observations	948	883	1203
Control Mean	.27	.28	4.65
Fixed Effects	Block	Block	Block

Table delineates the ITT effects of complaint filing assistance treatment on three different electoral outcomes: probability of winning the election conditional on running (column1), vote share of ward members conditional on running (column 2), total number of candidates who contest for the ward members' post in 2021 local elections (column 3). The regression specification across all columns is our pre-specified estimating equation. All regressions contain GP-level controls. Standard errors are reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

We now investigate possible reasons for negative treatment effects on reelection probability. Reduction in reelection can arise both because ward members decide not to run in the elections and are less likely win conditional on running. We find that treated ward members are 4.8 p.p less likely to run in local elections((Table 10, col 2). Likelihood of winning conditional on running continues to be negative and statistically insignificant (table 11). We also look at the impact of our treatment on vote share and total number of candidates but do not find any meaningful impact.

We carry out additional analysis to test if the negative treatment effects on reelection weakens when we expect the backlash from upper-tiered politicians to be lower. Recall the main spillover effects of our treatment on complaint filing and project initiation: treatment results in a mere 2.6 p.p increase in likelihood of complaints being filed but we still see increase in project initiation by 8 p.p. As the spillover effects on complaint filing rates are positive but very small, one should not expect much backlash on the spillover sample. We test for this by estimating the effects of having a treated ward member on the reelection probability neighboring wards: unlike the main effects, the spillover effects on reelection

probability is not negative. The treatment effects are extremely small and are not statistically significant (Table A3).

6 Heterogeneity by caste of upper-tiered politicians

One main dimension of heterogeneity is caste. As discussed earlier, upper-tiered politicians – GP heads – play a key role in implementation of WAS projects. Does their caste affect how they respond to complaints?

We first show that even prior to the experiment, complaint filing rates among low-caste ward members vary depending on the caste of the GP-head. To do so, we rely on our administrative data of complaints covering the period 2016 - 2019. We exploit a rule used to "reserve" seats for GP heads that creates exogenous variation in their caste. Essentially, GPs with SC populations above a threshold have SC GP heads. This allows us to identify causal effects of having a GP head through an RD design. We can estimate the treatment effects of having an SC GP head using fuzzy RD design with a strong first stage (compliance to the reservation rule is not perfect). The underlying identifying assumptions and other details of this RD is explained in the appendix A.4.

We begin by showing that low-caste ward members are more likely to file complaints about WAS schemes when exogenously paired with a high-caste GP head (Table A8). Column (1) of panel A says that SC ward members paired with non-SC GP heads are twice as likely to file complaints regarding non-implementation of WAS schemes. [17] This, we interpret as evidence of the importance of the caste of the GP head in determining GP-level outcomes and take-up of the complaints' system.

We now test if caste of the GP village head affects take-up and outcomes in our ex-

[17] Furthermore, this is not the case for non-SC ward members, who file no additional complaints when paired either with high-caste or low-caste GP heads.

periment. Two caveats before we proceed to our results: first, we did not pre-register this heterogeneity analysis for our experiment; second, many characteristics vary along with group identity which makes it difficult to interpret the underlying reasons for differences in treatment effects across groups. In our case, average characteristics of GPs headed by low-caste leaders are very different from the ones headed by higher-caste leaders.

We proceed using a strategy similar to the standard Differences-in-Discontinuity designs. We have two "treatment" variables: (a) the treatment from the experiment, which is randomly assigned and (b) the treatment of having a GP head who is SC, which is assigned randomly within the RD bandwidth and close to the threshold.

Under the assumption of continuity of all other GP characteristics, the fuzzy RD estimator calculates the local average treatment effect (LATE) of having an SC representative with a population equal to the cutoff population for a block. Since we are interested in heterogeneous treatment effects so we estimate the following regression using 2SLS framework where we treat with SCReserved and Treated*SCReserved as endogenous:

$$\begin{aligned} SCReserved_{gb} = \gamma_0 + \gamma_1 1(SCPop_{gb} > T_b) + \gamma_2(SCPop_{gb} - T_b)* \\ 1(SCPop_{gb} \geq T_b) + \gamma_3 Treated_{igb} + \gamma_4 Treated_{igb} * 1(SCPop_{gb} > T_b) \\ + \delta X_g + \psi + \eta_{gb} \\ Y_{igb} = \beta_0 + \beta_1 SCReserved_{gb} + \beta_2(SCPop_{gb} - T_b) * 1(SCPop_{gb} \geq T_b) + \\ \beta_3 Treated_{igb} + \beta_4 Treated_{igb} * SCReserved_{gb} + \omega X_g + \alpha + \epsilon_{gb} \end{aligned}$$

Where Y_{igb} is the outcome of interest in ward i GP g and Block b. T_b is the SC population cutoff for GPs in block b, $SCPop_{gb}$ is the SC-GP population, X_g is a vector of GP-level controls and ψ indicates block fixed effects. η_{gb} and ϵ_{gb} are error terms. GP level controls

include total population of GP, distance to the nearest town/district head-quarters, whether GP was reserved for women/OBCs/STs.

Here we treat $SCReserved_{gb}$ and $Treated_{igb} * SCReserved_{gb}$ as endogenous and use predicted values from stage 1, $\hat{SCReserved}_{gb}$ and its interaction with complaint filing assistance treatment, $Treated_{igb} * \hat{SCReserved}_{gb}$ as instruments.

The bandwidth used for the RD estimator is the same used in Table A8. The table in the appendix (Table**??** & TableA10) shows robustness to RD bandwidths.

Table 12 shows the heterogeneous treatment effects on four main outcomes of interest: whether a complaint was filed (stage1), project initiated, whether received threats in the short run and the likelihood of getting reelected. Given relatively small sample size, it seems that we are not powered to detect heterogeneous treatment effects: coefficients for most of the outcome variables are not statistically significant. But the magnitudes are reasonably large and the direction of the effects flips completely when we change the caste identity of upper-tiered politicians (GP heads).

First, ward members who are governed by low-caste (SC) GP heads, are 0.8 p.p. less likely to file complaints in response to our treatment. They are 4 p.p. more likely start projects, 4.9 p.p. less likely to receive threats from upper tiered officials, and 18 p.p. more likely to get reelected in the local elections. Thus, when our ward members (low-caste) match with low-caste GP heads, our treatment seems to work well for them on all fronts.

However, when we look at the treatment effects for ward members who are governed by high-caste (Non-SC) GP heads, the results change in the opposite direction. The ward members are 22 p.p. more likely to file complaints, 8 p.p. less likely to initiate projects, 6.1 p.p more likely to receive threats, and 11 p.p. less likely to get reelected in local elections. These results suggest that the positive impact of complaint filing on minority leaders might be dampened if upper-tiered officials are from dominant social groups.

Why do higher-caste, upper-tiered politicians respond negatively to the complaints? This

might be because the upper-tiered politicians see complaints from low-caste ward members as a challenge to their authority. The upper-caste politicians are likely to react more strongly due to 'status-threat' concerns (Gidron and Hall (2017), Mutz (2018)).

Table 12: Heterogeneous Treatment Effects: Does Identity of Upper-tiered Politicians Matter?

	(1) **Complaint Filed**	(2) **Project Initiated**	(3) **Received Threats**	(4) **Reelection Probability**
Treatmnet*SCReserved	-0.084	0.047	-0.049	0.188
	(0.090)	(0.111)	(0.051)	(0.125)
SC Reservd	-0.074	-0.078	0.066*	-0.224**
	(0.069)	(0.086)	(0.039)	(0.090)
Filing Assistance Treatment	0.382***	0.023	0.045**	-0.080*
	(0.034)	(0.043)	(0.020)	(0.045)
Observations	666	666	666	616
Control Mean	0	.26	.02	.26
Treat*Non-SC	.223	-.008	.061	-.115
Fixed Effects	Block	Block	Block	Block

This table delineates the impact our complaint filing assistance treatment for wards governed by low-caste (SC) and higher-caste (Non-SC) GP heads on four different outcomes. In column 1, we measure whether a complaint was filed by the lower-tiered representatives. In column 2, we focus on whether a project was initiated in thupper-tieredvention period. Column 3 looks at whether lower-tiered representatives report receiving threats or demand for commission in the 3-month survey. Column 4 looks at if lower-tiered representatives get reelected in the 2021 local elections. The interaction term Treated*SCReserved (row 1) captures the effects of our treatment for wards governed by low-caste (SC) representatives. The treatment effects for wards governed by higher-caste (Non-SC) GP heads is calculated by adding the first three coefficients and reported against Treat*Non-SC (row 6). These estimates are generated using fuzzy RD specifications described in the paper. We control for GP-level covariates and Block-fixed effects. All standard errors are clustered at the Block level. Standard errors are reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

7 Conclusions

Lack of cooperation across different layers of government can be a major impediment to public goods provision. Local leaders from disadvantaged backgrounds often suffer the most owing to their low status in bureaucratic and social hierarchy. This paper shows that complaint resolution systems can serve as an effective tool for minority leaders to improve their bargaining power with upper-tiered officials of the state. We show that complaint filing by minority leaders helps remove the impediments in public goods projects implementation in their constituencies.

However, our study also reveals that, mere presence of a complaint resolution system is not sufficient in improving outcomes: less than 1% minority leaders had filed complaints prior to our intervention. There seems to be several barriers that prevent them from using it. We show that increasing awareness levels leads to a small increase in complaint filing but reducing transaction costs associated with complaint filing results in a far greater improvement. Unpacking various types of transaction costs and empirically testing their relative importance would be an useful avenue for future research.

Lastly, our results also suggest that complaint filing could invite backlash from upper-tiered functionaries of the state who are affected by the complaints. It's surprising that despite improvement in project implementation, our treatment does not fetch positive electoral returns. If anything, the electoral consequences appears to be negative. This makes it important to study the dynamic consequences of complaint filing. If complaint filing reduces reelection probability of politicians, it might dissuade politicians from using it in future which could limit the effectiveness of complaint systems in the long run. Studying the dynamic consequences of complaint should be an important avenue for future research.

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A Appendices

A.1 Tables

Table A1: Balance Checks for Complaint Filing Assistance Treatment: Spillover Sample

Variable	(1) Control	(2) Treatment	(3) Difference
Mean SC Wealth Score (in GP)	-0.383	-0.354	0.029
	(0.605)	(0.635)	(0.458)
Mean non-SC Wealth Score (in GP)	0.312	0.342	0.030
	(0.786)	(0.769)	(0.539)
Proportion of SCs (Census 2011)	0.193	0.192	-0.002
	(0.089)	(0.083)	(0.773)
Distance to District Headquarters (Census 2011)	30.988	31.003	0.015
	(18.066)	(18.087)	(0.989)
Total GP Area (Census 2011)	1,176.696	1,114.613	-62.082
	(1,024.201)	(663.783)	(0.249)
Total Population of GP (Census 2011)	11,949.118	11,671.403	-277.715
	(5,237.602)	(4,103.171)	(0.345)
Percentage of SCs in Main SC Village (Census 2011)	0.562	0.557	-0.005
	(0.248)	(0.248)	(0.770)
Percentage of all SCs in Main SC Village	0.302	0.315	0.012
	(0.188)	(0.195)	(0.313)
Margin of Victory of Upper-Tiered Representative (Votes)	170.949	173.879	2.930
	(169.181)	(172.773)	(0.786)
Lower-Tiered Representative's Age	39.157	38.378	-0.779
	(11.569)	(10.781)	(0.266)
Lower-Tiered Representative's Gender	0.454	0.470	0.016
	(0.498)	(0.500)	(0.612)
Lower Tiered Representative is Illiterate	0.114	0.095	-0.019
	(0.318)	(0.294)	(0.319)
Lower Tiered Representative is Literate	0.509	0.533	0.024
	(0.500)	(0.499)	(0.444)
Observations	517	506	1,023

Table presents category-wise averages and t-tests of difference in means.Standard errors are reported in parentheses

Table A2: Balance Checks for Information Treatment

Variable	(1) Control	(2) Treatment	(3) Difference
Mean SC Wealth Score (in GP)	-0.403	-0.427	-0.024
	(0.518)	(0.615)	(0.070)
Mean non-SC Wealth Score (in GP)	0.320	0.369	0.048
	(0.750)	(0.837)	(0.097)
Proportion of SCs (Census 2011)	0.193	0.198	0.006
	(0.090)	(0.075)	(0.010)
Distance to District Headquarters (Census 2011)	33.056	30.453	-2.603
	(20.119)	(16.141)	(2.211)
Total GP Area (Census 2011)	1,088.290	1,026.992	-61.298
	(688.985)	(583.970)	(77.395)
Total Population of GP (Census 2011)	11,978.191	11,813.661	-164.530
	(4,533.703)	(5,026.136)	(583.165)
Percentage of SCs in Main SC Village (Census 2011)	0.580	0.558	-0.022
	(0.249)	(0.257)	(0.031)
Percentage of all SCs in Main SC Village	0.282	0.331	0.048
	(0.164)	(0.230)	(0.025)
Margin of Victory of Upper-Tiered Representative (Votes)	169.125	183.985	14.860
	(162.299)	(184.369)	(21.334)
Lower-Tiered Representative's Age	38.411	38.138	-0.273
	(10.663)	(10.427)	(1.282)
Lower-Tiered Representative's Gender	0.441	0.508	0.067
	(0.498)	(0.502)	(0.061)
Lower Tiered Representative is Illiterate	0.135	0.123	-0.012
	(0.343)	(0.330)	(0.041)
Lower Tiered Representative is Literate	0.525	0.538	0.014
	(0.501)	(0.500)	(0.061)
Observations	141	130	271

Table presents category-wise averages and t-tests of difference in means.Standard errors are reported in parentheses

Table A3: Effects of Treated Neighboring Wards on Reelection

	If Reelected			
	(1)	(2)	(3)	(4)
Treated Neighbor	0.002	0.002	-0.009	-0.007
	(0.010)	(0.013)	(0.008)	(0.008)
Observations	10347	10347	10347	10347
Control Mean	.2	.2	.2	.2
Fixed Effects	Block	Block	SubDivision	District
Clustered SE	NO	YES	NO	NO

Table delineates the impact of having a treated neighboring ward on the reelection probability in 2021 local elections. We restrict our sample to GPs that have only one experimental ward. For this analysis, we include all non-experimental wards in a given GP. The first column is our pre-specified estimating equation. Other columns vary the level of fixed effects and cluster standard errors at the block level where mentioned. All regressions contain GP-level controls. All regressions contain GP-level controls. Standard errors are reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A4: Effects of Complaint Filing on Likelihood of Running For Higher Posts

	If Ran for Higher Posts			
	(1)	(2)	(3)	(4)
Complaint Filing	0.004	0.004	0.006	0.005
	(0.004)	(0.005)	(0.004)	(0.004)
Observations	1247	1247	1247	1247
Control Mean	0	0	0	0
Fixed Effects	Block	Block	SubDivision	District
Clustered SE	NO	YES	NO	NO
Pre-specified	YES	NO	NO	NO

Table delineates the impact of complaint filing assistance treatment on probability of running for higher posts in 2021 local elections. Each column considers a different regression specification. The first column is our pre-specified sestimating equation. Other columns vary the level of fixed effects and cluster standard errors at the block level where mentioned. All regressions contain GP-level controls. All regressions contain GP-level controls. Standard errors are reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

A.2 Robustness Checks

Table A5: ITT Effects of Complaint Filing on WAS Projects: robustness

	PANEL A: Problem Solved			
	(1)	(2)	(3)	(4)
Filing Assistance Treatment	0.105	0.105	0.075	0.076
	(0.030)	(0.035)	(0.028)	(0.027)
Observations	1332	1332	1332	1332
Control Mean	.41	.41	.41	.41
	PANEL B: If Project Started			
	(1)	(2)	(3)	(4)
Filing Assistance Treatment	0.069	0.069	0.045	0.046
	(0.028)	(0.031)	(0.025)	(0.024)
Observations	1332	1332	1332	1332
Control Mean	.27	.27	.27	.27
Fixed Effects	Block	Block	SubDivision	District
Clustered SE	NO	YES	NO	NO
Pre-specified	YES	NO	NO	NO

Table delineates the impact of the complaint filing assistance treatment on our two main outcome variables. Columns 1 and 2 provides results for the whole sample and columns 3 and 4 for the restricted sample. The restricted sample is generated by dropping GPs that have more than one experimental wards. 'Problem solved' outcome variables captures whether the problem preventing the SC ward members from initiating projects was resolved. Project initiated measures whether the project was, subsequently, initiated. Column (1) and (2) is our pre-specified estimating equation. Column (3) and (4) attempts to check the extent to which within-GP spillover could effect our main results. All regressions contain block fixed effects and GP-level controls. Standard errors are reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

The ITT effects of complaint filing assistance treatment on reelection seem robust to changing the level of fixed effects and clustering errors at different levels as shown in Table A7 (cols (2)-(4)).

Table A6: ITT Effects of Complaint Filing on WAS Projects: spillovers a concern?

	Whole Sample		Restricted Sample	
	(1) **Problem Solved**	(2) **Project Initiated**	(3) **Problem Solved**	(4) **Project Initiated**
Filing Assistance Treatment	0.105***	0.069**	0.138***	0.082**
	(0.030)	(0.028)	(0.035)	(0.032)
Observations	1332	1332	1109	1109
Control Mean	.41	.27	.4	.27
Fixed Effects	Block	Block	Block	Block
Pre-specified	YES	YES	YES	YES

Table delineates the impact of the complaint filing assistance treatment on our two main outcome variables across different samples. Columns 1 and 2 provides results for the whole sample and columns 3 and 4 for the restricted sample. The restricted sample is generated by dropping GPs that have more than one experimental wards. 'Problem solved' outcome variables captures whether the problem preventing the SC ward members from initiating projects was resolved. Project initiated measures whether the project was, subsequently, initiated. Column (1) and (2) is our pre-specified estimating equation. Column (3) and (4) attempts to check the extent to which within-GP spillover could effect our main results. All regressions contain block fixed effects and GP-level controls. Standard errors are reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A7: Effects of Complaint Filing on Reelection

	If Reelected			
	(1)	(2)	(3)	(4)
Complaint Filing	-0.037	-0.037	-0.033	-0.030
	(0.029)	(0.038)	(0.026)	(0.026)
Observations	1224	1224	1224	1224
Control Mean	.27	.27	.27	.27
Fixed Effects	Block	Block	SubDivision	District
Pre-specified	YES	NO	NO	NO

Table delineates the impact of complaint filing assistance treatment on reelection probability in 2021 local elections. Each column considers a different regression specification. The first column is our pre-specified estimating equation. Other columns vary the level of fixed effects and cluster standard errors at the block level where mentioned. All regressions contain GP-level controls. All regressions contain GP-level controls. Standard errors are reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A8: How do caste differences affect complaining rates?

	PANEL A: SC Lower-Tiered Representatives				
	(1) **WAS Pubic Goods**	(2) **Local Government**	(3) **All Public Goods**	(4) **Mention Ward**	(5) **Placebo Private**
Upper-Caste GP Head (SC)	0.015	0.016	0.031	0.012	0.002
	(0.007)	(0.008)	(0.012)	(0.005)	(0.013)
Observations	16917	16917	16917	16917	16917
Control Mean	.01	.01	.02	0	.02
Upper Band	440.59	345.68	446.96	359.17	444.97
Block FE	YES	YES	YES	YES	YES
	PANEL B: Non-SC Lower-Tiered Representatives				
	(1)	(2)	(3)	(4)	(5)
Upper-Caste GP Head (NSC)	0.001	0.003	-0.004	0.003	0.027
	(0.003)	(0.004)	(0.012)	(0.003)	(0.019)
Observations	65775	65775	65775	65775	65775
Control Mean	.01	.01	.02	.01	.02
Upper Band	329.31	200.85	172.6	292.35	253.8
Block FE	YES	YES	YES	YES	YES

Outcome variables are binary variables and are as follows: column (1) indicates whether a WAS complaint is filed by the lower-tiered representative; column (2) indicates whether a complaint about local government is filed; column (3) refers to whether a complaint is filed regarding the GP administration; column (4) indicates whether the text of the complaint mentioned the term "ward"; column (5) indicates whether a complaint was filed regarding a "private" issue of the individual/their household. In panel A, "Caste Differences" is the treatment variable which takes the value of 1 if the SC-GP population is below the population threshold (and hence differences occur). For lower-tiered SC representatives (who we restrict attention to here), this implies potential caste matching above and caste differences below. In Panel (B), Caste Differences (NSC) is the treatment variable which takes the value of 1 if the SC-GP population is above the population threshold. We use CCT triangular bandwidths and estimate fuzzy RD specifications described in the paper (equation **??** and **??**).We control for GP-level covariates, ward-level covariates and add Block-fixed effects. All standard errors are clustered at the GP-level.

Table A9: Heterogeneous Treatment Effects: Does Identity of Upper-tiered Politicians Matter? (Bandwidth=250)

	(1) **Complaint Filed**	(2) **Project Initiated**	(3) **Received Threats**	(4) **Reelection Probability**
Treatmnet*SCReserved	-0.035	-0.077	0.006	0.326**
	(0.100)	(0.125)	(0.063)	(0.139)
SC Reservd	-0.184**	0.002	0.037	-0.340***
	(0.085)	(0.107)	(0.054)	(0.110)
Filing Assistance Treatment	0.343***	-0.005	0.060**	-0.116*
	(0.046)	(0.058)	(0.029)	(0.060)
Observations	376	376	376	346
Control Mean	0	.27	.02	.28
Treat*Non-SC	.123	-.079	.103	-.129
Fixed Effects	Block	Block	Block	Block

This table delineates the impact our complaint filing assistance treatment for wards governed by low-caste (SC) an higher-caste (Non-SC) GP heads on four different outcomes. In column 1, we measure whether a complaint was filed b the lower-tiered representatives. In column 2, we focus on whether a project was initiated in the post intervention perio Column 3 looks at whether lower-tiered representatives report receiving threats or demand for commission in the 3-mont survey. Column 4 looks at if lower-tiered representatives get reelected in the 2021 local elections. The interaction ter Treated*SCReserved (row 1) captures the effects of our treatment for wards governed by low-caste (SC) representative The treatment effects for wards governed by higher-caste (Non-SC) GP heads is calculated by adding the first thre coefficients and reported against Treat*Non-SC (row 6). These estimates are generated using fuzzy RD specificatio described in the paper. We control for GP-level covariates and Block-fixed effects. All standard errors are clustered at th Block level. Standard errors are reported in parentheses $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

Table A10: Heterogeneous Treatment Effects: Does Identity of Upper-tiered Politicians Matter? (bandwidth=750

	(1) **Complaint Filed**	(2) **Project Initiated**	(3) **Received Threats**	(4) **Reelection Probability**
Treatmnet*SCReserved	-0.143	0.182*	-0.018	0.021
	(0.090)	(0.109)	(0.044)	(0.121)
SC Reservd	-0.038	-0.064	0.033	-0.154*
	(0.062)	(0.076)	(0.031)	(0.081)
Filing Assistance Treatment	0.416***	0.035	0.038**	-0.072*
	(0.032)	(0.039)	(0.016)	(0.042)
Observations	890	890	890	826
Control Mean	.01	.25	.02	.28
Treat*Non-SC	.235	.153	.053	-.204
Fixed Effects	Block	Block	Block	Block

This table delineates the impact our complaint filing assistance treatment for wards governed by low-caste (SC) an higher-caste (Non-SC) GP heads on four different outcomes. In column 1, we measure whether a complaint was filed b the lower-tiered representatives. In column 2, we focus on whether a project was initiated in the post intervention perio Column 3 looks at whether lower-tiered representatives report receiving threats or demand for commission in the 3-mont survey. Column 4 looks at if lower-tiered representatives get reelected in the 2021 local elections. The interaction ter Treated*SCReserved (row 1) captures the effects of our treatment for wards governed by low-caste (SC) representative The treatment effects for wards governed by higher-caste (Non-SC) GP heads is calculated by adding the first thre coefficients and reported against Treat*Non-SC (row 6). These estimates are generated using fuzzy RD specificatio described in the paper. We control for GP-level covariates and Block-fixed effects. All standard errors are clustered at th Block level. Standard errors are reported in parentheses $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

A.3 Understanding Adoption of Formal Complaints Technology

Complaint filing appears to be an effective tool for the local leaders. However, in absence of our treatment, very few leaders file a complaint [18]. Naturally, we would like to understand the constraints to adoption of this formal complaints technology. We can think of a large number of constraints: lack of information, pessimistic beliefs about effectiveness, ability to carry out paper work, cost of complaint filing, fear of backlash from higher state officials. It's beyond the scope of this study to experimentally test the importance of all these factors, we have tried study the role of information about the complaints resolution system.

Aside from our complaints filing assistance treatment arm, we ran a smaller experiment with a sample of lower-tiered SC representatives where we offered them information about the formal complaints technology. We find that information alone increases filing rates, but at a relatively lower rate. Compared to the control group, information results in 7 p.p more grievances (see Figure 6). Compare this to our complaint filing assistance treatment arm where complaints filed increased by 40 p.p. Thus, information is a constraint, but there are other costs to grievance-filing that make it less commonly used. While complaint filing assistance treatment results in a far bigger increase ,it is important to recognize that it is a very strong strong treatment. Under this treatment, local leaders do not have to put in any effort as complaints are filed on their behalf by the research team. Thus, it reduces the cost of complaint filing to zero. However, complaint filing assistance treatment fail to remove some constraints including fear of backlash from higher state officials, pessimistic beliefs about effectiveness.

Other Constraints: In our setting, complaints can be filed in three ways: via the phone, via the internet and in person. During piloting, we experimented with trying to get lower-tiered representatives to file complaints via the phone. This proved extremely difficult, since complaint filing is a complex process, involving clear communication of the nature of

[18] In control group, less than 1 percent ward members file a complaint

the problem that extends beyond yes-no binaries. The call-centres were manned by urban youth; the representatives speaking to them were leaders, but from extremely marginalized groups in villages. Only 3% of complaints are filed via the call-centre. If complaining via the phone is difficult, accessing the internet and filling up text on an online portal is even harder. Thus, an intermediary is necessary for both these ways of filing complaints. These results echo closely the work of Gupta (2017), who finds that information and mediation are both crucial factors in helping marginalized citizens access the state. Complaining in person is easier to navigate relative to via the phone or the internet. This is because the grievance centres often have trained operators who convert verbal or written complaints into a standardized format that is fed into the online system. However, there is one grievance centre for every 80 GPs on average. Traveling to these centres is costly. Our survey estimates put it at INR 140 per trip and the loss of a full day's wage. Indeed, our data shows that the number of complaints filed falls away sharply as distance to the grievance redressal centre increases.

A.4 RD Framework

The state of Bihar is divided into 38 districts, which are further divided into 534 blocks and 8400 GPs. Within each block, selection of GPs to be reserved is carried out in two steps. First, the total number GPs to be reserved for SCs is determined by the share of SC population in a given block.Next, all the GPs in the block are arranged in descending order based on their GP level SC population and top GPs are selected. This reservation rule gives rise to an exogenous SC population cut-off, below which no GP is reserved. Above the cut-off, not all GPs are reserved for SCs, as some are blocked to be reserved for OBCs. In practice, as Figure **??** shows, once we throw away GPs above the cut-off that are blocked, the first stage results in a near 85 percent jump in the probability of reservation. Thus, we have a fuzzy pooled RD with a strong first stage. We also check for manipulation around

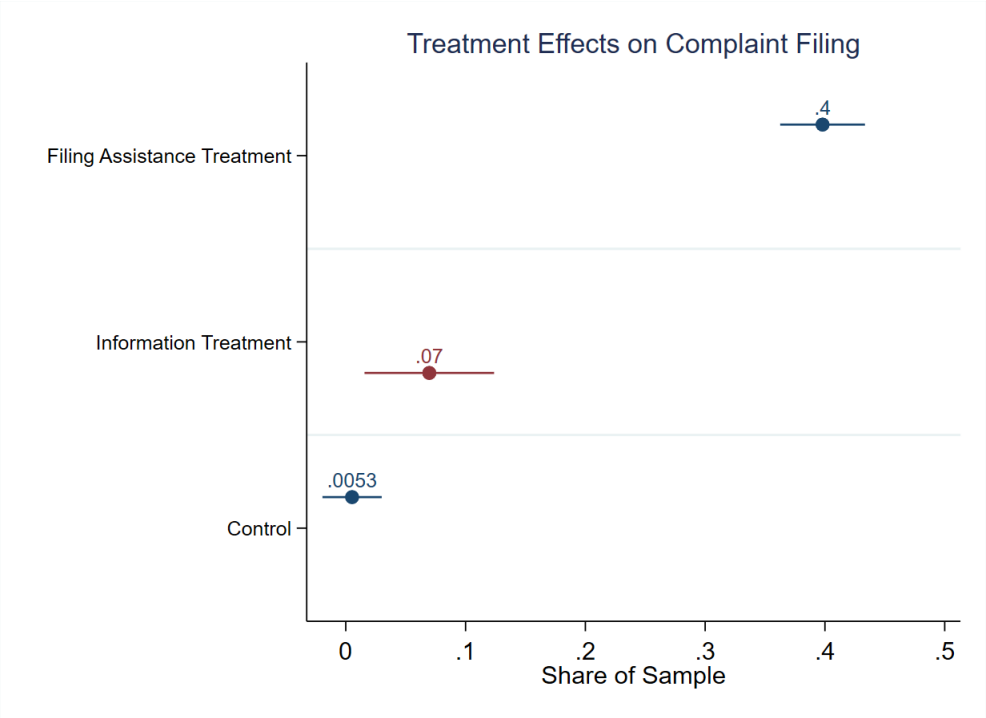


Figure 6: Information Treatment and Complaint Filing
This figure displays the impact of our two main intervention arms – a complaint filing assistance treatment and the information treatment on whether complaints are filed (this is the "first stage" of the experiment

the cutoff by carrying out McCray test and find that the density is reasonably smooth at the cutoff (see Figure 8).

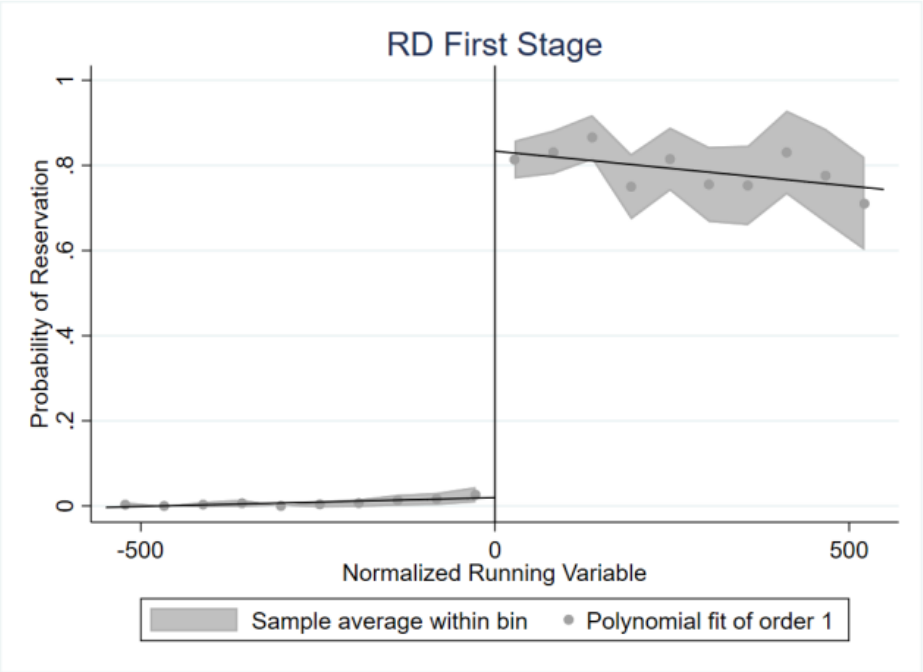


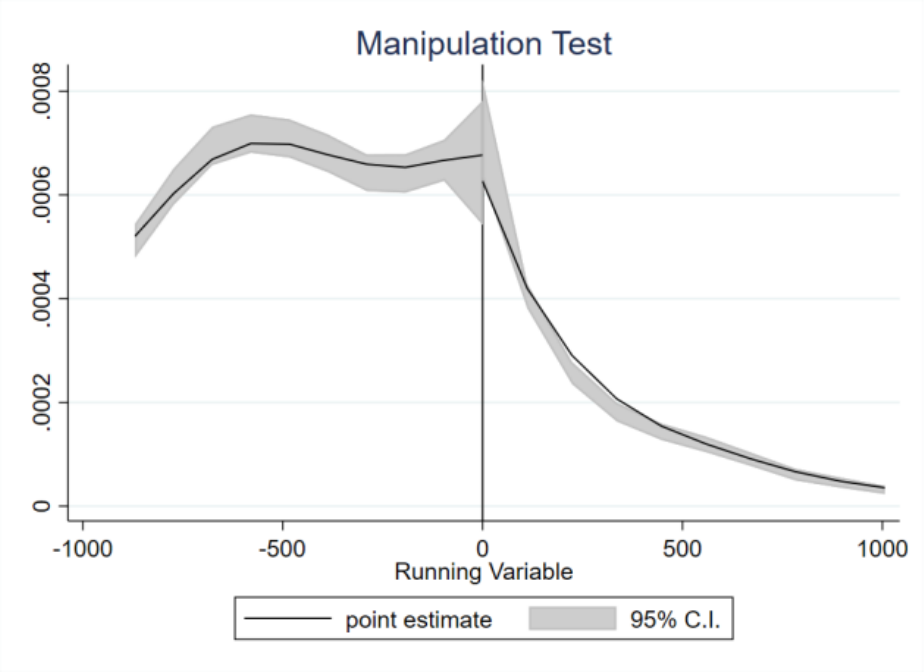
Figure 7: Probability of reservation based on the rank of a GP within a Block

Our running variable is the difference in SC population of a GP and the mean of the SC Population of the last Panchayat to not be reserved and the first GP to be reserved. Thus, for GP i i in Block j:

$$Running_{ij} = SCPop_{ij} - \left(\frac{SCPop_{1j} + SCPop_{0j}}{2} \right)$$

where SCPop refers to SC Population and 0 and 1 subscripts stand for the the last GP to not be reserved and the first GP to be reserved, respectively.

Under the assumption of continuity of all other GP characteristics, the fuzzy RD estimator calculates the local average treatment effect (LATE) of having an SC representative



(a) Mccrary Test

Figure 8: Mccrary Test for Manipulation

with population equal to the cutoff population for a block. We use the following two-stage instrumental variables specification:

$$Reserved_{gb} = \gamma_0 + \gamma_1 1(SCPop_{gb} > T_b) + \gamma_2(SCPop_{gb} - T_b) * 1(SCPop_{gb} \geq T_b) + \delta X_g + \psi + \eta_{gb}$$

$$Y_{gb} = \beta_0 + \beta_1 Reserved_{gb} + \beta_2(SCPop_{gb} - T_b) * 1(SCPop_{gb} \geq T_b) + \omega X_g + \alpha + \epsilon_{gb}$$

Where Y_{gb} is the outcome of interest in GP g and Block b. T_b is the SC population cutoff for GPs in block b, $SCPop_{gb}$ is the SC-GP population, X_g is a vector of GP-level controls

and ψ indicates block fixed effects. η_{gb} and ϵ_{gb} are error terms. GP level controls include total population of GP, distance to the nearest town/district head-quarters, whether GP was reserved for women/OBCs/STs.