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ABSTRACT

This thesis consists of three chapters. In Chapter 1, 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 treat- ment 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 com- plaint 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.

In Chapter 2, we study the distributional consequences of reservation policies in the context of mandated political representation ("reservation") in favour of the marginalized Scheduled Caste (SC) groups in India. We bring to bear a wealth of data: secondary data on public goods from across 45,000 villages, private assets from over 19 million rural households, political candidacy data of over 300,000 candidates, and a primary survey of over 8,700 households from the state of Bihar. Using a regression discontinuity design framework, we show that reservation for SCs for the post of local government head (a) lowers SC-non-SC disparities in access to public goods in the short-run (5 years later) and long-run (13 years later) (b) lowers inter-group private asset inequality modestly in the short-run and substantially in the long-run (c) creates different sets of winners and losers *within* SCs and non-SCs (d) has no efficiency consequences in the short-run and (e) increases political participation and presence of SCs in local government in the long-run. We exploit a unique

feature of our RD design to show that the causal impact of the reservation is largest when SCs are neither too large nor too small in number. Turning to mechanisms, we show that (i) government schemes are better targeted towards SCs in reserved constituencies and (ii) intra-SC heterogeneity lowers impacts of reservation.

Chapter 3 studies why minorities are underrepresented in enterprise ownership and leadership positions in big firms. This chapter empirically investigates the role of one potential reason for this: discrimination against minority employers by subordinate workers. I embed a field experiment in the recruitment of entry-level workers by a set of firms based in India. The field experiment aims to answer two main research questions: 1) Do minority employers face discrimination from below in labor markets? 2) What are the underlying motivations? I specifically test for two potential motives: attention discrimination and social image concerns. Preliminary results show that applicants are 3 p.p. (26%) less likely to apply for jobs advertised by minority employers. I also find strong evidence for 'attention discrimination' against minority employers.

CHAPTER 1

COMPLAINT RESOLUTION SYSTEMS: EXPERIMENTAL EVIDENCE FROM RURAL INDIA

1.1 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

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 federal or state government department. 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

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 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 large 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

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

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 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 prioritized 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

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of funds to ward members for implementing WAS projects.

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.

technology and offered to file complaints regarding WAS project initiation on their behalf. In 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 A5

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.

1.2 Background and Context

1.2.1 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 3.1 depicts the state administration structure and shows where Blocks, GPs, Wards are placed in the overall structure of the state.

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%

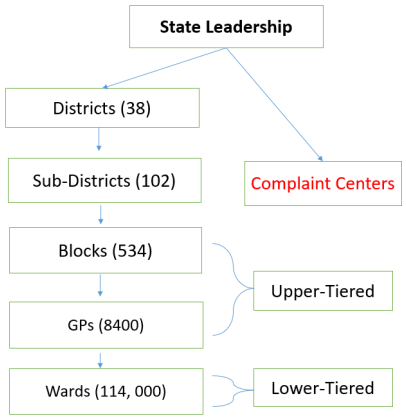


Figure 1.1: State Administration Structure
This figure displays how different layers of the state are connected to each other. Districts are the Main administrative unit below the state secretariat which is further divided in sub-districts. sub-districts are further divided into blocks that are the local arm of the bureaucracy: bureaucrats at the block level oversee the implementation of public good 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 at the sub-district level.

1.2.2 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 constituted 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 his-

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

tory, 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 mention a variety of procedural reasons such as ward not on the list of selected wards for

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

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.

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.

1.2.3 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 (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]).

1.3 Experimental Design

1.3.1 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 A1 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.

1.3.2 Intervention

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 3.1. 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%.

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.

1.3.3 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 1.1 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 1.1: Test of Randomization Balance for Complaint Filing Assistance Treatment

	(1)	(2)	(3)
Variable	Control	Treatment	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 A3

1.4 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.

1.4.1 Secondary Data Sources

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.

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.

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.

1.4.2 Primary Data

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

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.

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.

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.

1.5 Estimation Strategy and Results

1.5.1 Short-Run (3-Month)

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

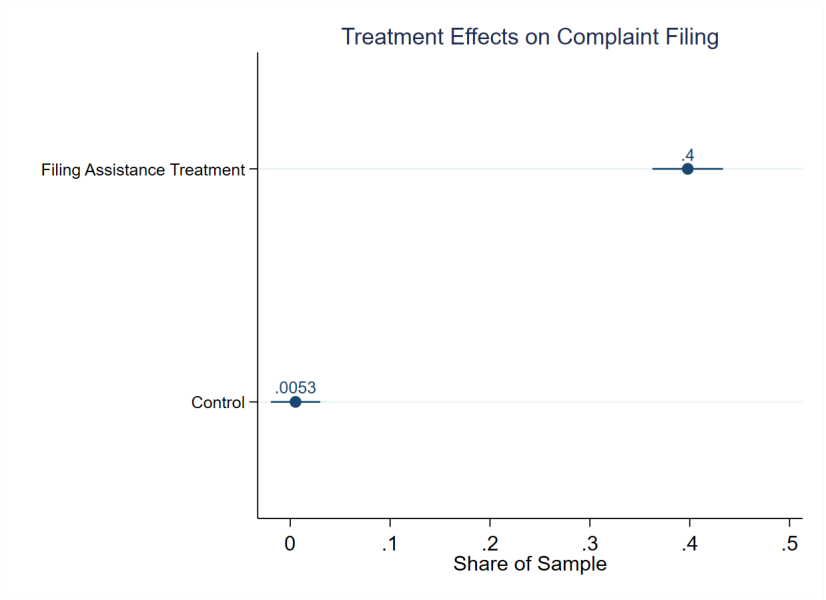


Figure 1.2: Impact on Complaint Filing Rates
This figure displays the impact of our main intervention arm – a complaint filing assistance treatment –on whether complaints are filed. This is the "first stage" of the experiment). Outcomes are measured as per administrative data on complaints.

the GP and ward-level.S indicates block fixed effects. T_{ig} is a dummy that takes the value 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 1.2) as per administrative data.[14] Thus, our treatment results in a strong first stage which should allow us to detect effects on downstream outcomes if they exist.

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. The impact of information-only treatment on complaint filing is much lower: it leads to only a 7 p.p. increase. This suggests that access to information is not a big constraint in the adoption of complaints technology. We discuss contraints to formal complaints technology adoption in Appendix 1.8.2

We now turn to impacts on projects being undertaken. We focus on two outcome variables from our 3-month endline survey[15] : (i) whether the problem preventing projects from starting had been resolved and (ii) whether projects had, consequently, started.

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 1.2, col (1)). Second, we find that our treatment improves project initiation by 7 p.p over a control mean of 27 p.p (Table 1.2, 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 A6 (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 A6.

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

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

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

15. Outcomes were pre-registered

Table 1.2: Effects of Complaint Filing on WAS Projects

	ITT Effects		**TOT Effects**	
(lr)2-3(lr)4-5	(1)	(2)	(3)	(4)
	Problem Solved	Project Initiated	Problem Solved	Project Initiated
Complaint Filing Assistance	0.105***	0.069**		
	(0.032)	(0.030)		
Complaint Filed			0.266***	0.177***
			(0.071)	(0.065)
Observations	1332	1332	1330	1330
Control Mean	.41	.27	.41	.27
Fixed Effects	Block	Block	Block	Block

Table delineates the impact of the complaint filing assistance treatment on our two main outcome variables. Column 1 presents the ITT effects of our treatment on whether the problem preventing the ward members from initiating projects was resolved. Column 2 presents the ITT effects on whether a project was initiated in the post-intervention period. Column 3 and Column 4 present the TOT effects where the treatment status of the ward members serves as an instrument for the actual complaint filing rates. The regression specification across both panels is our pre-specified estimating equation. All regressions contain GP-level controls and block fixed effects. Standard errors are robust to heteroskedasticity and reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Here, we assume that the impacts on project initiation come only from the individuals that filed complaints. As we can see in Table 1.2, the ToT effects are much bigger: our 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.

1.5.2 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.

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 A2).

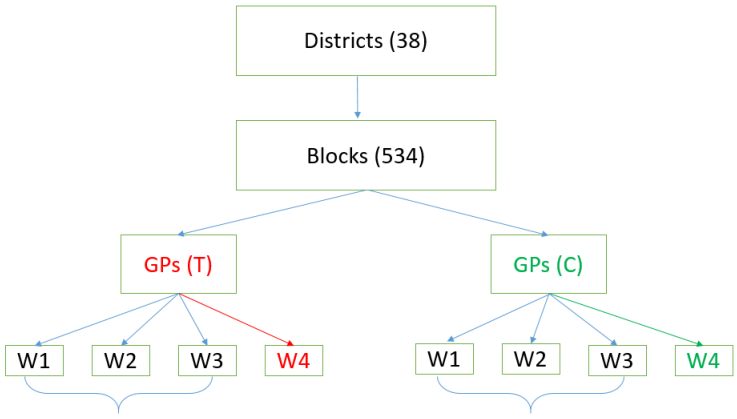


Figure 1.3: Within-GP SPillovers Estimation
This figure depicts our within-GP spillover estimation strategy. We restrict our analysis to GPs with either one treated or control ward. We compare untreated wards of GPs with one treated ward (marked in red) with GPs that have one control ward (in green).

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 that 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 1.3 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. ibyase compared to control where no one complains. The effects are also positive for private complaints but much smaller in magnitude.

Table 1.3: Spillover Effects on Complaint Filing

	Complaints Filed		
(lr)2-4	(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 spillover effects of the complaint filing assistance treatment on different types of complaints filed. We rely on administrative data for measuring complaint filing rates. Each column lists a different outcome. In column 1, we measure whether a WAS project-related complaint was filed by the representative of the neighboring wards. In column 2, we look at any complaints related to local public goods, including the complaints filed related to WAS projects. In column 3, we look at the effects on private complaints filed by ward members regarding the non-delivery of certain services ward members are entitled to as a citizen. All regressions contain GP-level controls. Standard errors are reported in parentheses $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

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 1.4 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 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 1.4, 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 1.5 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.

Table 1.4: Spillover Effects of Complaint Filing Assistance Treatment

(lr)2-3	(1)	(2)
	Complaint Filed	**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 $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

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 1.5: Heterogeneity in Spillover Effects by Ward Leaders' Caste

	PANEL A: Ward Member is Low-Caste	
(lr)2-3	(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	
(lr)2-3	(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 $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

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:

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

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

Table 1.6 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 1.4) 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 1.6: Spillovers at Block Level

(lr)2-2(lr)3-3(lr)4-4	Control Wards (1) **Project Initiated**	Treated Wards (2) **Project Initiated**	All Wards (3) **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 $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

1.5.3 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

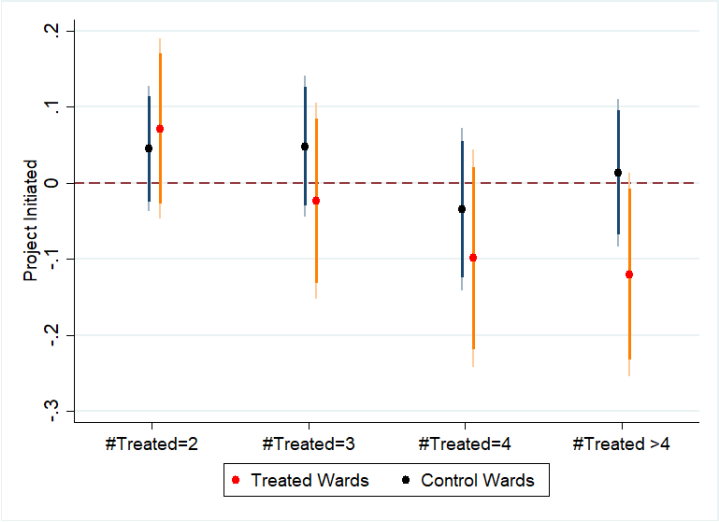


Figure 1.4: Impact of Block-Level Treatment Intensity on Treated Wards
This figure displays how the effects of block-level treatment intensity on treated wards vary across various levels of treatment intensity. The control group comprises of wards from blocks with only one treated ward. Y axis measures 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.

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 1.7. 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.

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

Table 1.7: Backlash Effects of Complaint Filing in the Short-Run

	ITT Effects		**TOT Effects**	
(lr)2-3(lr)4-5	(1)	(2)	(3)	(4)
	Officials Approached	Unfriendly Conversation	Officials Approached	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

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.

1.5.4 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 1.8 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 1.8: Effects of Complaint Filing on Reelection

	ITT Effects		**TOT Effects**	
(lr)2-3(lr)4-5	(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 1.9: Effects of Complaint Filing on Electoral Outcomes

(lr)2-4	(1)	(2)	(3)
	Win	**Vote Share**	**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 1.8, col 2). Likelihood of winning conditional on running continues to be negative and statistically insignificant (table 1.9). 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 weaken 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 the likelihood of complaints being filed but we still see an 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 in neighboring wards: unlike the main effects, the spillover effects on reelection probability are not negative. The treatment effects are extremely small and are not statistically significant (Table A4).

1.6 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 the 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 1.8.3.

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 A9). 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 experiment. 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:

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.

$$SCReserved_{gb} = \gamma_0 + \gamma_1 1(SCPop_{gb} > T_b) + \gamma_2(SCPop_{gb} - T_b) * 1(SCPop_{gb} \geq T_b) + \gamma_3 Treated_{igb} + \gamma_4 Treated_{igb} * 1(SCPop_{gb} > T_b) + \delta X_g + \psi + \eta_{gb}$$

$$Y_{igb} = \beta_0 + \beta_1 SCReserved_{gb} + \beta_2(SCPop_{gb} - T_b) * 1(SCPop_{gb} \geq T_b) + \beta_3 Treated_{igb} + \beta_4 Treated_{igb} * SCReserved_{gb} + \omega X_g + \alpha + \epsilon_{gb}$$

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 A9.

Table 1.10 shows the heterogeneous treatment effects on four main outcomes of interest: whether a complaint was filed (stage1), whether a project was initiated, whether received threats in the short run, and the likelihood of getting reelected. Given the 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 relatively (1)

less likely to file complaints in response to our treatment,(2) more likely to report project initiation, (3) less likely to receive threats from upper-tiered officials, and (4) 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. 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 1.10: Heterogeneous Treatment Effects: Does Identity of Upper-tiered Politicians Matter?

(lr)2-5	(1) **Complaint Filed**	(2) **Project Initiated**	(3) **Received Threats**	(4) **Reelection Probability**
Treatment*SCReserved	-0.084	0.047	-0.049	0.188
	(0.090)	(0.111)	(0.051)	(0.125)
SC Reserved	-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
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 the post-intervention 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 differential effects of our treatment for wards governed by low-caste (SC) representatives. These estimates are generated using fuzzy RD specifications described in the paper. We control for GP-level covariates and add Block-fixed effects. All standard errors are clustered at the Block level and reported in parentheses $*p < 0.1$, $**p < 0.05$, $***p < 0.01$

1.7 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 the 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 to public goods project implementation in their constituencies.

However, our study also reveals that the 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 seem 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 a 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 appear to be negative. This makes it important to study the dynamic consequences of complaint filing. If complaint filing reduces the reelection probability of politicians, it might dissuade politicians from using it in the future which could limit the effectiveness of complaint systems in the long run. Studying the dynamic consequences of complaints should be an important avenue for future research.

1.8 Appendix A.

Table A1: 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 $Prob > F = 0.00$.

Table A2: 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 A3: 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 A4: Effects of Treated Neighboring Wards on Reelection

	If Reelected			
(lr)2-5	(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 A5: Effects of Complaint Filing on Likelihood of Running For Higher Posts

	If Ran for Higher Posts			
(lr)2-5	(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 $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

1.8.1 Robustness Checks

Table A6: ITT Effects of Complaint Filing on WAS Projects: robustness

	PANEL A: Problem Solved			
(lr)2-5	(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			
(lr)2-5	(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 A8 (cols (2)-(4)).

Table A7: ITT Effects of Complaint Filing on WAS Projects: spillovers a concern?

	Whole Sample		Restricted Sample	
(lr)2-3(lr)4-5	(1)	(2)	(3)	(4)
	Problem Solved	**Project Initiated**	**Problem Solved**	**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 A8: Effects of Complaint Filing on Reelection

	If Reelected			
(lr)2-5	(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 A9: How do caste differences affect complaining rates?

	PANEL A: SC Lower-Tiered Representatives				
(lr)2-6	(1)	(2)	(3)	(4)	(5)
	WAS Pubic Goods	**Local Government**	**All Public Goods**	**Mention Ward**	**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				
(lr)2-6	(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 2.2 and 2.3).We control for GP-level covariates, ward-level covariates and add Block-fixed effects. All standard errors are clustered at the GP-level.

1.8.2 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 1.5). 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.

1.8.3 RD Framework

The state of Bihar is divided into 38 districts, which are further divided into 534 blocks and 8400 GPs. Within each block, the 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

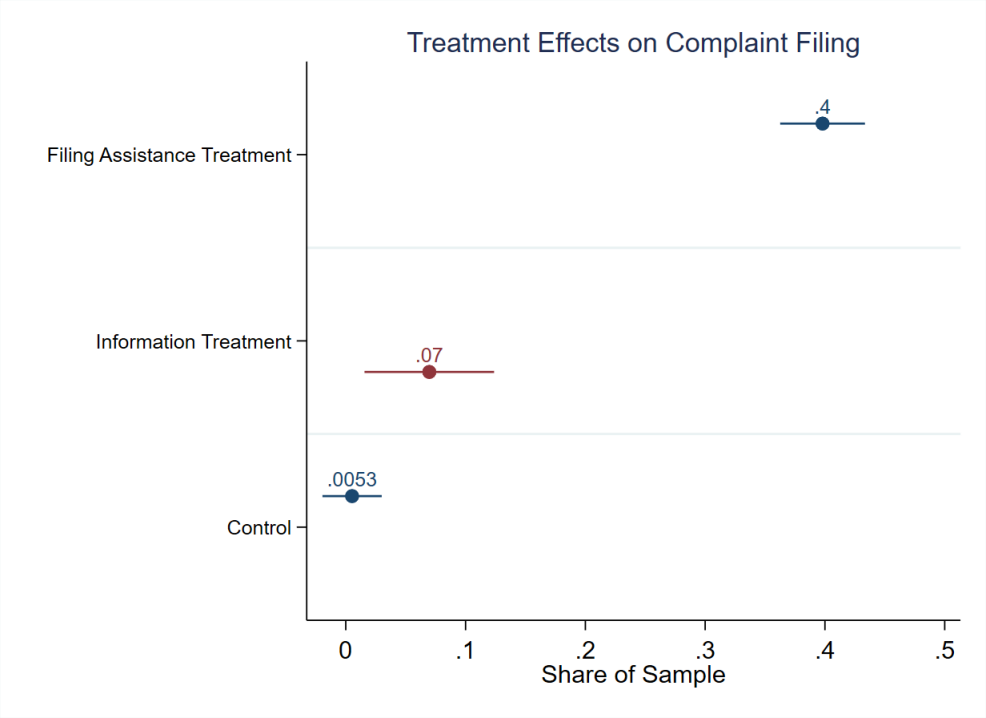


Figure 1.5: Information Treatment and Complaint Filing
This figure displays the impact of our two main intervention arms – a complaint filing assistance treatment and the information treatment on whether complaints are filed (this is the "first stage" of the experiment

the cutoff by carrying out McCray test and find that the density is reasonably smooth at the cutoff (see Figure 1.7).

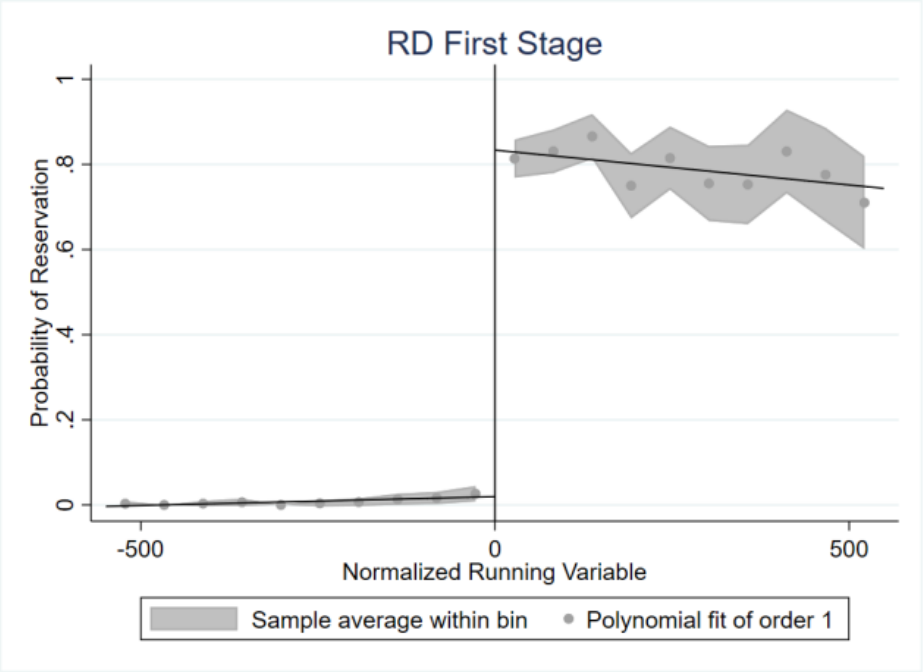


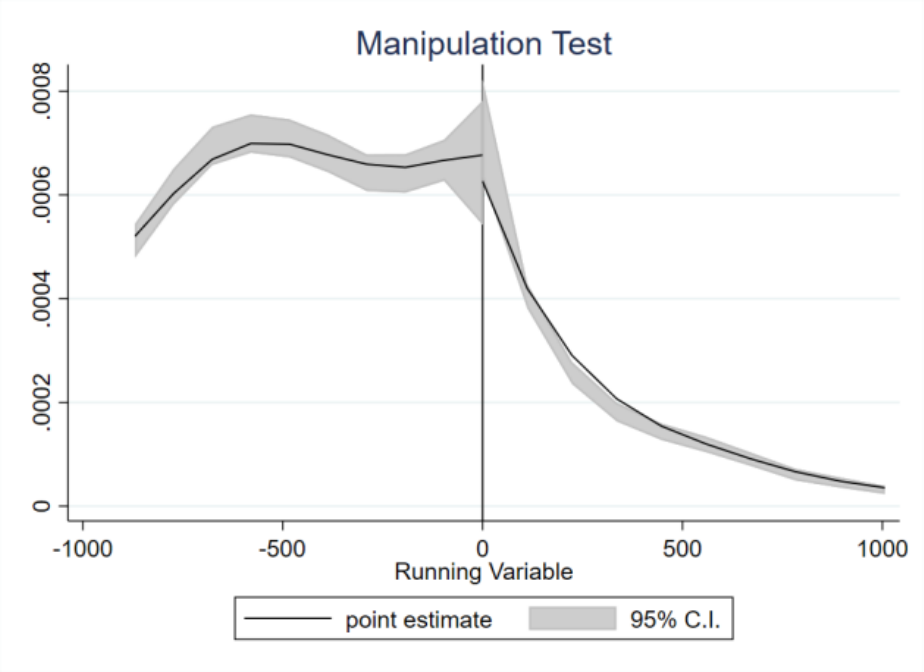
Figure 1.6: Probability of reservation based on the rank of a GP within a Block

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



(a) Mccrary Test

Figure 1.7: Mccrary Test for Manipulation

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.

CHAPTER 2

THE SHORT- AND LONG-RUN DISTRIBUTIONAL CONSEQUENCES OF POLITICAL RESERVATION

2.1 Introduction

Government across the world have implemented affirmative action policies to reduce inter-group disparities.[1] In this paper, we focus on one such policy: ethnic quotas in local government – or political "reservation" [Pande, 2003] [Duflo, 2005] – for members from socioeconomically disadvantaged caste groups in India. The primary aim of such policies, especially in democracies, is to ensure adequate representation of historically under-represented minorities in government. The implicit assumption here is that representation would socially and economically empower disadvantaged groups and, consequently, reduce inter-group inequality.[2]

A body of literature documents how reservation affects outcomes for marginalized castes in India (Pande [2003], Besley et al. [2004a]). Yet, gaps remain: First, rigorous evidence on the long-run welfare consequences of political reservation at the local level is lacking. Indeed, while there is some work documenting the long-run political empowerment of minorities, whether this coincides with material gains remains an open question. Second, the dynamic nature of relative welfare gains – do miniorities catch up with majorities in the short/long run? – and the theoretical trade-off between equity (reduction in inter-group disparities) and efficiency (reduction in overall welfare) consequences is under-explored. Finally, how

1. Many countries have such policies enshrined in their constitution. In India, the focus of this paper, the Supreme Court in *N. M Thomas vs State of Kerala* ruled that quotas for minorities in jobs is fundamental to furthering the constitution's view of equality.

2. For instance, [Pande, 2003] motivates the case for political reservation by noting that: "There are strong moral and *economic* arguments suggesting that it is in the interest of society to improve the economic standing of historically disadvantaged minority groups" [emphasis mine].; Similarly, Duflo [2005] states: "The reservation policy [in India] was expected to alter the distribution of public goods in favour of minority groups."

reservation's effects vary with the size and diversity of the minority group has received relatively less scrutiny than the measurement of its overall effects.

In this paper, we shed light on all three gaps in the literature delineated above. We study how reservation affects allocation a range of of resources – public goods, private assets and political posts, both across- *and* within-castes, over the short- *and* long-run. To do so, we brings to bear a wealth of data sources from Bihar in India – a census of nearly 20 million rural households, public goods from all its 45,000 villages, data on over 300,000 local political actors and a primary survey of 8748 households – and delineate impacts both on the targeted minority group and map out distributional effects across all households and villages. We also then ask: how does the effects of reservation vary by (a) size and (b) ethnic diversity of the minority group?

This paper focuses on reservation of posts for the post of village head in favour of Scheduled Castes (SCs). Bihar has over 8400 Gram Panchayats (GPs –"village councils"). GPs are run by an elected representative, the village head (locally called "Mukhiyas"). SCs are a collection of heterogeneous sub-castes who occupy the bottom rung of the caste hierarchy and have historically been severely discriminated against. SCs continue to lag behind other caste groups on a host of socioeconomic indicators [Deshpande, 2011]. After a delay of two decades caused by opposition by powerful non-SC caste groups, a legislation was finally passed approving 17 % reservation of GP head posts for SCs.

Our empirical strategy exploits the algorithm used to reserve village head posts for Scheduled Castes (SCs). Within each block (a collection of 15.7 GPs), this population-based rule mandates that GPs with SC populations above a threshold will only have an SC village head. In practice, GPs just above the population threshold are 80 percentage points (p.p) likelier to be reserved than those marginally below. By focusing on outcomes from GPs on either side of the threshold, we can causally measure the impact of SC reservation using a fuzzy

regression discontinuity design (RD) framework.[3]

Bihar reserved seats for two consecutive election cycles. A GP reserved in 2006 had, by law, an SC head from 2006 - 2016. The elected head may or may not have been voted out in reserved GPs in the 2011 elections, but their replacement would, by law, be another SC person. To measure short-run impacts, we collect village-level public good data from 2011, politician characteristics from 2011 and household-level private assets from 2012. For the long-run, we collect politician characteristics from 2016, public goods from 2016-18 and private assets from 2018-19.

We draw upon previous literature and our field insights to outline a framework for how SC GP heads employ their policy levers to influence asset accumulation in the short- and long-run and political participation in the long-run. Asset accumulation, we argue, is influenced by better targeting of welfare programs and public goods. Political participation is both driven by better material well-being and helps sustain long-run asset accumulation. Our framework structures our analysis, guiding our choice of outcome variables and interpretation of results.

Our first set of results show that SC reservation reduces private asset inequality between SCs and non-SCs, modestly in the short-run and substantially in the long-run. We employ asset data from a state-wide census of *all* rural households conducted in 2011-12 (the Socioeconomic Caste Census) to calculate asset scores. We show that SC reservation reduces the difference in the average asset scores between SCs and non-SCs by 0.08 SD. This is driven partly by an *increase* (statistically insignificant) in mean SC asset scores and a *decrease* in non-SC asset scores (also statistically insignificant).[4] We find no evidence of an equity-efficiency trade-off: overall, reservation has no impact on the mean or median asset score of

3. In practice, SC reserved GPs close to the threshold are likelier to *not* be reserved for women: hence, in some sense, the RD is more likely measuring the impact of having a male SC village head vs a non-SC village head (who is female about 40% of the time). All our regressions control for the gender reservation status of GPs.

4. Throughout this paper, we drop Scheduled Tribes (STs) from our analysis. They comprise only 1.6% of households in the SECC data.

a household in the GP.

How is within-group inequality in asset wealth affected? We are powered to see effects in the short-run. For each SC quartile, we measure catch-up with the median non-SC quintile. The estimate of the impact of reservation on the mean asset score is positive across all quartiles, but significantly so only for the 3rd (0.1 SD, p=0.069) and 4th quartiles (0.13 SD, $p = 0.027$). Households in the top decile of the SC distribution are 0.18 SD ($p < 0.01$) better off in reserved GPs than their counterparts in unreserved GPs. This, then, suggests a slight increase in inequality within SCs.

Who among SCs benefits in reserved GPs? First, we show that the "dominant caste" [Srinivas et al., 1955] within SCs, as defined by the numerically largest sub-caste, does not benefit more than the average SC household. We proxy sub-castes by surnames. Second, we show that Mahadalits - the weakest SC sub-castes - are not differentially affected by reservation either. Among non-SCs, however, the dominant sub-caste is *worse* off (-0.07 SD, $p = 0.128$), suggesting a reduction in elite dominance.

Consistent with the literature on clientelism and coethnic favoritism, we show that reservation brings more benefits to those who are "close" to the elected village head, either owing to sharing their surname (which we use as a proxy for subcaste) or living in proximity to them. To do so, we first track down the village head's household in the SECC data, matching on head-level demographics (caste, sub-caste, village, name, father/husband name). We are able to trace 61% of SC village heads. We then show that those *close* to the head are relatively better off.[5] Having the same surname as the village head is associated with a 0.33 SD increase in relative wealth scores of SCs; the commensurate figure is 0.86 SD for households within a 20-household distance of the village head and 1.47 SD for the village head's own household.[6]

5. These results are not strictly causal as we discuss in section 2.6.1.

6. This last result points to the notion of "self-dealing" by village heads: see [Besley et al., 2012] and [Jeong et al., 2021].

In order to measure impacts in the long-run, we rely on a primary survey we conducted of 8748 households across 107 GPs in 2019. Our “treated" GPs would have had exactly ten years (out of 13) of SC reservation and our “control" GPs would have had at most three years of SC reservation. Our primary survey tracks all but one of the private assets used to calculate the asset score from the 2012 census data.[7] We replicate the procedure to create asset indices and find that political reservation results in further catch-up between SCs and non-SCs. Despite the small sample, our point estimates on the reduction in difference in asset scores between the two groups (1.12 SD improvement) are large and significant and robust to a range of bandwidths and other robustness checks. The lower end of the 99% confidence interval shows a 0.45 SD improvement in SCs' relative asset scores.

Our second set of results show that political reservation improves public good access for SCs in the short- and long-run. Using population data from the decennial Census of India (2001), we identify the main SC village in the GP as the one where the most SCs live. To determine public good provision, we bring to bear data from the Census Village Amenities List (2011). We focus on 4 key public goods that a survey of village heads from 22 districts of Bihar revealed as the most important: construction of government primary schools, creation of functional ration shops, construction of roads and building child nutritional centres (Anganwadi centres). Following Duflo et al. [2005], we calculate the population normalized share of these public goods accruing to the main SC village in reserved and unreserved GPs. This share increases by 0.2 SD in reserved GPs. We do not find any evidence of an equity-efficiency trade-off: the availability of public goods at the GP-level remains unaffected.

For long-run public good outcomes, we focus on a set of key water and sanitation (WAS) schemes that were launched after 2016, when the reservation cycle had switched. Here, we find that *past* reservation reduces delays in WAS provision by 0.17 SDs.

7. We do not collect data on phone ownership, but this was already 82% in 2012 and, as per the government, Bihar added another 20 million phones between 2014 and 2020. Thus, we expect phone ownership to be near-universal in the state.

Our third finding is that reservation at the GP-level increases political participation of SCs in local government in the long-run. We begin by showing that SC village heads continue to win elections even in the absence of reservation. Many GPs that narrowly missed out on being reserved in 2006 are reserved in 2016. Hence we drop these in our analysis and restrict attention to GPs that are currently unreserved in 2016-2021 cycle. In other words, we compare GPs that were reserved in 2006 with GPs that were never reserved. We find that even in the absence of reservation, formerly reserved GPs are six times as likely to have an SC village head than GPs that were never reserved.

Reservation also improves political participation at lower tiers of government. Each GP comprises 13.6 wards on average. Bihar has a robust electoral system at the ward level. Reverting to our full sample, we find that having an SC village head for 10 years causes a 30% increase in the number of SC winners from unreserved wards in the ward elections.[8]

Turning to mechanisms, we find, consistent with Besley et al. [2004a], greater targeting of government schemes towards SCs in reserved GPs. We look at work-days provided under the National Rural Employment Guarantee Act (NREGA) in the last two years of the reservation cycle, i.e April 1st 2014 - March 31st 2016. We find that the population normalized share of households receiving 100 days of work increases by 0.17 SD. Second, we turn to house construction under the Pradhan Mantri Awas Yojana (PMAY). The data for PMAY is available only for years after 2016, when the reservation cycle switched. This, however, works to our advantage: at least some of the impacts we see could be attributed to the persistence of reservation's impacts even after the cycle is switched. We look at cumulative number of houses constructed up to 2019, the year our primary household survey measuring long-run effects was conducted. We find that the population normalized share of houses constructed for SCs rises by 0.13 SD. Quality of the house is a key component of our asset index, so this result is consistent with the increase in asset scores for SCs in the long-run. A

8. The number of winners from SC-reserved wards stays the same. This is trivially the case since the number of wards does not jump across the RD threshold.

potential channel that mediates impacts is changes to SCs' labour market outcomes. While we do not have data on wages, we show, using data from the SECC, that SC households are more likely to report their primary source of income as being derived from cultivation (as opposed to manual casual labour).

We then show that politically active SC citizens in the GP are much likelier to articulate their demands and make claims on the state via a formal complaints system that was launched *after* the reservation cycle was switched. This could explain part of the reservation policy's ability to channel gains in the long-run. Finally, we reject the mechanism that reservation works because SC leaders elected in 2006 are likelier to return in 2011. In fact, we find that reserved GPs show more electoral churn. Thus, even if the effects of the policy are being driven by reservation being fixed for two terms, it is not because the same set of leaders are being elected in reserved GPs.

We then ask: where does reservation work best? We exploit the uniqueness of our pooled RD design to answer this. We have 525 separate SC population threholds, one within each block. We divide blocks into 3 groups based on the SC population thresholds and estimate separate treatment effects within each. This allows us to ask how the effects of reservation vary at different levels of SC GP population.

We show that reservation works best when SCs are neither too small nor too large in number. When SCs are small in number, reservation brings material gains and access to public goods. However, the lack of a set of own-caste core voters makes long-run political power hard to achieve. When SCs are large in number, reservation does not bring in additional material gains for SCs. This is perhaps because, even in unreserved GPs, (non-SC) GP heads cannot ignore SCs entirely. When SCs are neither too small nor too large, reservation brings in benefits while in place. Moreover, SCs are sufficiently large as a group to ensure SC heads are voted into power even in the absence of reservation.

Finally, there is a vast literature that argues that ethnic divisions worsen public good

provision [Alesina et al., 1999]. We test to see if the effects of reservation vary by the ehtnic diversity within SCs. Our measure of ethnic differences is sub-caste variation within SCs.[9] Once again, we group blocks into 3 terciles, but this time based on the herfindahl index of SC sub-castes. Our results indicate that, in line with the literature, reservation is most effective in targeting benefits towards SCs when SCs are homogeneous in nature.[10]

This paper contributes to the literature on impacts affirmative action policies in the political arena in four different ways. First, while there has been work measuring the longer-term impacts of reservation at higher state legislator levels (Jensenius [2015], Pande [2003], Gulzar et al. [2020], Chin and Prakash [2011]), the literature on impacts at the local level tends to focus largely on the short-run effects (Besley et al. [2004a], Duflo et al. [2005], Das et al. [2017], Dunning and Nilekani [2013]). Both these sets of papers, with some notable exceptions, find mostly modest effects. We show that *long-run* effects of reservation at the *local* level shows relatively large impacts, a finding that is in line with a small number of papers showing that affirmative action policies are more effective after two terms (Beaman et al. [2009], Deininger et al. [2015], Bardhan et al. [2010a]).[11]

Second, the literature typically documents impacts of affirmative action policies on the minority group (SCs in this instance) and, less frequently, on the majority group (non-SCs here). This paper, owing to the presence of census data from the universe of Bihar's near-20 million rural households, delineates impacts *within* groups. In other words, we provide evidence on who – or which sub-castes – among SCs and non-SCs benefit. We also

9. As before, we proxy for sub-caste by surnames.

10. Unlike the result on variation in treatment effects based on SC population of GPs, which focuses on breaking down a pooled treatment effect by its components, the estimates here are picking up more conventional heterogeneous treatment effects by sub-caste diversity.

11. A close pair of papers is [Bardhan et al., 2010a] and Bardhan et al. [2010b], which examine the effects of reservation for SCs at the GP level in the neighbouring state of West Bengal. While Bardhan et al. [2010b] finds overall negative impacts of SC/ST reservation in the very short-run (1 year after reservations were introduced), [Bardhan et al., 2010a] argues that the impact of SC-reservation results in better targeting of benefits to SC/ST households 5-7 years after reservation is in place. Another related paper is [Afridi et al., 2017], which shows that women leaders in reserved seats perform initially worse than their male counterparts in implementing the employment guarantee scheme, but eventually catch up.

document the ex-post asset wealth distribution curves across *all* SCs and non-SCs. These questions assume considerable importance because minority groups across the world are rarely homogeneous. This is even more true in our context because, as we discuss in section 2.2, broad caste groupings mask considerable heterogeneity.

Third, we also contribute to the literature by studying how the impact of quotas varies by underlying group characteristics. There is a literature that focuses on the relationship between group size/diversity and welfare outcomes (e.g Alesina et al. [1999], Banerjee and Pande [2007]). However, papers focusing on how affirmative action policies interact with size and diversity of the minority group are relatively uncommon [Munshi and Rosenzweig, 2008].[12] We exploit the uniqueness of our reservation algorithm to shed light on this question and show that the effects vary considerably based on underlying size (and diversity) of the minority group.

Finally, while some papers show the effects of affirmative action on material well-being, access to public goods or political empowerment, this paper shows how these could all be connected. The nascent literature on political impacts of affirmative action – especially political reservation – has found positive long-run effects for women (Beaman et al. [2009], Bhavnani [2009], Deininger et al. [2015]), but no effects for SC reservation on higher state political posts (Bhavnani [2017])[13]. Indeed, while causal attribution is difficult, our paper strongly suggests that over the long run, initial political empowerment through quotas catalyses welfare gains for the minority group which further spurs political empowerment and material well-being.

This paper has implications on the design of affirmative action policies: first, the paper

12. The one exception at the local level is [Das et al., 2017], who document that affirmative action works best when group sizes are skewed and quotas are established in favour of the larger group. In our context, SCs are almost never over 50% of the population - which is the median case in their setting. We also focus on a wide range of outcomes and over longer time horizons.

13. See also Auerbach and Ziegfeld [2020] for a comprehensive discussion on contemporaneous effects of political reservation across multiple tiers of government in India.

suggests that the true effects of reservation may be realised over longer time horizons. Second, given the heterogeneous effects within broad caste categories, creating safety nets in favour of those who lose out or are left behind remains important. For instance, Bihar's specific policies targeted towards extremely disadvantaged sub-castes within SCs, called Mahadalits, may play an important, independent role in redistributing benefits. Third, the heterogeneous impacts of such policies by size of minority groups has implications for how SC reservation policies are designed in India. In particular, those areas where SCs are either a small minority (e.g Gujarat) or where they are in large numbers (e.g Punjab) may have to implement complementary policies to ensure the gains from reservation are suitably achieved.

The popular discourse in India on reservations characterizes these as policies that either prove empowering for minorities or inefficient and ineffective, benefiting only an undeserving elite among the targeted group. This paper, in the tradition of empirical works challenging these facile binaries (Chauchard [2017], Chattopadhyay and Duflo [2004]), argues that the impacts can be quite complex. The answers depend on types of outcomes evaluated, their time-horizons and the nature of the comparisons being made.

2.2 Context

Historical Roots of Caste

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* (or the "Laws of Manu") - a text written around the dawn of the common era. The text, *inter*

alia, classified society into for broad hierarchical groups[14] that subsumed the thousands of sub-castes that constituted Indian society. The text prescribed strict rules for engagement between classes and castes, codified discriminatory practices by specifying punishments for rule violations and crystallized hierarchical norms. Lower castes and upper-castes were forbidden from dining together. Inter-marrying across castes continues to be rare in modern Indian society. The more egregious practices include notions of "pollution" emanating from contact with lower-castes, including the slightest touch with even their shadows. Modern India's first (and greatest) scholar of caste, Dr B.R. Ambedkar described the *Manusmriti* thus: "There is no code of laws more infamous regarding social rights than the Laws of Manu. Any instance from anywhere of social injustice must pale before it." (Ambedkar [1936]).

The broad caste groupings - Scheduled Castes (SCs), Scheduled Tribes (STs), Other Backward Castes (OBCs) - are not in the least homogeneous, comprising within them thousands of sub-castes and hierarchies. Indeed, the broader classifications are somewhat arbitrary and there is considerable differences within each group.

2.2.1 Bihar's SCs

Bihar is arguably India's poorest state, with a population of over 130 million. Over 85% of Bihar lives in villages. SCs comprise 17% of Bihar. Historically SCs 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]. Summarizing the literature from the two-decades leading up to 2012 and looking specifically at material well-being across castes, [Deshpande, 2011] argues that while there exists substantial regional variation, there is no "reversal of traditional caste hierarchies".

14. These four groups, ranked by hierarchy, were the Brahmins (priests), the Kshatriyas (warriors), the Vaishyas (traders) and Shudras (workers and farmers).

Caste-Barriers in India/Bihar today

Caste barriers continue to persist in India today, a fact rigorously documented across several social science disciplines, including economics. A mere 11 % of marriages in Bihar, the setting for our study, are inter-caste. On the other hand, 47 % of respondents surveyed say that someone in their household practices untouchability [Desai and Vanneman, 2015]. Caste-barriers continue to dictate labor-market outcomes (Deshpande [2011], Singh and Thorat [2014]) and labor-market opportunities, with resume-studies confirming the presence of discrimination, even in urban India [Thorat and Newman, 2007]; caste-networks are seen as barriers to rural-urban migration [Munshi and Rosenzweig, 2016]. [Lowe, 2018] presents evidence of considerable prejudice among youths towards non-caste matched peers and rigorously documents discrimination against lower-caste members.

2.2.2 Local Administrative Structure

Bihar's villages 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". Each GP is divided into wards. There are over 114000 wards in Bihar. Each ward is headed by an elected ward member. The year 2006 marked the beginning of political reservations for disadvantaged groups and women.

This considerably changed the composition of the new cohort of Mukhiyas. In 2001, when there was no reservation, roughly 1 % of Mukhiyas were SCs [Gupta, 2002]. This number went up to nearly 17 % in 2006.

Bihar's path to political reservation for SCs across Gram Panchayats was anything but smooth, featuring a series of false dawns and fiery challenges, often playing out in the court of law. In the Appendix section **??**, we summarize the three attempts made by incumbent state governments to introduce political reservation and the challenges thrown by powerful non-SC caste groups (who stood to lose from reservations for low castes).

2.3 Data Sources

This project brings together multiple secondary data sources and one primary survey.

2.3.1 Explanatory Variables

From the State Election Commission in Bihar, we collected data on reserved and unreserved Panchayats and characteristics of village heads elected in 2006 (N = 8380, 99.7%), 2011 (N = 3489; 41.5%) and 2016 (N = 7736; 92.1%). We also have data on ward members and ward candidates from the 2016 elections (N = 96754; 84.9%).

Second, we collect data on census village characteristics using Census of India's Village Amenities Surveys of 2011. This contains indicators related to size, demographics and geography of these villages.

From the state election commission (SEC) in Bihar, we collected data on GP populations: total population, number of SCs. We use this to create our main running variable.

Our main treatment variable is whether a village was reserved for SCs. This data is also obtained from the SEC in Bihar.

2.3.2 Primary Outcomes

Public Goods (Short Run): The Census Village Amenities list provites us with the availability of various types of public goods in villages in reserved and unreserved GPs. The data is available at the village level. The dataset contains 45,000 villages across 8392 GPs.

Private Assets (Short Run): Third, we use the Socioeconomic Caste Census (SECC). This survey from 2011-12 covered all rural households - nearly 20 million - of Bihar. The survey allows us to create our main asset and caste indicators. At the within-household level, the survey contains basic information on members of the household including gender, broad caste category, age, type of occupation and education status. At the household level,

the dataset contains information on the following: type of dwelling including number of rooms, characteristics of wall and roof; employment and income characteristics including whether household has a member having a government job and main source of household income; asset ownership (vehicle, fridge, mechanical agricultural equipment etc); details on land-owned.

Public Goods (Long Run): Fourth, we collect data on the universe of 98,000 local government-constructed water and sanitation (WAS) projects constructed under a scheme launched in 2016. The main types of projects are (a) piped water to households and (b) lanes and drains. These WAS public goods are described and studied in detail by [Sharan and Kumar, 2021].

Here, we mention a few important features of these goods: first, these WAS projects were supposed to prioritize wards headed by SCs in the GP. Thus, our measure of targeting of public goods in the long-run is the share of SC headed wards that had projects implemented two years after the scheme was launched. Ideally, we would have wanted a measure of the same public goods seen in the short run, but Census 2021 data is being collected still. The advantages of the WAS public goods are that they are specifically meant to be targeted towards SCs in the early years. If SC wards see fewer projects, that is an indication of rule-breaking. Rule-breaking by GP heads is easier when they face lesser opposition to their actions, i.e. when SCs are a smaller share of the population.

Private Assets (Long Run): Finally, in 2019-20, we collected primary data on our asset index indicators covered under the SECC across 8748 households across 107 GPs in Bihar. Our main set of GPs come from sampled pairs close to the block RD threshold, one on either side. Of the reserved GPs, we picked the *nth* GP to be reserved (the last reserved GP). Of the unreserved GPs, we sampled the next GP in the list i.e the *n+1th* GP. This GP would have been reserved in a counterfactual world where *n+1* GPs were to be reserved in the block. We only selected those blocks where (a) the reservation rule was properly

implemented (491 of the 534 blocks) and (b) the *nth* GP and the *n+1th* GP both fell within a bandwidth of 50 from the RD threshold. We surveyed 3811 households from 22 blocks with 43 GPs.[15] In addition to these, the data also has an additional set of GP pairs (from these and other blocks), which are sampled from around the 2016 RD threshold. This accounts for the additional 64 GPs.[16] While households were randomly sampled from two villages in every GP, SCs were over-sampled: our final sample comprises 38% SCs (across GPs whose proportion of SCs in 2001 was 18.5%).

2.3.3 Secondary/Mechanism-based Outcomes

NREGA data: From the government website, We scraped person-days generated under the Mahatma Gandhi National Rural Employment Guarantee Scheme (more commonly known as the NREGA) for Bihar for the years 2014-16. We have two main outcome variables: (a) the population-normalized share of persondays accruing to SC households and (b) the population-normalized share of SC households receiving 100 days of work in a year. The latter is important because households that receive 100 days of work make substantial amounts, often enough to save to purchase assets.

PMAY Data: We collect GP-caste level aggregate data on houses constructed under the main housing scheme operated via the local government, the Pradhan Mantri Awas Yojana (PMAY). Our housing data comes for the years 2016-2019. We create two outcome variables: (a) the number of houses constructed per SC person in the GP (b) the population-normalized share of SC-houses constructed.

Complaints data: we collect data on nearly 550,000 complaints filed by individuals under Bihar's grievance redressal system for the period from 2016-19. This system is described in detail in [Sharan and Kumar, 2021].

15. One GP-pair couldn't be completed.

16. Of these 64 GPs, we were unable to interview SCs in 7 GPs, so we drop these from our sample.

2.4 Framework

The main policy levers GP heads wield are (i) construction of village-level public goods and (ii) household-level welfare programs. Drawing on the literature and our field insights, we discuss below how SC GP heads could employ these levers to change patterns of asset accumulation in the short- and long-run. We also discuss the channels through which reservation affects long-run political participation and how political participation, in turn, interacts with asset gains.

How could reservation affect SC asset ownership in the short-run? One reason is via better targeted welfare schemes. The three main welfare programmes that GP heads could influence are: the subsidized ration scheme (the PDS), the NREGA and the housing scheme (IAY). Better targeting of the IAY scheme directly affects our asset index, which takes into account the quality of the house citizens live in. The NREGA has a more indirect impact on our index. It can increase household savings which can then cause the purchase of assets like phones or the construction of an additional room in the house. This is particularly the case if households get their full quota of 100 days of work in the year. [Muralidharan et al., 2021a] show that improved NREGA has significant impacts on assets like livestock, not just on beneficiary households, but also neighbours. In line with the above observations, we not only test for impacts on asset ownership across SCs and non-SCs in the short run (section 2.6.1), but also show impacts on NREGA outcomes (in the medium-run; section 2.8.1) and the IAY scheme (in the long-run; section 2.8.1).

A different policy channel that impacts asset ownership of SCs is via improved access to village-level public goods. Interviews with GP heads active between 2006 and 2016 indicate that there were four main public goods they could exercise discretion over. These were: construction of roads, primary schools, child-care centres and PDS shops. We use data from the Census of India (2011) and create a public goods index based on these and test impacts in the short run (section 2.6.2). Recent work has suggested that the effects of roads on incomes

or asset ownership in the short-run may be small (e.g Asher and Novosad [2020]). However, the long-run impacts of public goods like primary schools on incomes is well-documented [Duflo, 2001]. The presence of a PDS shop in a village[17] has a more immediate impact on incomes: access to subsidized rations could free up income for asset accumulation.[18]

A third channel through which SC asset wealth could improve, both in the short- and long-run, is through changing labour market outcomes. This could be improved either by an increase in wages or even changes in occupational patterns. Better access to land (a component of our asset index) could move SCs from being manual casual workers to cultivators. Even though large scale land reforms were not implemented during the study period, Bihar did introduce - and patchily implement - a series of land-related schemes in favour of SCs [Kumar and Somanathan, 2016]. We do not have data on wages, but we do test for changes in occupational patterns and land ownership among SCs relative to non-SCs (section 2.8.2).[19]

Previous work indicates that some SCs will benefit more than others as a consequence of reservation; and, similarly, some non-SCs may lose out more than others. In particular, the rich literature on clientelism and self-dealing predicts that, among SCs, those closer to the GP head – i.e. those from the same sub-caste or those who share geographical proximity – should gain more than others (Besley et al. [2004a]; Besley et al. [2012]; Mookherjee [2015]). Consequently, those who lose out are those who would have controlled local government in the absence of reservation, typically members of the numerically dominant powerful non-SC sub-castes [Bardhan and Mookherjee, 2006].[20] In line with this, we document effects on

17. The PDS is targeted at households and is therefore, different in flavour from our other public goods - which are targeted more broadly. However, we do not have PDS beneficiary lists from prior to 2011. We only have the location of the PDS shop, which, we therefore take as a proxy for where the main beneficiaries come from.

18. A more indirect channel through which public good location operates is in the impact it has on changing the spatial nature of public activity within a village, which, in turn, affects the network structure of villages with implications for credit and trade.

19. Needless to say, there are several other channels - that we cannot test for - through which reservation could affect asset accumulation. For example, better access to credit [Ao and Chatterjee, 2018].

20. Most of the empirical work is focused on the short-run, but theoretically, it is unclear why these

those who are in close proximity to the winning GP head and for the numerically dominant non-SC sub-caste (section 2.6.1).

We turn to the link between reservation and political participation of SCs in the long-run. First, [Beaman et al., 2009] show that GPs in West Bengal that had female leaders for ten years saw increased political participation of women, an effect driven by changes in perceptions among voters regarding suitability of women leaders. The fact that Bihar mandated that GPs be reserved for two consecutive terms suggests a similar effect could be at play even here. Another way reservation could change political participation of SCs is through improved material well-being of SCs. Political entry, even at the GP head level, is correlated strongly with household wealth. Only those who can afford to incur the upfront costs of campaigning in a very competitive political environment contest. Indeed, a candidate for GP head has a less than 10% chance of winning the elections. Therefore, any positive shock to a community's well-being is likely to result in more political entry from that group.[21] While we do report the effects of reservation on political participation (section 2.7.3), we can only provide suggestive evidence to delineate between these mechanisms. [22]

When will reservation reap benefits even after it is withdrawn?[23] First, benefits could accrue because of the stickiness of policies put in place by SC heads: put simply, PDS shops and primary schools cannot be demolished or moved very easily. Second, if wealth gains made during the period of reservation allow poor SCs to escape poverty traps (Ghatak [2015]; Balboni et al. [2022]), households could be pushed into a path of wealth accumulation.

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predictions will not hold even across two terms.

21. Political entry, even after reservation, may also vary with size of the vote-bank, which translates to the number of SCs in the GP or the size of the main SC sub-caste.

22. In sections 2.10 and 2.9, we do test for where reservation is most effective, based on size of SC population of GP and intra-SC sub-caste heterogeneity: here, we also discuss if GPs that have higher wealth accumulation in the short-run also show greater political participation.

23. A mechanical reason: measuring cross-sectional poverty after reservation has been withdrawn will not tell us *when* the gains were made. It could be that a significant share of the asset improvements in the long run among SC households could also have been made when reservation was still in place.

A third reason for material gains to sustain in the long run is if reservation could cause political empowerment that sustains material well-being. As long as a pool of SC GP heads and SC ward members are elected in unreserved areas as a direct consequence of past reservation, they will continue to push policies that favour SCs and sustain wealth gains. Moreover, even if SCs do not win, having a more politically active SC citizenry with voice could result in improved claim-making on the state [Kruks-Wisner, 2018] and sustain asset accumulation. In this paper, we test if gains persist beyond the period of reservation (sections 2.7.1; 2.7.2) and, also, if politically active SCs make more claims on the state in historically reserved GPs under a newly launched citizens' complaints system (section 2.8.4).[24]

2.5 Empirical Strategy

Bihar has 38 districts, which are further divided into 534 blocks and 8400 GPs. Within each block, the number of GPs, say N to be reserved are determined based on (a) the total number of GPs in the block and (b) the share of SCs in that block. Once N is determined, GPs in the block are arranged in descending order of SC population and the top N GPs are reserved. Thus, within each block, the rule for reservation gives rise to an exogenous SC population cut-off below which no GP is reserved.[25]

Above this threshold, not all GPs are reserved for SCs, as some are blocked to be reserved

24. Higher complaint-filing could also be interpreted to mean that SC households now face a larger set of problems. We believe that this may not be the case here. SCs across the board are likely to have a host of unaddressed grievances. If anything, owing to past reservation, SCs in previously reserved GPs should have lower number of grievances. The fact that politically active SC citizens complain more therefore suggests greater reporting of grievances than a greater number of actual grievances.

25. Chauchard [2014] documents the SC reservation rule for Rajasthan. While the rule for Bihar is very similar, there are two broad ways in which it differs: first, within each block (Panchayat Samiti in Rajasthan), GPs are arranged in order of SC population in Bihar, as opposed to SC proportion in Rajasthan. Once arranged, the top X GPs in both contexts are reserved. X is decided based on the proportion of SCs in the block (Panchayat Samiti in Rajasthan). Focusing on GP population of SCs as opposed to GP proportion of SCs makes Bihar's algorithm somewhat harder to manipulate (proportions are sometimes subject to rounding errors, as [Chauchard, 2014] notes). A second difference pertains to gender reservation: [Chauchard, 2014] argues that gender reservation in Rajasthan is randomly assigned, whereas in Bihar, SC reserved GPs are ranked in order of SC population and the higher half of GPs in the list are assigned gender reservation.

for Other Backward Classes (OBCs). In practice, as Figure 2.1b shows, the first stage results in a near 80 % jump in the probability of reservation[26]. We have a fuzzy RD with a strong first stage.

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

$$Running_{ij} = SCPop_{ij} - (\frac{SCPop_{1j} + SCPop_{0j}}{2}) \tag{2.1}$$

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.

Following Calonico et al. [2019], we estimate a fuzzy RD with covariates. Essentially, our primary specification uses a local linear regression within the Calonico-Cattaneo-Titiunik (CCT) triangular bandwidth[27] of the treatment threshold, and controls for the running variable (SC population in the GP) and a small number of covariates, in addition to block fixed effects, on either side of the threshold. Block fixed effects are useful since we have a different threshold for each block. We use the following two-stage instrumental variables specification:

26. In addition, the reservation rule was not perfectly implemented – about 1.5 % of the GPs (including 33 SC reserved GPs) are to be reserved as per our reproduction of the algorithm, but are not reserved as per election commission records. We asked election officials serving at the time about this and were told this may have been because of the following reasons: officers calculating the cut-off wrongly; disputes regarding actual SC population figures; manipulation by local officials of the status of reservation of GPs. At least one instance of manipulation was flagged and officials punished. Our results go through irrespective of whether we keep or drop these GPs. See

27. For estimating long-run effects of reservation on private assets, we fix the bandwidth to be 150, because we are working with a sample of GPs that were explicitly chosen from around the RD threshold. Section 2.7.1 has more.

$$Reserved_{gb} = \gamma_0 + \gamma_1 1(SCPop_{gb} > T_b) + \gamma_2(SCPop_{gb} - T_b) + \\ \gamma_3(SCPop_{gb} - T) * 1(SCPop_{gb} >= T_b) + \delta * X_g + \psi + \eta_{gb} \tag{2.2}$$

$$Y_{gb} = \beta_0 + \beta_1 Reserved_{gb} + \beta_2(SCPop_{gb} - T_b) + \\ \beta_3(SCPop_{gb} - T) * 1(SCPop_{gb} >= T_b) + \omega * X_g + \alpha + \epsilon_{gb} \tag{2.3}$$

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 *psi* indicates block fixed effects. eta_{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, total area of the village and number of villages in the GP.[28] We cluster standard errors at the block-level.

In all tables, we present estimates for 50% and 150% of the CCT bandwidths: see panels B and C.

Figure 2.1a performs a McCrary test on the running variable and shows there is little evidence to suggest any manipulation. Table B1 (appendix) presents balance tests for a host of GP level controls.

The main threat to validity of this specification is the fact that SC reserved GPs close to the cutoff are much less likely to be female reserved than their non-SC reserved counterparts. This is an artefact of the reservation rule for women. In some sense, our main treatment effect, therefore, is more likely to be the impact of having an SC male village head vs a typical non-SC head (40% of those close to the threshold are women). Following Calonico et al. [2019], we control for female reservation in *all* our specifications. In section **??**, we

28. Table B13 in the appendix shows that all our main results remain unchanged even when we drop all the controls.

show that the results are robust to dropping all female-reserved seats on either side of the threshold.[29]

2.6 Short-Run Impacts

2.6.1 Private Assets

We create an asset index based on 6 binary asset indicators found in the SECC dataset. We focus on ownership of the most common assets from the data: land; type of the roof (concrete or not) and wall (whether made of burnt brick or concrete) of the main dwelling room of the house structure; whether the house has 4 or more rooms; whether the household has a phone; whether the household owns a vehicle. Each of these assets is owned by at least 10 % of the population (see Figure **??**).

For each household, we create two types of asset scores: a "raw sum of assets" score, where each indicator gives the household one point; a PCA score of all assets (first component). Our private asset index is simply the standardized sum of the two scores.[30]

We first show that reservation has no impact on average private asset score of a household in a GP (Table B4, column (1)). This is an important fact to establish because reserved leaders come from worse socioeconomic backgrounds. As Table B3 indicates, they earn less, attain fewer years of schooling and are younger. Moreover, they are 47 percentage points more likely to report that they belong to the "lower" class (as opposed to "middle" or "upper" income class) on their affidavits. Since this is the first time that reservation is introduced in these villages, they are, by definition, also more likely to be first-time leaders. Yet, they

29. Another minor threat to validity can come from the reservation algorithm. The algorithm was not perfectly implemented and a small set of GPs (N = 78) have a different reservation status from what is predicted by our replication of the algorithm. We show that our results are robust to dropping these GPs entirely (see tables B17 and B18).

30. Table **??** in the online appendix shows that the results are robust to working with only the RSOA or PCA scores and to defining the index as the mean of the two standardized scores (instead of the sum).

perform just as well as the non-SC village heads on this metric.

However, this masks considerable variation, since there are distributional impacts, both across- and within-groups. Column (2) of Table B4 shows that the difference between SCs and non-SC asset scores falls by 0.084 SD in reserved GPs. These results are robust to changing the bandwidth (Tables B4, Panel B and C, and alternate definitions of the asset index (Table **??**).

What drives this reduced gap between SCs and non-SCs? Figure **??** plots impacts by various sub-groups. The top two estimates show that the overall fall is driven by a (statistically insignificant) increase in the mean asset scores of SCs and a commensurate (statistically insignificant) decrease in the mean asset scores of non-SCs.

We then estimate catch-up with the median non-SC quintile across the 4 SC quartiles. We see that while the mean impact is positive for 4 SC quartiles, the effects are significant - and magnitudes higher - for the top two quartiles. These effects are even more pronounced if we restrict attention to only SCs in the top decile. Thus, the top half of the ex-post distribution of SCs in reserved GPs is significantly better off than its counterparts in unreserved GPs. This suggests a small increase in within-SC inequality in reserved GPs.

We proxy for dominant sub-caste among SCs by the most commonly repeated surname. We then see that the numerically dominant sub-caste among SCs does no better than the average SC (see fourth estimate in Fig **??**). However, the literature has long documented clientelistic behaviour by local leaders. We show results that speak to this literature. Figure **??** plots SD impacts by categories of individuals who are typically "close" to the village head. To do so, we track down village heads in reserved GPs from the Socioeconomic Caste Census (SECC) dataset. We match elected head's name, age, gender, occupation and village name (within a GP) to households within the SECC. We are able to track down 61% of the SC heads. We estimate catch-up by comparing difference in asset scores between households close to the village head (as defined below) with the median non-SC quintile household. We

compare this to the average catch-up of all SC households in unreserved GPs.

As the bottom four estimates in Figure **??** show, being close to the village head is associated with a significant increase in relative asset scores. Having the same surname as the head (0.33 SD), living within a 20-household distance[31] (0.86 SD increase) or being the head (1.47 SD increase) are all associated with improved relative well-being.

The proximity results are not causal - since the identity of the head is a post-treatment variable. However, given the large effects, we present these results nonetheless, as strongly suggestive evidence that proximitiy to the head - along subcaste and geographical lines - results in improved asset scores.

We perform one check: prior to election in 2006, nominees were asked to submit their "income class" as part of their nomination forms. The fact that the estimates are broadly the same when we restrict attention to only those SC village heads who, at the time of election, reported being from a low income class (as opposed to middle or high) suggests that our proximity results are causal (Appendix table **??**).

Among non-SCs, the numerically dominant sub-caste is worse off in reserved villages. The impact on asset scores is -0.07 SD and marginally insignificant (p = 0.128) (second estimate in Figure **??**).

2.6.2 Public Goods

From a survey of 50 elected village heads from 2006-2011, we zero in on 4 main public goods. These were selected because villages heads were most likely to name them. Our list includes creation of government primary schools, construction of tar roads, running fair-price shops that distribute grains via the Public Distribution System (PDS) and setting up rural child care centres under the Integrated Child Development Scheme (ICDS).

We first measure effects on overall provision of public goods. We create an index of public

31. SECC data has house numbers.

goods that is the mean of the standardized scores of the 4 public goods in our list. Column (4) of Table B4 presents the results. The index increases by a statistically insignificant 0.02 SD.

We turn to inequalities in public good access. First, we define the main SC village in the GP. Using Census (2011) data, we code the village with the most number of SCs as the main SC village. We then calculate, for each public good, the population normalized share of the good accruing to the main SC village in the GP. Our main outcome variable is the mean of these normalized shares. In an equal society with no hierarchies, the benchmark value of the normalized share should be 1, i.e. the main SC village, like every other village, should get as much access to public goods as its share in the overall population in the GP. However, for unreserved GPs close to the threshold, the main SC village has a share of 0.8, indicating substantial bias in provision against the main SC village.

Column (5) of Table B4 shows that the index increases by 0.2 SD in reserved GPs.[32][33]

2.7 Long-Run Impacts

2.7.1 Private Assets

Using a household survey of 8748 randomly sampled households across 107 GPs, we recreate the poverty indices we use in the previous section. Three caveats here: first, we do not have data on phone ownership, but we do not expect that to bias our index since there is near-universal phone ownership in Bihar;[34] Also, we are working with only 107 GPs, but 43(/66) of these GPs lie within a bandwidth of 50(/150) from the cutoff. Our main specification

32. This corresponds to an increase in the normalized share by 0.09.

33. Table B12 breaks down the index to its constituents and while all 4 public goods are positively impacted by reservation, the coefficients are large and significant for primary schools and paved roads.

34. Indeed, our data from the SECC of 2012 indicates that 82% of rural households already had access to a phone. Official government estimates from 2020 indicates that the total number of mobile phone users in the state grew by 47%. See: https://www.hindustantimes.com/india-news/6-2-crore-mobile-phone-users-3-93-crore-internet-users-in-bihar-it-minister-prasad/story-idCJPhIJDxkvhEErbKXLwI.html.

for these regressions uses a bandwidth of 50, which is substantially smaller than what the CCT bandwidth is, but reflects the survey sampling strategy to focus on GPs very close to the threshold; third, this survey was conducted in 2019-20, 3.5 years *after* the reservation cycle had changed. We are, therefore, not comparing reserved GPs with unreserved GPs. Rather, in our sample, a "treated" GP has 10 years of exposure to a village head from an SC background (but is currently unreserved) and a "control" GP has at most 3.5 years of exposure. Crucially, however, at the time of the survey, there is no treated GP currently reserved for SCs.

Our main result is that the catch-up observed in the short term continue to hold in the long-term. We run the same specification as we do with the short run, with differing bandwidths (Table B4, column (3) of Panels A and C). We find strong evidence of continued catch-up: there is a 1.12 SD reduction in the difference between mean asset scores of SCs and non-SCs. Owing to the low sample size, the confidence intervals are wide, but the minimum end of the 99% interval suggests a 0.45 SD improvement in SCs' relative asset scores.

Given our relatively small sample sizes within groups, we are unable to make claims about the distributional consequences.

We show robustness to the following bandwidths/specifications in Table B16: triangular bandwidth of 100, CCT triangular bandwidth, CCT Triangular bandwidth with no block fixed effects and CCT Uniform bandwidth. In each of our specifications, we find that the effect sizes are large (the p-value is always < 0.01) and they never fall below the coefficient in our main specification from Table B4.[35]

2.7.2 Public Goods

To measure improvements in public good provision in the long-run, we turn to our administrative dataset comprising water and sanitation (WAS) public goods. These are to be

35. These effects persist even when we control for the 2012 (i.e. short-run) difference in asset scores between SCs and non-SCs in these 2 GPs.

provided to every ward over a 3 year period (2017 - 2020). As Sharan and Kumar [2021] document: (a) wards headed by SCs, which almost always are numerically dominated by SCs, are to be prioritized in allocation of these goods across wards and (b) wards headed by SCs often see delays, but there is eventual catch-up.

Our main measure of long-run provision relates to delays. We measure the SC-proportion normalized share of WAS projects[36] allocated to SC wards at the end of the first year of implementation of these WAS schemes (March 31st, 2018).

Once more, as in the case of long-run private assets, we are comparing GPs that have had 10 years of reservation with GPs that have had, at most, 2 years of reservation.[37] Column (6) of Table B4 presents results: GPs reserved from 2006 - 2016 see a 0.11 SD increase in the SC-proportion normalized share of WAS projects.

2.7.3 Political Participation

Impact on GP elections

We first estimate the impact of reservation on the caste of the GP head in the next reservation cycle. To do so, we restrict attention to only those GPs that are not reserved in the next cycle. Note that, by law, all GPs reserved between 2006-16 are not reserved again. We end up with 4693 GPs that we have data for and are currently unreserved.

1.6% of never-reserved GPs have an SC village head. This number jumps by 11.6 p.p in reserved GPs close to the threshold (Table B5, Column (1)). The number of SC candidates contesting elections increases from 0.83 to 2.141 (Column (2)).

36. Proportion normalized share = (Share of Projects in SC wards/Share of SCs in the GP)

37. 35% of GPs that were previously unreserved and lie within the RD bandwidth are now reserved for SCs.

2.7.4 Impact on ward elections

Wards are a tier of local government below the GP. There are about 13.6 wards for every GP. The advantage of measuring effects at the ward level is that we run our RD for all available GPs. Here too, we find positive impacts. GPs reserved between 2006-16 see about 30% more SC winners and candidates contesting (Columns (3) and (4) of Table B5) when compared to counterparts in previously unreserved GPs. We report results only for unreserved wards in these GPs. As a placebo check, we show, in Column (5) that there are no additional winners coming from wards that are already reserved for SCs.

Prior to 2016, ward heads had no access to financial resources and had very little decision-making powers.[38] Thus, these elections were not contested by local elites. On the other hand, GP heads enjoyed enormous powers with respect t0 creation of public goods and last-mile implementation of government programs - as this paper shows too. Political entry at the GP-head level, therefore, was strongly correlated with wealth. The fact that we find greater long-run effects on political entry of SCs at the GP head level in unreserved GPs suggests that the improved material well-being of SCs played a part in better their political empowerment.

2.8 Mechanisms

2.8.1 Targeting Benefits

Housing

A main component of our asset index is house quality. Allocation of houses under the subsidized housing scheme, the Pradhan Mantri Awas Yojana (PMAY), falls under the ambit of the GP head. However, our housing data is only available for years after 2016, after the

38. See Sachchidananda [2007] for detailed qualitative interviews with Dalit ward members from the mid-2000s. Chapter 5 of Sharan [2021] talks about limited decentralisation of Bihar and the lopsided power equation between GP heads and ward members.

reservation cycle is switched. This, therefore, allows us to test if better targeting of housing schemes could explain some of the improved wealth scores in the long-run. Note that our primary survey was conducted in 2018-19. Hence, we focus on houses constructed up to March 31st, 2019.

As in the case of our long-run private asset and public good regressions, we are comparing GPs that have been reserved for 10 years but are currently unreserved with those that have, at most, been reserved for a period of 3 years.

Our first outcome is the number of houses constructed per SC in the GP. Column (1) of Table B6 shows that this outcome improves by 0.13 SD. To calculate how this translates to reductions in inequality between SCs and non-SCs, we normalize the share of houses completed for SCs by their share in the population of the GP. This improves by 0.08 SD (Column (2)), suggesting a reduction in SC- non-SC inequality in accessing to housing.

NREGA

We turn now to NREGA persondays data for the years 2014-2016, the last two years before which the reservation cycle switched.

We first look at the impact on SC persondays generated per SC individual in the GP (SC Persondays/SC Population). This rises – though somewhat imprecisely – by 0.08 SD (Column 3).

Bihar is not among the front-runners in the implementation of the NREGA (Kumar et al. [2021]). Indeed, the median SC household in our dataset receives about 6 man-days per year. NREGA pays minimum wage – so, 6 days of work is unlikely to affect asset scores for households, even over the very long run.

Hence, we turn to an alternate measure: the share of households receiving 100 days of work. A household receiving a full 100 days of work makes a substantial sum of money

through the scheme over the year.[39] To measure the impact on inequality between SCs and non-SCs, we calculate the share of SC households among all households receiving 100 days of work normalized by the proportion of SCs in the GP. Reservation increases this share by 0.17 SD (Column (4)).

2.8.2 Short-Run Changes in Occupation Patterns

One potential way for SCs to accumulate assets is through improved labour market outcomes. While we do not have access to data on wages, we use two other variables from the SECC to approximate changes in occupational patterns and labor market outcomes in the short term. First, we test if the proportion of SCs owning land increases.[40] The literature has long recognized land as a key asset for development [Bardhan, 1984]. Additionally, we investigate if households' primary source of income comes from cultivation, as opposed to manual casual labor, which is the overwhelmingly dominant source of income for SCs.[41] We also measure catch-up with non-SCs, as before.

Table B7 reports the results. We find that SCs are slightly more likely to own land in reserved GPs. While the coefficients are positive, the effects are minimal and statistically insignificant. However, when we compare SCs to non-SCs, we observe more significant effects. Overall, we report a 0.09 SD improvement in the relative share of SCs owning land, which also leads to more SCs deriving their primary income from cultivation (column 2) and catching up with non-SCs (column 4).

We interpret these findings as suggestive evidence that SCs' labor market outcomes and occupational trajectories may have been affected by reservation. The difficulty in altering these outcomes, particularly in the short term, indicates that SC wages in the short term or

39. Back-of-the-envelope calculations suggest that two years of 100 days of work would account for over 40% of the transfers under the housing scheme for the period 2014-16.

40. Land ownership is already part of our asset index.

41. On average, 7% of SCs report that their main source of income comes from cultivation, as opposed to 86% for manual casual labour.

their occupational trajectories are likely to be more strongly impacted.

2.8.3 Political Mechanisms

2.8.4 Complaints

We begin by looking at complaints made by politically active individuals under a formal complaints system (described in [Sharan and Kumar, 2021]). The complaints system was launched in 2016 (a few months after the election) and we have data on the universe of complaints filed until September 2019. We define politically active citizens as those who have contested and lost the 2016 local elections at the GP head, ward and sarpanch levels. We focus on these for two reasons: first, we have data on the near-universe of such individuals, linked to their caste group; second, we have phone numbers of these individuals which we use to merge with the complaints data. We drop all GPs that were reserved in 2016 for SCs: this allows us to compare complaints by politically active citizens across currently unreserved GPs, with the only difference being the treated group saw reservation for ten years while the control group had no reservation.[42]

Column (6) of Table B6 shows that the population normalized share of SC complaints increases by 0.15 SDs. This shows that politically active SC citizens, even while not in government, are substantially more likely to make claims on the state in GPs that have been reserved for ten years. This, therefore, translates to improvements in relative material well-being for SCs.

42. The main reason we don't want to include SC reserved GPs from 2016 is because complaints by losing SC candidates in those GPs could be politically motivated. By dropping those GPs, we are comparing GPs with relatively identical GP-level administrations.

2.8.5 Stability of tenure?

For 3489 GPs, we have data on whether the leader elected in 2006 won the election again in 2011. Since the reservation status of GPs was unchanged, SC leaders continued to have a restricted set of competitors. Column (5) of Table B6 shows that SC leaders have consistently lower re-election rates than their non-SC counterparts, a finding that suggests that redistribution continued apace despite greater political churn in reserved areas.

Lower re-election rates for SC GP heads could suggest many things: first, that voters – the majority of whom are non-SC – are less satisfied with the incumbent's ability to serve them (Ferejohn [1986]). On the other hand, in deeply unequal societies with entrenched elites, higher re-election rates could imply an inability to vote out low-performing leaders. In unreserved GPs in particular, the landed elite could capture power, irrespective of how they perform when in power. Thus, both high and low re-election rates could affect how outcomes diverge across different groups.

2.9 SC Population and Effectiveness of Reservation

Affirmative action quotas in favour of ethnic minorities exist across the world, across a vast variety of contexts. The share and number of minorities vary considerably across these contexts. In India alone, the share of SCs varies from under 1% in some states to up to 30% in others. Bihar's share is almost exactly equal to the national average, but there is considerable heterogeneity in share of SCs even within Bihar. We exploit this heterogeneity in the prevalence of SCs and the flexibility afforded by our RD design to ask: how does the effectiveness of SC reservation vary as SC prevalence varies? In other words, do SC GP heads have the largest impacts when SCs are a small/large share of the population?

Our RD is a pooled estimate of multiple cutoffs, with a separate SC GP population threshold in every block. This gives rise to 525 separate natural experiments - we cannot, for reasons of power, estimate precise effects at every threshold, but we could make categories

of thresholds and estimate effects for each category separately. Figure 2.2 gives us the distribution of thresholds across blocks. The median block has a threshold SC GP population of 1858. The average GP around this threshold has 17.5% SCs. However, the block with the lowest cutoff has a threshold value of 522 SCs (SC percentage = 2.6%) and the block with the highest cutoff has a threshold of 3862 SCs (SC percentage = 41.7%). These blocks, therefore, encompass within them the diversity of SC percentages seen across Indian states, underlying the importance of measuring the effectiveness of reservation in this context by prevalence of SCs.

We divide our sample into three groups, based on terciles of the cutoffs. GPs in tercile 1, 2 and 3 have SC percentage rates of 11.1%, 16.2% and 21.5% respectively. We estimate equations 2.2 and 2.3 within each of these terciles and report results. We report findings for the main catch-up related variables in the short- and long-run in Table B8.[43]

In the short run, reservation seems to work best in GPs where SCs are small in number (and a small fraction of the population). Indeed, in the top tercile of GPs, reservation has no effects in the short-run at all. On the other hand, in the bottom tercile the difference between SC and non-SC asset scores falls by 0.15 SD (imprecisely estimated) and the index of public goods in the main SC village increases by 0.39 SD ($p < 0.02$).[44] This suggests that, in the short-run at least, reservation works best to give voice to SCs where they do not have strength in numbers. However, once reservation is withdrawn, SC leaders from these very GPs have the hardest time winning elections. Column 3 of table B15 shows that this is not because of a drop in candidacy rates - SCs are empowered enough to contest, but do not have the numbers to win. This potentially results in relatively poor implementation of WAS projects in these GPs in the long-run.

Reservation works best when SCs are neither too small nor too large in number i.e. the

43. The full set of results are in appendix table B15.

44. The estimate for the first and the second tercile together is 0.139 SD, $p = 0.04$.

middle tercile of our GPs. Here, SCs do not have strength in numbers, so reservation helps reorient policies towards them in the short run. However, in the long-run, their numbers are not so small that, even in the absence of reservation, they cannot *win* elections - or, at the very least, influence village policies.[45] This allows them to reap benefits even after reservation is withdrawn.

2.10 Within-SC Heterogeneity and Effectiveness of Reservation

A vast literature argues that ethnic diversity worsens public good provision (Alesina et al. [1999]; Banerjee et al. [2007]). It is hypothesized that this is because diversity worsens the ability of groups to act in their collective interests. In our context, we ask: does within-SC diversity change how effective reservation is? Theoretically, it is unclear what to expect: if SCs in a GP belong to a variety of sub-castes, then the elected leader may only serve their own sub-caste's interests and SCs as a whole may not be better off. Indeed, some of this is reflected in the results documented in Figure **??**. On the other hand, a more diverse set of sub-castes could imply that the elected GP head faces stiffer competition from within the pool of SCs[46] and therefore has stronger re-election incentives. Thus, ex ante, the predictions are ambiguous.

As in the previous section, we categorise blocks into 3 categories based on our variable of interest at the block level. A caveat here: unlike in the previous section (section 2.9), which focuses on breaking down a pooled treatment effect by its components, the regressions here are picking up more conventional heterogeneous treatment effects by sub-caste heterogeneity.

We proceed as follows: we first calculate the HHI of surnames - our proxy for sub-caste -

45. The median election margin of victory in unreserved seats in the 2016 GP elections is 126. The median difference in SC population between tercile 2 and tercile 1 GPs is 437. Assuming adults are 50% of the overall population, this amounts to approximately 220 additional SC voters. Thus, the additional SCs in GPs in the middle tercile could swing elections on their own.

46. GPs were reserved for two terms. So, all candidates in the 2011 elections also came from the pool of SCs.

for each block. We define the HHI as the sum of the squares of the shares of every surname in the GP. The average HHI of a GP is 0.23. We then categorize GPs into 3 groups (terciles) based on block-level heterogeneity. Block level HHI is very strongly correlated with GP-level HHI in our sample, as figure 2.3 shows.

Our results suggest that reservation is most effective in homogeneous GPs. The top tercile of GPs - i.e the most homogeneous tercile – show the largest effects in catch up in private assets (0.177 SD) and targeting of public goods (0.3 SD) in the short run and the largest improvements in political empowerment (1.19 SD improvements in having a SC GP head) even after reservation is removed.[47]The only place where we do not see better outcomes is long-run WAS public goods - the treatment effects are still positive, however, though imprecisely estimated. Overall, it does appear that for 4 out of 5 outcomes, reservation makes SC most better off in relatively homogeneous GPs.

2.11 Conclusion

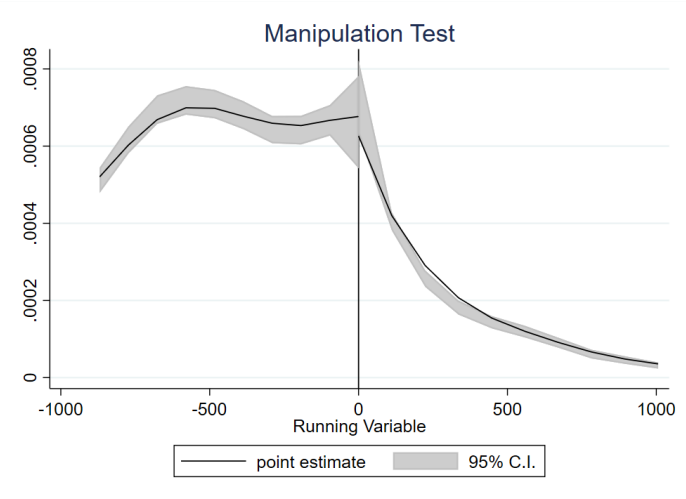
This paper brings to bear a wealth of data to show that affirmative action policies like political reservation could help reduce inequality in access to public goods, accumulation of private assets and political posts, creating a complex web of winners and losers with varying effects in the short and long-run.

The policy implications of this paper are three-fold: first, our results show that political reservation could be an important tool in reducing inter-group inequality in the short- and long-run; second, this paper shows the importance of having sustained affirmative action policies: while, at the end of the first electoral cycle SCs' access to public goods had improved, the true gains in private assets are realized much more in the long-run; third, the results suggest that well-implemented affirmative action policies could actually be self-reinforcing:

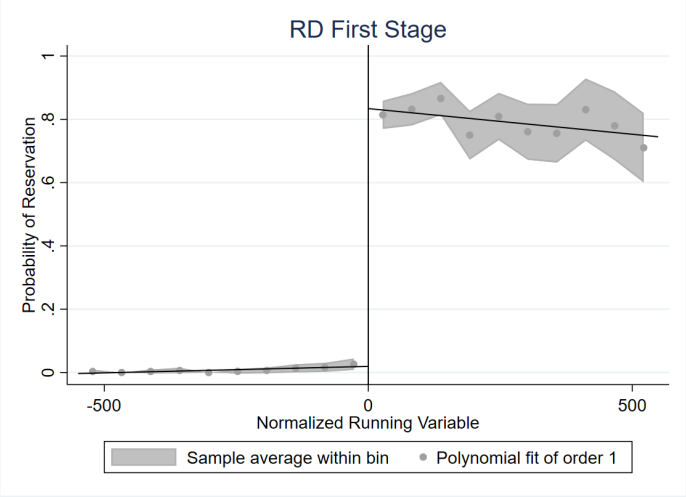
47. Once again, as column (3) of Table B14 shows, the effects on candidates likelihood to stand are similar throughout - if anything, more heterogeneous SC GPs throw up more candidates, but fewer winner. These regressions control for proportion of SCs.

reservation improves resource access which results in more political participation; fourth, the paper suggests that the gains of reservation are reaped best when the minority group is neither too small or too large and when it is more homogeneous.

2.12 Figures



(a) Mccrary Test



(b) First Stage

Figure 2.1: Part a plots the density of the running variable around the RD threshold and displays results from a Mccrary test. The results indicate no discontinuity on either side of the RD threshold and allows us to reject the claim that the running variable is manipulated. 95% confidence intervals are also shown in the shaded region. Part b plots the likelihood of a GP being reserved on either side of the RD threshold. The probability of being reserved is nearly zero to the left of the threshold and jumps to over 0.8 to the right. 95% confidence intervals are also shown in the shaded region.

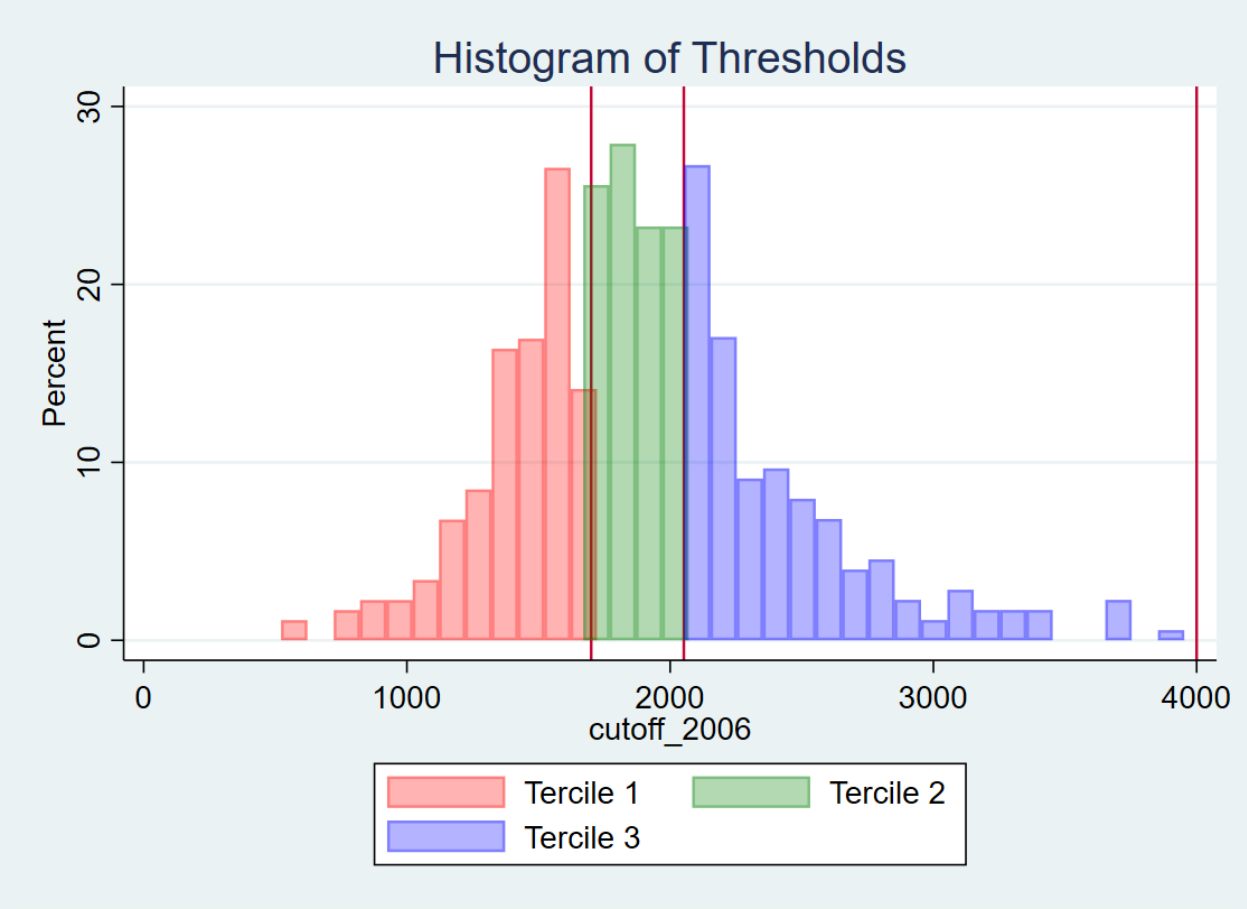


Figure 2.2: Figure shows histogram of SC population cutoffs across 525 blocks. The different shades of colours indicate the GPs that fall under each tercile of thresholds - with the lines displaying where every tercile ends.

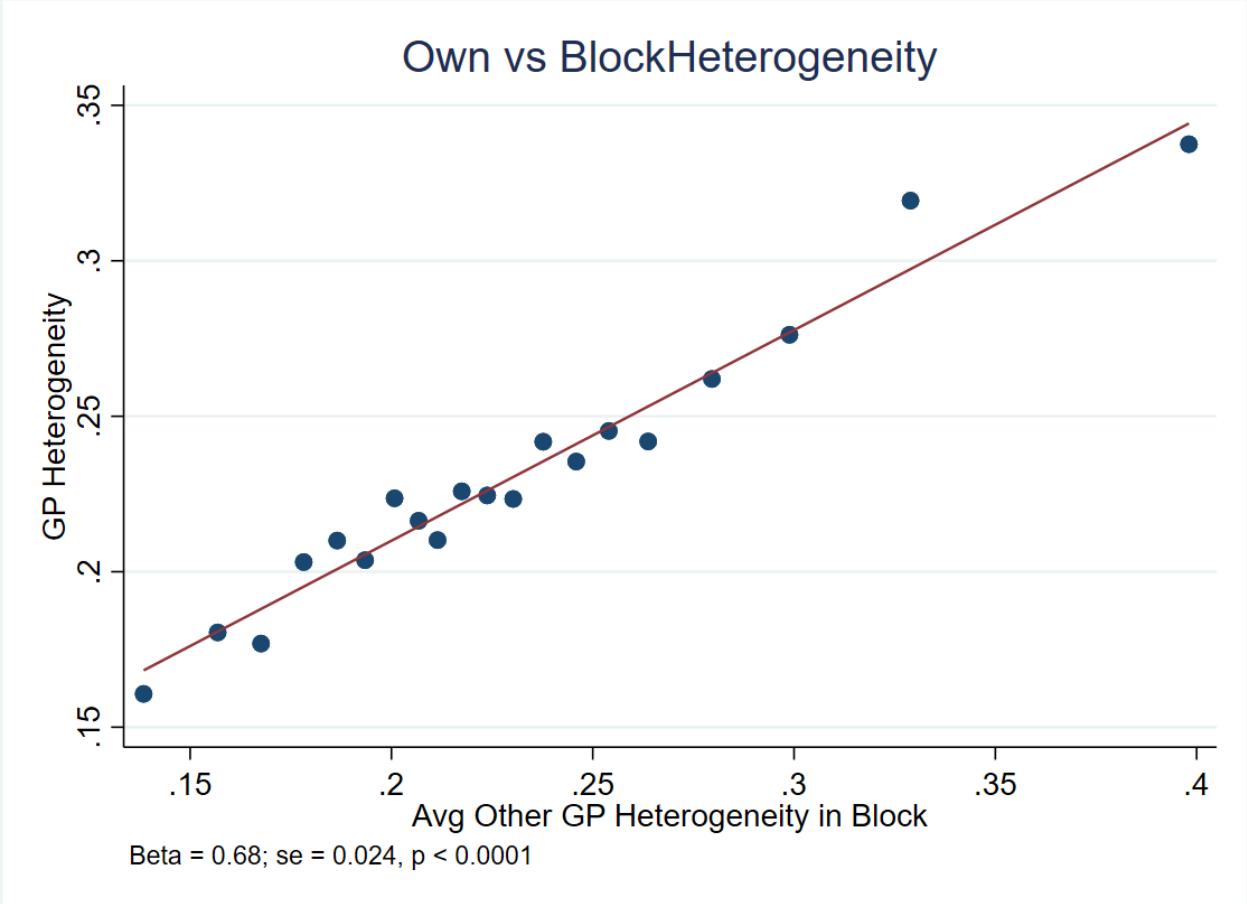


Figure 2.3: Figure shows how own-GP HHI of sub-castes varies with block heterogeneity of sub-castes (leaving own-GP out).

2.13 Appendix B.

2.14 Tables

Table B1: Balance Across the RD Sample (GP-level Controls)

Variable	Reserved	Unreserved	Difference	SE
Proportion of SCs (Census 2001)	0.17	0.17	0.00	0.00
Distance to Nearest Statuatory Town (Census 2011)	20.93	20.52	0.41	0.43
Distance to District Headquarters (Census 2011)	32.14	31.64	0.50	0.49
Number of Villages in GP (Census 2011)	5.64	5.57	0.07	0.25
Total GP Area (Census 2011)	1,222.14	1,118.53	103.61	61.88
Total Population of GP (Census 2001)	9,667.35	9,537.61	129.74	275.31
Percentages of SCs in Main SC Village (Census 2011)	0.57	0.56	0.01	0.02
Female Reservation	0.06	0.42	-0.36	0.03

NOTE: Table presents results from a series of balance tests for GP-level variables across the population-based RD cutoff. We operationalize tests in the following manner: we run a fuzzy RD with bandwidth = 550. Standard errors are clustered at the Block level.

Table B2: Balance Across the RD Sample (Non-Gender Reserved Seats ONLY)

Variable	Reserved	Unreserved	Difference	SE
Proportion of SCs (Census 2001)	0.18	0.18	-0.00	0.00
Distance to Nearest Statuatory Town (Census 2011)	21.25	20.56	0.69	0.56
Distance to District Headquarters (Census 2011)	32.09	31.77	0.32	0.60
Number of Villages in GP (Census 2011)	5.64	5.61	0.03	0.28
Total GP Area (Census 2011)	1,167.80	1,129.80	37.99	79.66
Total Population of GP (Census 2001)	9,367.62	9,170.94	196.68	326.32
Percentages of SCs in Main SC Village (Census 2011)	0.56	0.56	0.00	0.02

NOTE: Table presents results from a series of balance tests for GP-level variables across the population-based RD cutoff, keeping only non-gender reserved seats. We operationalize tests in the following manner: we run a fuzzy RD with bandwidth = 550. Standard errors are clustered at the Block level.

Table B3: Impact of SC Reservation on Characteristics of the GP head

	Reservation Impacts on Politician Characteristics				
(lr)2-6	(1)	(2)	(3)	(4)	(5)
	Barely (Literate)	**High School Or Below**	**Age**	**Yearly Income**	**Lower Class**
SC Reserved	0.080	0.157	-3.662	-2.3e+04	0.479
	(0.027)	(0.033)	(0.762)	(5538.694)	(0.029)
Observations	6939	6939	6877	6101	7200
Mean	.202	.652	39.121	52399.791	.087
Bandwidth	633	700	612	554	666
Block FE	YES	YES	YES	YES	YES

Table displays impacts of SC reservation on political characteristics of the SC GP Head in the 2006 elections. Column (1) is an indicator for whether the politician is illiterate or literate, but not primary schooling level; Column (2) is an indicator for whether the GP head's educational level is below high school; Column (3) is age; Column (4) is annual income (self-reported); (5) Column 5 is an indicator variable that takes the value of 1 if the politician reported their income class as being "lower" (the other two options were "middle" and "upper"). We use CCT triangular bandwidths and estimate fuzzy RD specifications described in the paper (equation 2.2 and 2.3). We control for GP-level covariates and Block-fixed effects. All standard errors are clustered at the Block level.

Table B4: Impact of SC Reservation on Public Goods/Private Assets in the GP

	Private Assets			Public Goods		
(lr)2-4(lr)5-7	(1)	(2)	(3)	(4)	(5)	(6)
	Overall (Short Run)	**SC-Others (Short Run)**	**SC-Others (Long Run)**	**Overall (Short Run)**	**SC Village Short Run**	**SC Village Long Run**
SC Reserved	-0.015	0.090	1.112	0.012	0.202	0.151
	(0.037)	(0.047)	(0.258)	(0.043)	(0.075)	(0.069)
Observations	8170	7952	43	8171	8044	7758
Effective Observations	4428	5196	41	3904	4349	3513
Mean	.003	.045	0	.001	.001	-.002
Bandwidth	571	704	50	495	564	466
Block FE	YES	YES	YES	YES	YES	YES
	PANEL B: 0.5 BW					
(lr)2-6	(1)	(2)	(3)	(4)	(5)	
	Overall (Short Run)	**SC-Others (Short Run)**		**Overall (Short Run)**	**SC Village Short Run**	**SC Village Long Run**
SC Reserved	-0.010	0.097		0.045	0.303	0.203
	(0.052)	(0.061)		(0.058)	(0.097)	(0.093)
Observations	8170	7952		8171	8044	7758
Effective Observations	2445	2860		2182	2355	2278
Mean	0	0		0	0	-.001
Bandwidth	285	352		248	275	275
Block FE	YES	YES		YES	YES	YES
	PANEL C: 1.5 BW					
(lr)2-7	(1)	(2)	(3)	(4)	(5)	(6)
	Overall (Short Run)	**SC-Others (Short Run)**	**SC-Others (Long Run)**	**Overall (Short Run)**	**SC Village Short Run**	**SC Village Long Run**
SC Reserved	-0.013	0.071	1.300	0.004	0.175	0.108
	(0.031)	(0.041)	(0.252)	(0.036)	(0.067)	(0.055)
Observations	8170	7952	100	8171	8044	7758
Effective Observations	6148	6744	53	5551	5381	5185
Mean	0	0	-.077	0	0	0
Bandwidth	856	1056	75	743	725	725
Block FE	YES	YES	YES	YES	YES	YES

Table displays SD impacts of reservation on public goods and private assets in the short- and long-run. Private assets are covered in columns (1) - (3). Columns (3) - (6) pertain to public goods. Outcome variables are (in standardized units): (1) The average private asset score across all households in a GP in 2011-12 (measured via the Socioeconomic Caste Census) (2) the difference between private asset scores between SCs and non-SCs in a GP in 2011-12 (measured via the Socioeconomic Caste Census) (3) The difference between private asset scores between SCs and non-SCs in a GP in 2018-19 (Primary Survey) (4) Public good index from 4 main public goods in the GP from Census 2011 (we trim the top 1% of observations) (5) Normalized share of public goods to the main SC village in the GP from Census 2011 (we trim the top 1% of observations) (6) SC Proportion-normalized share of water and sanitation (WAS) public goods provided to SC wards up to 31st March 2018 (WAS Admin Data). We use CCT triangular bandwidths and estimate fuzzy RD specifications described in the paper (equation 2.2 and 2.3). Except for column (3), where: in Panel A, we fix a bandwidth of 50 and have a uniform weights; in panel B, we are not powered to see effects with 0.5 BWs; in Panel C, we fix a bandwidth of 100 (2 times Panel A) and use triangular weights. We control for GP-level covariates and Block-fixed effects in all regressions. All standard errors are indicated in brackets below the estimates and are clustered at the Block level. Panels (B) and (C) show estimates when bandwidth are halved or increased by a factor of 1.5 respectively.

Table B5: Impact of SC Reservation on Long-Run Political Participation of SCs

	GP		Ward		
(lr)2-3(lr)4-6	(1)	(2)	(3)	(4)	(5)
	Winner	**Candidates**	**Winner (Unreserved)**	**Candidate (Unreserved)**	**Placebo: Winner Reserved**
SC Reserved	0.113	1.297	0.254	0.896	-0.013
	(0.024)	(0.183)	(0.074)	(0.217)	(0.064)
Observations	4663	4700	7369	7369	7369
Effective Observations	2561	1878	4014	3619	3696
Mean	.018	.85	.841	3.135	2.194
Bandwidth	675	508	576	510	522
Block FE	YES	YES	YES	YES	YES
PANEL B: 0.5 BW					
(lr)2-6	(1)	(2)	(3)	(4)	(5)
	Winner	**Candidates**	**Winners (Unreserved)**	**Candidates (Unreserved)**	**Placebo: Winner Reserved**
SC Reserved	0.092	1.275	0.186	0.769	-0.057
	(0.037)	(0.252)	(0.100)	(0.287)	(0.085)
Observations	4663	4700	7369	7369	7369
Effective Observations	971	974	2145	2145	2145
Mean	.019	.9350000000000001	.778	2.97	2.398
Bandwidth	275	275	275	275	275
Block FE	YES	YES	YES	YES	YES
PANEL C: 1.5 BW					
(lr)2-6	(1)	(2)	(3)	(4)	(5)
	Winner	**Candidates**	**Winners (Unreserved)**	**Candidates (Unreserved)**	**Placebo: Winner Reserved**
SC Reserved	0.111	1.316	0.244	0.890	-0.016
	(0.024)	(0.157)	(0.068)	(0.182)	(0.058)
Observations	4663	4700	7369	7369	7369
Effective Observations	2767	2781	4896	4896	4896
Mean	.017	.781	.833	3.091	2.036
Bandwidth	725	725	725	725	725
Block FE	YES	YES	YES	YES	YES

Table displays impacts of SC reservation on political outcomes in the long-run. Columns (1) and (2) pertain to GP-level outcomes; Columns (3) - (5) pertain to ward level outcomes. Outcome variables are: (1) An indicator of whether an SC won the 2016 GP elections (sample restricted to only unreserved GPs in 2016, hence N = 4693) (2) The number of SC candidates contesting elections in a GP in 2016 (sample restricted to only unreserved GPs in 2016) (3) The number of SC winners from unreserved wards in the 2016 ward elections (4) The number of SC candidates from unreserved wards in the 2016 ward elections (5) The number of winners from SC-reserved wards in the 2016 ward elections (this is called a 'placebo' because it should remain unchanged trivially). We use CCT triangular bandwidths and estimate fuzzy RD specifications described in the paper (equation 2.2 and 2.3). We control for GP-level covariates and Block-fixed effects. All standard errors are clustered at the Block level. Panels (B) and (C) show estimates when bandwidth are halved or increased by a factor of 1.5 respectively.

Table B6: Mechanisms

	IAY Housing		NREGA (Work)		Political	
(lr)2-3(lr)4-5(lr)6-7	(1)	(2)	(3)	(4)	(5)	(6)
	Houses Per SC	**Normalized SC Houses**	**Persondays Per SC**	**Normalized 100 Days Hhds**	**Normalized Complaints**	**Re-elected 2011**
SC Reserved	0.171	0.059	0.085	0.169	0.124	-0.139
	(0.066)	(0.057)	(0.057)	(0.064)	(0.070)	(0.047)
Observations	6112	6110	7856	7837	6410	3423
Effective Observations	2899	2665	3274	3409	4001	1860
Mean	-.005	.009	-.004	-.007	-.007	.266
Bandwidth	568	526	420	440	739	540
Block FE	YES	YES	YES	YES	YES	YES
			Panel B: 0.5 Bandwidth			
(lr)2-7	(1)	(2)	(3)	(4)	(5)	(6)
	Houses Per SC	**Normalized SC Houses**	**Persondays Per SC**	**Normalized 100 Days Hhds**	**Normalized Complaints**	**Re-elected 2011**
SC Reserved	0.129	0.060	0.102	0.202	0.222	-0.193
	(0.077)	(0.059)	(0.068)	(0.087)	(0.153)	(0.067)
Observations	7390	7386	7856	7837	6410	3423
Effective Observations	2183	2180	2310	2311	1428	1043
Mean	-.001	-.001	.001	-.001	-.001	.269
Bandwidth	275	275	275	275	275	275
Block FE	YES	YES	YES	YES	YES	YES
			Panel B: 1.5 Bandwidth			
(lr)2-7	(1)	(2)	(3)	(4)	(5)	(6)
	Houses Per SC	**Normalized SC Houses**	**Persondays Per SC**	**Normalized 100 Days Hhds**	**Normalized Complaints**	**Re-elected 2011**
SC Reserved	0.129	0.067	0.037	0.119	0.125	-0.120
	(0.052)	(0.041)	(0.047)	(0.051)	(0.071)	(0.041)
Observations	7390	7386	7856	7837	6410	3423
Effective Observations	4959	4956	5266	5271	3923	2370
Mean	0	0	0	0	0	.268
Bandwidth	725	725	725	725	725	725
Block FE	YES	YES	YES	YES	YES	YES

Table shows the impact of SC reservation on scheme and electoral outcomes. Outcome variables are (in standardized units): (1) The number of houses constructed for SCs under the PMAY (IAY) scheme per SC individual in a GP (2) the SC proportion normalized share of scheme benefits (houses) received by SCs (3) The person-days generated per SC individual in a GP under the NREGA (4) The SC proportion normalized share of SC households receiving 100 days of work under the NREGA (5) The SC proportion normalized share of complaints made by politically active SC citizens. N = 6410 because we drop GPs that were reserved for SCs in the 2016 cycle. In Column (6), we report a non-standardized outcome: an indicator for whether a leader was re-elected in the 2011 GP elections. N = 3423 because we do not have data for all districts for the 2011 winners. We use CCT triangular bandwidths and estimate fuzzy RD specifications described in the paper (equation 2.2 and 2.3). We control for GP-level covariates and Block-fixed effects. All standard errors are clustered at the Block level. Panels (B) and (C) show estimates when bandwidth are halved or increased by a factor of 1.5 respectively.

Table B7: Occupation & Land Ownership

		SCs		Catch-Up (Differences)
(lr)2-4(lr)5-5	(1)	(2)	(3)	(4)
	Land Owned (SCs)	**Cultivator Share (SCs)**	**Land Owned (SC-Others)**	**Cultivator (SC-Others)**
SC Reserved	0.047	0.084	0.092	0.094
	(0.045)	(0.058)	(0.050)	(0.057)
Observations	7973	7973	7945	7945
Mean	0	-.005	-.005	-.008
Bandwidth	479	523	521	494
Block FE	YES	YES	YES	YES
PANEL B: 0.5 BW				
(lr)2-4(lr)5-5	(1)	(2)	(3)	(4)
	Land Owned (SCs)	**Cultivator Share (SCs)**	**Land Owned (SC-Others)**	**Cultivator (SC-Others)**
SC Reserved	0.020	0.077	0.061	0.031
	(0.061)	(0.088)	(0.069)	(0.078)
Observations	7973	7973	7945	7945
Mean	0	0	0	0
Bandwidth	240	262	260	247
Block FE	YES	YES	YES	YES
PANEL B: 1.5 BW				
(lr)2-4(lr)5-5	(1)	(2)	(3)	(4)
	Land Owned (SCs)	**Cultivator Share (SCs)**	**Land Owned (SC-Others)**	**Cultivator (SC-Others)**
SC Reserved	0.022	0.058	0.065	0.071
	(0.037)	(0.048)	(0.042)	(0.048)
Observations	7973	7973	7945	7945
Mean	0	0	0	0
Bandwidth	719	785	781	740
Block FE	YES	YES	YES	YES

Table shows the impact of SC reservation on land ownership and occupational patterns. Outcome variables are (in standardized units): (1) The share of SC households in a GP that own land (2) the share of SC households in a GP whose main source of income is reported to be derived from cultivation (3) The difference between SCs and non-SC share of households in a GP who own land (4) The difference between SCs and non-SC share of households in a GP who are report their main source of income to be derived from cultivation We use CCT triangular bandwidths and estimate fuzzy RD specifications described in the paper (equation 2.2 and 2.3). We control for GP-level covariates and Block-fixed effects. All standard errors are clustered at the Block level. Panels (B) and (C) show estimates when bandwidth are halved or increased by a factor of 1.5 respectively.

Table B8: Where is Reservation Most Effective? Analysis by SC Population

	PANEL A: 1st Tercile				
(lr)2-6	(1)	(2)	(3)	(4)	(5)
	SC-Others (Private Assets (SR))	**SC Village Public Goods (SR)**	**WAS Public Goods (LR)**	**GP Wins (LR)**	**Ward Wins (LR)**
SC Reserved	0.145	0.389	-0.123	0.451	0.241
	(0.141)	(0.153)	(0.118)	(0.342)	(0.132)
Observations	2587	2623	2502	1728	2354
Mean	-.093	-.025	-.012	.021	-.032
Bandwidth	284	348	484	377	474
Block FE	YES	YES	YES	YES	YES
	PANEL A: 2nd Tercile				
(lr)2-6	(1)	(2)	(3)	(4)	(5)
	SC-Others (Private Assets (SR))	**SC Village Public Goods (SR)**	**WAS Public Goods (LR)**	**GP Wins (LR)**	**Ward Wins (LR)**
SC Reserved	0.129	0.333	0.218	1.838	0.247
	(0.086)	(0.125)	(0.100)	(0.478)	(0.128)
Observations	2615	2648	2571	1674	2460
Mean	-.013	.006	.013	.035	.007
Bandwidth	499	580	603	359	738
Block FE	YES	YES	YES	YES	YES
	PANEL A: 3rd Tercile				
(lr)2-6	(1)	(2)	(3)	(4)	(5)
	SC-Others (Private Assets (SR))	**SC Village Public Goods (SR)**	**WAS Public Goods (LR)**	**GP Wins (LR)**	**Ward Wins (LR)**
SC Reserved	0.006	-0.044	0.306	1.061	0.300
	(0.095)	(0.137)	(0.118)	(0.362)	(0.140)
Observations	2756	2774	2685	1554	2555
Mean	-.016	-.01	0	-.013	-.01
Bandwidth	519	520	583	456	586
Block FE	YES	YES	YES	YES	YES

Table shows the impact of SC reservation on main outcomes, broken down by SC population. Outcome variables are (in standardized units where not mentioned otherwise): (1) the difference between private asset scores between SCs and non-SCs in a GP in 2011-12 (measured via the Socioeconomic Caste Census) (2) Normalized share of public goods to the main SC village in the GP from Census 2011 (we trim the top 1% of observations) (3) SC Proportion-normalized share of water and sanitation (WAS) public goods provided to SC wards up to 31st March 2018 (WAS Admin Data). (4) An indicator of whether an SC won the 2016 GP elections (sample restricted to only unreserved GPs in 2016) (5) The number of SC winners from unreserved wards in the 2016 ward elections. We use CCT triangular bandwidths and estimate fuzzy RD specifications described in the paper (equation 2.2 and 2.3). We control for GP-level covariates and Block-fixed effects. All standard errors are clustered at the Block level. Panels (A), (B) and (C) show estimates when we restrict sample to the the first, second and third tercile of GPs by SC population.

Table B9: Where is Reservation Most Effective? Analysis by SC HHI

	PANEL A: 1st Tercile				
(lr)2-6	(1)	(2)	(3)	(4)	(5)
	SC-Others (Private Assets (SR))	**SC Village Public Goods (SR)**	**WAS Public Goods (LR)**	**GP Wins (LR)**	**Ward Wins (LR)**
SC Reserved	0.045	0.122	0.180	0.459	0.162
	(0.097)	(0.129)	(0.123)	(0.326)	(0.129)
Observations	2642	2694	2649	1651	2453
Mean	.002	-.003	0	.014	.002
Bandwidth	568	597	550	517	587
Block FE	YES	YES	YES	YES	YES
	PANEL A: 2nd Tercile				
(lr)2-6	(1)	(2)	(3)	(4)	(5)
	SC-Others (Private Assets (SR))	**SC Village Public Goods (SR)**	**WAS Public Goods (LR)**	**GP Wins (LR)**	**Ward Wins (LR)**
SC Reserved	-0.010	0.150	0.131	0.532	0.320
	(0.094)	(0.130)	(0.109)	(0.463)	(0.128)
Observations	2663	2675	2563	1613	2428
Mean	-.016	-.001	.025	-.048	-.005
Bandwidth	486	565	476	290	558
Block FE	YES	YES	YES	YES	YES
	PANEL A: 3rd Tercile				
(lr)2-6	(1)	(2)	(3)	(4)	(5)
	SC-Others (Private Assets (SR))	**SC Village Public Goods (SR)**	**WAS Public Goods (LR)**	**GP Wins (LR)**	**Ward Wins (LR)**
SC Reserved	0.177	0.306	0.088	1.194	0.329
	(0.077)	(0.114)	(0.096)	(0.353)	(0.130)
Observations	2652	2675	2546	1689	2485
Mean	.065	-.002	-.006	0	-.009
Bandwidth	757	665	648	548	694
Block FE	YES	YES	YES	YES	YES

Table shows the impact of SC reservation on main outcomes, broken down by SC heterogeneity. Outcome variables are (in standardized units where not mentioned otherwise): (1) the difference between private asset scores between SCs and non-SCs in a GP in 2011-12 (measured via the Socioeconomic Caste Census) (2) Normalized share of public goods to the main SC village in the GP from Census 2011 (we trim the top 1% of observations) (3) SC Proportion-normalized share of water and sanitation (WAS) public goods provided to SC wards up to 31st March 2018 (WAS Admin Data). (4) An indicator of whether an SC won the 2016 GP elections (sample restricted to only unreserved GPs in 2016) (5) The number of SC winners from unreserved wards in the 2016 ward elections. We use CCT triangular bandwidths and estimate fuzzy RD specifications described in the paper (equation 2.2 and 2.3). We control for GP-level covariates and Block-fixed effects. All standard errors are clustered at the Block level. Panels (A), (B) and (C) show estimates when we restrict sample to the the first, second and third tercile of GPs by within-SC heterogeneity in GPs.

Table B10: Impact of SC Reservation on Overall Provision of Public Goods and Private Assets (only non-gender reserved seats)

	Private Assets			Public Goods	
(lr)2-3(lr)4-6	(1)	(2)	(3)	(4)	(5)
	Overall (Short Run)	**SC-Others (Short Run)**	**Overall (Short Run)**	**SC Village Short Run**	**SC Village Long Run**
SC Reserved	-0.007	0.194	0.046	0.203	0.188
	(0.050)	(0.065)	(0.048)	(0.091)	(0.094)
Observations	4512	4393	4512	4442	4329
Mean	.008	-.042	0	-.002	.038
Bandwidth	348	370	482	418	344
Block FE	YES	YES	YES	YES	YES
	Private Assets			Public Goods	
(lr)2-3(lr)4-6	(1)	(2)	(3)	(4)	(5)
	Overall (Short Run)	**SC-Others (Short Run)**	**Overall (Short Run)**	**SC Village Short Run**	**SC Village Long Run**
SC Reserved	-0.004	0.156	0.035	0.262	0.180
	(0.067)	(0.087)	(0.062)	(0.106)	(0.100)
Observations	4512	4393	4512	4442	4329
Mean	0	-.1	0	0	-.001
Bandwidth	174	185	241	275	275
Block FE	YES	YES	YES	YES	YES
	Private Assets			Public Goods	
(lr)2-3(lr)4-6	(1)	(2)	(3)	(4)	(5)
	Overall (Short Run)	**SC-Others (Short Run)**	**Overall (Short Run)**	**SC Village Short Run**	**SC Village Long Run**
SC Reserved	-0.012	0.176	0.022	0.170	0.157
	(0.041)	(0.054)	(0.041)	(0.075)	(0.070)
Observations	4512	4393	4512	4442	4329
Mean	0	-.1	0	0	-.001
Bandwidth	522	554	724	725	725
Block FE	YES	YES	YES	YES	YES

Table displays SD impacts of reservation on public goods and private assets in the short- and long-run. Here, we restrict sample to only non-gender reserved seats. Private assets are covered in columns (1) - (3). Columns (3) - (6) pertain to public goods. Outcome variables are (in standardized units): (1) The average private asset score across all households in a GP in 2011-12 (measured via the Socioeconomic Caste Census) (2) the difference between private asset scores between SCs and non-SCs in a GP in 2011-12 (measured via the Socioeconomic Caste Census) (3) The difference between private asset scores between SCs and non-SCs in a GP in 2018-19 (Primary Survey) (4) Public good index from 4 main public goods in the GP from Census 2011 (we trim the top 1% of observations) (5) Normalized share of public goods to the main SC village in the GP from Census 2011 (we trim the top 1% of observations) (6) SC Proportion-normalized share of water and sanitation (WAS) public goods provided to SC wards up to 31st March 2018 (WAS Admin Data). We use CCT triangular bandwidths and estimate fuzzy RD specifications described in the paper (equation 2.2 and 2.3). Only for Column (3), owing to the small number of GPs in our sample, we fix the bandwidth to be 150. We control for GP-level covariates and Block-fixed effects. All standard errors are indicated in brackets below the estimates and are clustered at the Block level. Panels (B) and (C) show estimates when bandwidth are halved or increased by a factor of 1.5 respectively.

Table B11: Impact of SC Reservation on Long-Run Political Participation of SCs (only non-gender reserved seats)

	GP		Ward		
(lr)2-3(lr)4-6	(1)	(2)	(3)	(4)	(5)
	Winner	**Candidates**	**Winner (Unreserved)**	**Candidate (Unreserved)**	**Placebo: Winner Reserved**
SC Reserved	0.076	1.405	0.172	0.736	-0.079
	(0.047)	(0.243)	(0.104)	(0.289)	(0.078)
Observations	2796	2822	4053	4053	4053
Mean	.025	.92	.742	3.019	2.369
Bandwidth	240	329	277	323	400
Block FE	YES	YES	YES	YES	YES
			PANEL B: 0.5 BW		
(lr)2-6	(1)	(2)	(3)	(4)	(5)
	Winner	**Candidates**	**Winners (Unreserved)**	**Candidates (Unreserved)**	**Placebo: Winner Reserved**
SC Reserved	0.078	1.426	0.171	0.686	-0.126
	(0.044)	(0.272)	(0.105)	(0.310)	(0.092)
Observations	2796	2822	4053	4053	4053
Mean	.021	.907	.745	2.955	2.461
Bandwidth	275	275	275	275	275
Block FE	YES	YES	YES	YES	YES
			PANEL C: 1.5 BW		
(lr)2-6	(1)	(2)	(3)	(4)	(5)
	Winner	**Candidates**	**Winners (Unreserved)**	**Candidates (Unreserved)**	**Placebo: Winner Reserved**
SC Reserved	0.118	1.268	0.259	0.758	-0.048
	(0.026)	(0.155)	(0.071)	(0.197)	(0.061)
Observations	2796	2822	4053	4053	4053
Mean	.02	.792	.797	3.007	2.11
Bandwidth	725	725	725	725	725
Block FE	YES	YES	YES	YES	YES

Table displays impacts of SC reservation on political outcomes in the long-run. Here, we restrict sample to only non-gender reserved seats. Columns (1) and (2) pertain to GP-level outcomes; Columns (3) - (5) pertain to ward level outcomes. Outcome variables are: (1) An indicator of whether an SC won the 2016 GP elections (sample restricted to only unreserved GPs in 2016, hence N = 4693) (2) The number of SC candidates contesting elections in a GP in 2016 (sample restricted to only unreserved GPs in 2016) (3) The number of SC winners from unreserved wards in the 2016 ward elections (4) The number of SC candidates from unreserved wards in the 2016 ward elections (5) The number of winners from SC-reserved wards in the 2016 ward elections (this is called a 'placebo' because it should remain unchanged trivially). We use CCT triangular bandwidths and estimate fuzzy RD specifications described in the paper (equation 2.2 and 2.3). We control for GP-level covariates and Block-fixed effects. All standard errors are clustered at the Block level. Panels (B) and (C) show estimates when bandwidth are halved or increased by a factor of 1.5 respectively.

Table B12: Impact of SC Reservation on Individual Public Goods

(lr)2-5	Share of Public Goods in the Main SC Village (1)	(2)	(3)	(4)
	Government (Schools)	**PDS**	**Anganwadi/ ICDS**	**Paved Road**
SC Reserved	0.247	0.049	0.080	0.184
	(0.075)	(0.086)	(0.063)	(0.072)
Observations	7750	6829	8041	7369
Mean	.002	-.005	.01	-.007
Bandwidth	540	514	706	605
Block FE	YES	YES	YES	YES

Outcome variables measure the population normalized share of public goods accruing to the main SC village reported in standardized units. All outcomes are calculated from the Census Village Amenities List (2011). The public goods measured are: (1) The total number of government schools in the village (2) An indicator for whether there was a PDS (subsidized ration) shop in the village (3) An indicator for whether a child nutritional center (an Anganwadi Centre/ICDS) exists in the village (4) An indicator for whether there is a paved road in the village. We use CCT triangular bandwidths and estimate fuzzy RD specifications described in the paper (equation 2.2 and 2.3). We control for GP-level covariates and Block-fixed effects. All standard errors are clustered at the Block level.

Table B13: Impact of SC Reservation on Main Outcomes (No Controls)

	Private Assets			Public Goods		
(lr)2-4(lr)5-7	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	**SC-Others (Short Run)**	**SC-Others (Long Run)**	**Overall**	**SC Village Short Run**	**SC Village Long Run**
SC Reserved	-0.014	0.058	1.148	0.022	0.191	0.177
	(0.040)	(0.049)	(0.321)	(0.068)	(0.074)	(0.074)
Observations	8177	7965	99	8220	8044	7904
Mean	.006	.027	-.008	.001	-.002	-.005
Bandwidth	565	628	150	492	585	536
Block FE	YES	YES	NO	YES	YES	YES
	GP			Ward		
(lr)2-3(lr)4-6	(1)	(2)	(3)	(4)	(5)	
	Winner	**Candidates**	**Winners (Unreserved)**	**Candidates (Unreserved)**	**Placebo: Winner Reserved**	
SC Reserved	0.126	1.386	0.209	0.766	-0.005	
	(0.026)	(0.179)	(0.073)	(0.207)	(0.067)	
Observations	4728	4765	7454	7454	7454	
Mean	.018	.845	.838	3.115	2.254	
Bandwidth	580	539	589	559	464	
Block FE	YES	YES	YES	YES	YES	

This table replicates exactly Panel (A) of B4 and Panel A of B5, but drops all controls.

Table B14: Where is Reservation Most Effective? Analysis by SC HHI (Additional Outcomes)

	LOW HHI: 1st Tercile			
(lr)2-5	(1)	(2)	(3)	(4)
	Private Assets (SR)	**Public Goods (SR)**	**GP SC Candidates (LR)**	**Ward SC (Candidtes (LR)**
SC Reserved	-0.025	0.064	1.302	0.162
	(0.073)	(0.072)	(0.250)	(0.129)
Observations	2733	2734	1664	2453
Mean	.004	-.002	-.037	.002
Bandwidth	640	541	615	587
Block FE	YES	YES	YES	YES
	LOW HHI: 1st Tercile			
(lr)2-5	(1)	(2)	(3)	(4)
	Private Assets (SR)	**Public Goods (SR)**	**GP SC Candidates (LR)**	**Ward SC (Candidtes (LR)**
SC Reserved	0.014	0.082	0.771	0.320
	(0.051)	(0.073)	(0.238)	(0.128)
Observations	2714	2714	1622	2428
Mean	.003	.003	.007	-.005
Bandwidth	585	487	389	558
Block FE	YES	YES	YES	YES
	LOW HHI: 1st Tercile			
(lr)2-5	(1)	(2)	(3)	(4)
	Private Assets (SR)	**Public Goods (SR)**	**GP SC Candidates (LR)**	**Ward SC (Candidtes (LR)**
SC Reserved	-0.003	-0.068	0.869	0.329
	(0.067)	(0.070)	(0.221)	(0.130)
Observations	2720	2720	1704	2485
Mean	-.016	-.018	-.002	-.009
Bandwidth	684	630	557	694
Block FE	YES	YES	YES	YES

Table extends Table B9 to additional outcomes. It shows the impact of SC reservation on scheme and electoral outcomes across terciles of SC population. SR indicates short run and LR indicates Long Run. All standard errors are clustered at the Block level. Panel A, B and C run separate regressions for the first, second and third tercile of SC cutoff populations respectively.

Below is the main political outcomes without non-compliers.

Table B15: Where is Reservation Most Effective? Analysis by SC Population (Additional Outcomes)

	Low SC GP Population: 1st Tercile			
(lr)2-5	(1)	(2)	(3)	(4)
	Private Assets (SR)	**Public Goods (SR)**	**GP SC Candidates (LR)**	**Ward SC (Candidtes (LR)**
SC Reserved	0.064	0.012	0.900	0.246
	(0.090)	(0.083)	(0.231)	(0.130)
Observations	2670	2670	1732	2351
Mean	.013	.007	.031	-.023
Bandwidth	222	442	378	484
Block FE	YES	YES	YES	YES
	Medium SC GP Population: 2nd Tercile			
(lr)2-5	(1)	(2)	(3)	(4)
	Private Assets (SR)	**Public Goods (SR)**	**GP SC Candidates (LR)**	**Ward SC (Candidtes (LR)**
SC Reserved	0.045	0.053	1.211	0.262
	(0.087)	(0.076)	(0.238)	(0.127)
Observations	2687	2688	1691	2460
Mean	.004	.008	.042	.007
Bandwidth	514	485	424	783
Block FE	YES	YES	YES	YES
	High SC GP Population: 3rd Tercile			
(lr)2-5	(1)	(2)	(3)	(4)
	Private Assets (SR)	**Public Goods (SR)**	**GP SC Candidates (LR)**	**Ward SC (Candidtes (LR)**
SC Reserved	-0.058	0.008	0.825	0.318
	(0.069)	(0.075)	(0.294)	(0.140)
Observations	2810	2810	1567	2555
Mean	.002	.022	-.018	-.01
Bandwidth	541	447	587	600
Block FE	YES	YES	YES	YES

Table extends Table B8 to additional outcomes. It shows the impact of SC reservation on scheme and electoral outcomes across terciles of SC population. SR indicates short run and LR indicates Long Run. All standard errors are clustered at the Block level. Panel A, B and C run separate regressions for the first, second and third tercile of SC GP HHI respectively.

Table B16: Impact of Reservation on Long-Run Private Asset Outcomes (Robustness)

	Robustness: Impact			
(lr)2-5	(1)	(2)	(3)	(4)
	SC-Others (LR)	**SC-Others (LR)**	**SC-Others (LR)**	**SC-Others (LR)**
SC Reserved	1.188	1.300	1.804	1.252
	(0.255)	(0.252)	(0.544)	(0.278)
Observations	100	100	100	100
Mean	0	.017	-.125	-.214
Bandwidth	100	76	90	45
Block FE	YES	YES	YES	YES
BW Selection/Kernel	Fixed 100 (Triangular)	CCT (Triangular)	CCT (Triangular)	CCT (Uniform)

Table shows the impact of SC reservation on our the private asset score under different bandwidths and specifications.

Table B17: Impact of SC Reservation on Public Goods/Private Assets in the GP (Drop Mismatched GPs)

	Private Assets			Public Goods		
(lr)2-4(lr)5-7	(1)	(2)	(3)	(4)	(5)	(6)
	Overall (Short Run)	**SC-Others (Short Run)**	**SC-Others (Long Run)**	**Overall (Short Run)**	**SC Village Short Run**	**SC Village Long Run**
SC Reserved	0.001	0.090	1.115	0.025	0.222	0.151
	(0.038)	(0.050)	(0.258)	(0.043)	(0.076)	(0.066)
Observations	8092	7882	42	8093	7968	7685
Mean	0	-.006	0	.014	-.006	.002
Bandwidth	544	521	50	416	475	472
Block FE	YES	YES	YES	YES	YES	YES

This reproduces Table B4 but drops GPs where there is a mismatch between what the algorithm predicts and what the data shows. Table displays SD impacts of reservation on public goods and private assets in the short- and long-run.

Table B18: Impact of SC Reservation on Long-Run Political Participation of SCs (Drop Mismatched GPs)

	GP			Ward	
(lr)2-3(lr)4-6	(1)	(2)	(3)	(4)	(5)
	Winner	**Candidates**	**Winner (Unreserved)**	**Candidate (Unreserved)**	**Placebo: Winner Reserved**
SC Reserved	0.087	1.424	0.220	0.794	-0.057
	(0.033)	(0.211)	(0.087)	(0.265)	(0.104)
Observations	4633	4670	7300	7300	7300
Mean	.019	.897	.835	3.112	2.236
Bandwidth	341	373	498	454	476
Block FE	YES	YES	YES	YES	YES

Table replicates Table B5, but drops GPs where there is a mismatch between what the algorithm predicts and what the data shows. It displays the impacts of SC reservation on political outcomes in the long-run.

CHAPTER 3

SELECTION, SORTING, AND DISCRIMINATION IN LABOR SUPPLY DECISIONS

3.1 Introduction

Prejudice, social norms, and cultural barriers keep productive people out of the labor market. Besides the obvious human costs, this misallocation of talent can constrain economic growth as well. In the US, for example, 20% to 40% of output growth between 1960 and 2010 can be explained by improved allocation of talent, especially the entry of talented black and women candidates in high-skilled occupations [Hsieh et al., 2019].

The premise of this project is that misallocation of entrepreneurial talent can prevent a country from achieving its growth potential as well. I focus on the world's 2nd most populous country and 5th largest economy, India. Like most developing countries, self-employment is the dominant form of work. However, minority groups are often underrepresented in leadership positions. For example, in the state of Bihar minorities run only 3% of registered enterprises although their share in the total population is 17% [Iyer et al., 2013].

While there are many reasons for minority under-representation in enterprise ownership, discrimination in factor and product markets is widespread and likely important. However, we lack credible empirical evidence on the extent of discrimination that minority entrepreneurs face in some of the markets, underlying motives for discrimination, and ways in which persistent discriminatory behavior has influenced their economic decisions. This project aims to fill that gap.

The focus of this study is on a specific type of discrimination: employees' discrimination against minority employers. For the large majority of owner-run economic enterprises, especially in the developing world, access to quality labor tends to be one the most critical inputs. More often than not, labor pools are dominated by non-minority groups given their higher

population share and historical advantage in access to education. In such economic settings, prejudiced employees could impose significant economic costs on minority employers: they could discriminate by either avoiding working for minority employers or by exerting lower effort when they decide to work for them. However, economists have not yet investigated whether minority employers end up paying the penalty in labor markets because of potential discrimination by workers they want to hire. In other words, are minority entrepreneurs unable to attract quality labor because non-minority workers have a distaste for minority bosses? Is this penalty big enough to act as a hindrance for minority entrepreneurs to succeed or prevent them from taking up managerial positions?

While there is a huge body of empirical literature on top-down (employer) discrimination across the world, there are few field experiments on the phenomenon of bottom-up (employee) discrimination [Bertrand and Duflo, 2017]. I propose a field experiment to test for the presence of bottom-up discrimination and shed light on possible underlying motivations. The experiment tries to detect bottom-up discrimination in a high-stakes field setting: the market for call center executives in the state of Bihar, India. The experimental design allows me to test for two potential underlying motives for discrimination. First, I investigate whether job applicants are less interested in acquiring additional information about minority employers. This would help us study the role of 'information avoidance' in the persistence of discrimination against minorities. Second, I formally test the extent to which this discrimination is influenced by concern for social approval as opposed to intrinsic distaste for minority groups. I also test whether this is a case of ethnic sorting or discrimination against minorities in particular.

To demonstrate proof of concept, I have conducted a pilot to test the feasibility of the experimental design. The results show that minority employers face discrimination from potential workers: job applicants who are randomly assigned to see a job ad with a minority employer are 3 percent points (26%) less likely to apply compared to the control group who

see a job ad with a non-minority employer. Furthermore, applicants who see a job description page with a minority employer are 9 percentage points (64%) less likely to acquire additional information about their potential employers.

3.2 The present state of knowledge

To run economic enterprises successfully, one has to engage in a large number of markets. Naturally, minority entrepreneurs are likely to face discrimination across multiple markets. For example, there are several studies suggesting that minority-run businesses face discrimination in accessing credit (Blanchard et al. [2008]; Blanchflower et al. [2003]). We have also seen some recent experimental evidence on discrimination against minorities in product markets (Doleac and Stein [2013]; List [2004]). Doleac and Stein [2013], for instance, studies discrimination in an online platform where iPods are sold through classified advertisement. Using experimental variation in skin-color of the hand as a signal for race, she finds that black sellers receive fewer and lower offers than white sellers.

However, possible discrimination in the market for a critical input of production, labor, has received surprisingly little attention. While Becker (Becker [1971] theorized several types of possible discrimination in labor markets — employer, customer, employee — the ensuing theoretical and empirical work has largely looked at employer discrimination (Neumark [2018]; Banerjee et al. [2009], Bertrand and Mullainathan [2004]). There has also been some progress on the idea of customer discrimination and co-worker discrimination (Hedegaard and Tyran [2018]; Combes et al. [2016]). The idea that employees could also discriminate against employers, however, hasn't been explored much, both by theorists and empirical economists. The poverty of research in this area is perhaps due to the assumption that the employees, even when they hold prejudice against employers of a certain type, may not be in a position to act on their prejudice, or their actions may not have much economic significance. This assumption, logical as it may sound, needs to be empirically investigated

as it may not hold in many economic settings.

3.2.1 Bottom-up Discrimination in Labor Markets

In the last couple of years, there has been some attempt to explore the idea of discrimination from below (Ayalew et al. [2021]; Abel [2022]; Asad et al. [2020] Ayalew et al. [2021] recruit university administrators in Ethiopia to play 'leadership' games. The results reveal that the study participants are 10 percent less likely to follow the advice from a randomly assigned female leader than an otherwise identical male leader. Asad et al. [2020], on the other hand, recruit white workers from an online platform – Amazon's Mechanical Turk – to work on a task of alternately press 'a' and 'b' on a keyboard for up to ten minutes. Using experimental variation in the race of the employer, they find that white workers do not discriminate against black employers.

While these papers have drawn attention to a dormant area of research, they suffer from several limitations. First, they both run lab-in-the-field type experiments that rely on fictitious economic tasks that have no substantive meaning. Second, the experiments are not based in real economic settings and participants face very low stakes, and therefore they fail to establish whether discrimination from below is a real phenomenon with real economic implications. Third, focus of these papers seems to be on the intensive margin: changes in effort level given a particular employer. They have completely ignored the extensive margin that might have bigger economic implications: whether high-quality workers are unwilling to work for minority employers.

Unlike most field experiments on discrimination in labor markets, my experimental design allows me to dig deeper into the underlying motivations for discrimination from below. First, I can test whether this is a case of general ethnic sorting of workers or is there discrimination against minority employers in particular. This is possible because the unique research design allows me to observe the group identity of both employers and their potential employees.

Second, this experiment is designed to formally test the extent to which this discrimination is influenced by concern for social approval as opposed to intrinsic distaste for minority groups. The theoretical framework for this test comes from the model of 'spontaneous discrimination' [Peski and Szentes, 2013]. Their model shows that a particular group could discriminate against the out-group not only due to their distaste (or beliefs that out-groups are likely to be of low-type), but also because such discriminatory behavior is expected and tolerated by other members of their group.

The proposed experiments hope to address some of these limitations and make a substantive contribution to the nascent literature in this area.

3.3 Experimental Design

3.3.1 Research Questions

This paper aims to answer two main research questions: 1) Do minority employers face discrimination from below in labor markets? 2) What are the underlying motivations? What is the extent to which this discrimination is influenced by concern for social approval as opposed to intrinsic distaste for minority groups?

3.3.2 Field Setting

I plan to conduct this experiment in the state of Bihar, India. Bihar is the third most populous state in India, with a population of more than 100 million. Bihar is one of the least developed states where social identities — gender, caste, and religion — continue to influence social and economic interaction. The focus of the study would be on two minority groups –Muslims and Women – who face widespread discrimination.

This experiment will be conducted online using Facebook's 'Jobs on Facebook' feature, which is increasingly used by employers to recruit entry-level workers. The job ads on

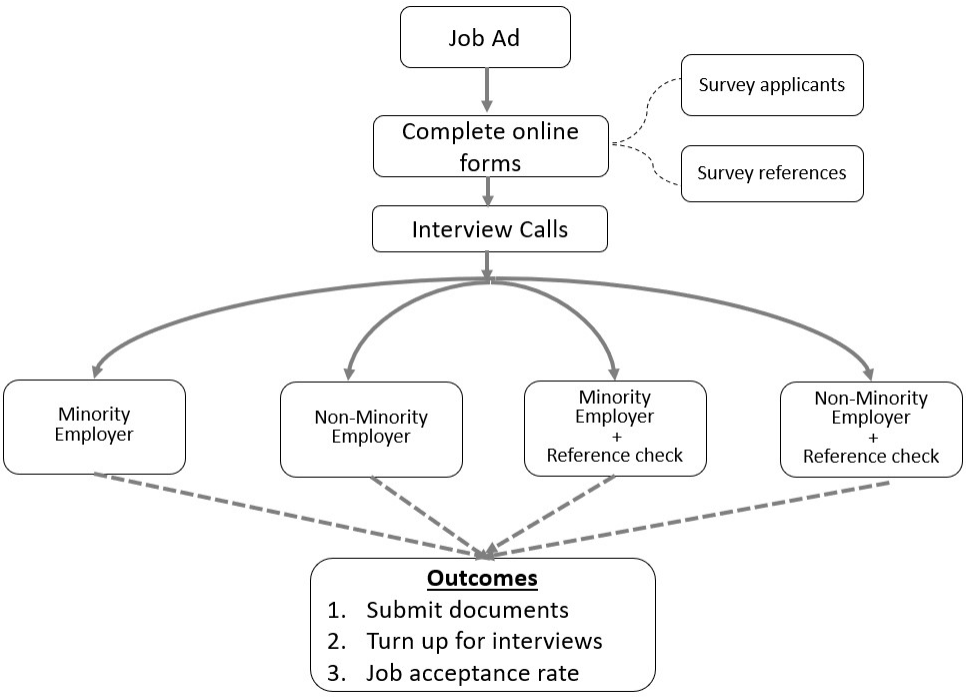


Figure 3.1: Experimental Design
This figure explains the experimental design of the study.

Facebook are extremely cost-effective as there is a large number of active Facebook users (260 million).

3.3.3 Experimental Design

There are two main ways in which prejudiced workers could discriminate against minority employers. First, they could decide against working for minority employers or put in a lower effort conditional on working. This paper focuses on the extensive margin: it tests whether job applicants are less likely to apply for and accept job offers from minority employers.

Figure 3.1 explains the experimental design for the proposed study. The experiment will be implemented as follows:

Recruitment

Using a "Bihar Job Connect" page on Facebook, I'll post a general ad that asks candidates interested in entry-level jobs —data entry operator, call center executives—to sign up. The candidates will be asked to provide info on basic demographics, prior work experience, and names and phone numbers of 3-5 professional references. Selected candidates will then be informed about a new job opening: phone surveyors for a survey firm.

Randomization

Each candidate who completes the sign-up form will receive a call from the "Bihar Job Connect" office informing them about the next steps. The script for the call would be identical in terms of info on the nature of the job and location, but I will experimentally vary the name of the firm and whether the firm will carry out reference checks using the data on references received during the 'sign-up' process.

Treatment Arms

The job applicants will be randomized into one of the following experimental groups:

1. **Minority Employer**: Candidates will be informed that they are being considered by the firm 'xyz'(minority-sounding name) and should submit the required documents if interested in getting interviewed. In case you have more questions about the job at firm 'xyz', we can request them to give you a call. Would you be interested in receiving a call from 'xyz' representative?

2. **Non-Minority Employer**: Candidates will be informed that they are being considered by the firm 'xyz'(non-minority-sounding name) and should submit the required documents if interested in getting interviewed.

3. **Minority Employer+ Reference Check**: Candidates will be informed that they are being considered by the firm 'xyz'(minority-sounding name) and should submit the required documents if interested in getting interviewed. They will also receive this additional information during the call: *"In case you show interest in working for the firm 'xyz' by submitting required documents over Whatsapp, a representative from the firm 'xyz' will call up your references 'abc" (names will be inserted using the data from sign up form) and inform them that you have applied to work for the firm 'xyz' and verify your prior work experience or education credentials."* This information about the potential call from the firm would change candidates' beliefs about the observability of their decision to apply for an employer with a given social identity.

4. **Non-Minority Employer+ Reference Check**: Candidates will be informed that they are being considered by the firm 'xyz'(non-minority-sounding name) and should submit the required documents if interested in getting interviewed. They will also receive information about the reference check from the potential employer as before.

3.3.4 Outcomes of interest

I will look at for main outcomes. First, the likelihood of submitting required documents over WhatsApp in order to be considered for the interview call. Second, the quality of applicants who submit the documents. Third, the likelihood of turning up for an in-person interview. Fourth, the likelihood of accepting a job offer and turning up for work.

In addition to these four outcomes, I'll also monitor the information acquisition behavior of applicants: whether they show interest in receiving a call from the potential employer, do they ask additional questions during the interview call invitation, do they ask questions over WhatsApp, classify the nature of these questions to test if they are more concerned about certain features of the job offered my minority employers.

3.4 Pilot

3.4.1 Key objectives

This pilot had three main objectives. First, to test the feasibility of registering new firms for the study and whether these firms can recruit and retain workers. Second, to test whether Facebook is an effective platform for recruiting entry-level workers. Third, test whether spillovers are a concern if the unit of randomization is at the applicant level. Fourth, to estimate the level of discrimination faced by minority employers at different stages of application. Fifth, to test the design for unpacking two potential underlying motives for discrimination: 'attention discrimination' and 'social image concerns.'

3.4.2 Design

The experimental design for this pilot is explained in Figure 3.2. Using a Facebook page created earlier, I advertised for the job of field surveyors. The applicants who clicked on the ad link were randomly directed to one of the two job description pages. The job description pages were identical in all respect except for one: the employer's name. I used minority-sounding and non-minority-sounding firm names that were registered for this experiment. Each pool of candidates who show interest in applying for the job after seeing the job description page is then randomized into two different experimental groups: 1. application form with references section 2. application form with no-references section. This randomization in the online application form is done to test whether 'social image' concerns could drive the difference in application completion rates. Within each employer type, we randomly ask half of the applicants to provide the names and contact details of three references that the employers could reach out to verify the applicant's credentials.

I examine three main outcomes to measure application completion: 1. Whether candidates are interested in applying for the job 1. Whether the online application form is

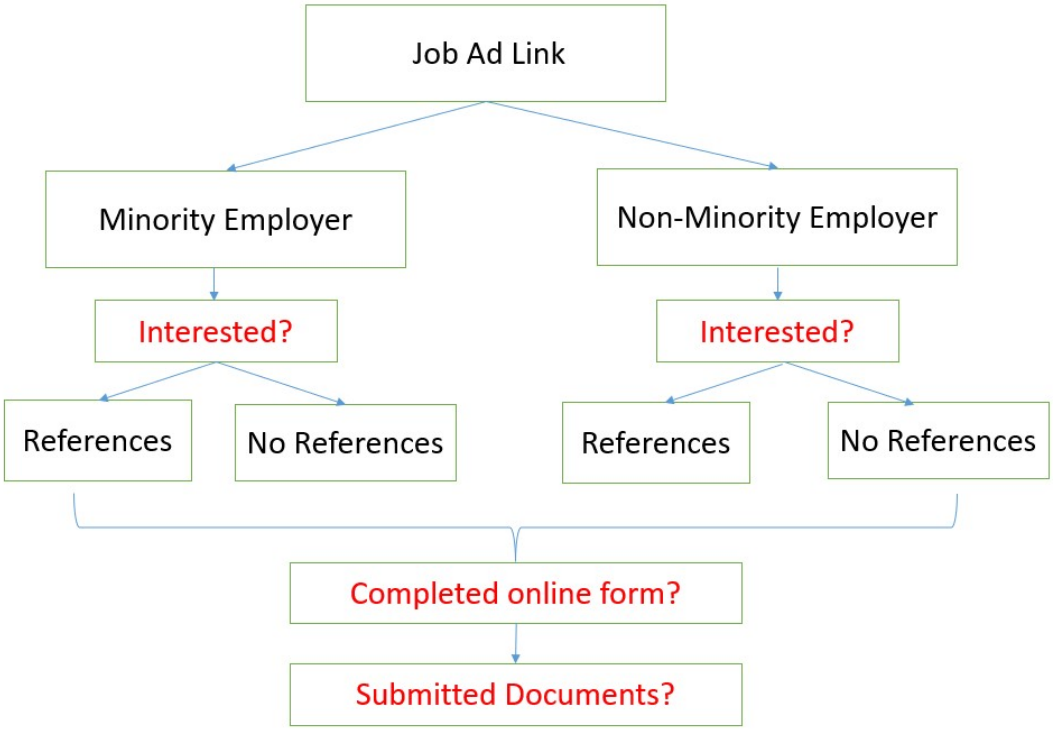


Figure 3.2: Pilot Experimental Design
This figure displays how the pilot study was implemented. The randomization of applicants was implemented sequentially. First, on clicking the job link, applicants are randomly assigned to see a job description with a minority or non-minority employer. Second, the pool of applicants who show interest in applying for the job is again randomized into two groups: they either receive an application form with a references section or without it. In the end, we track the application completion rate and submission of additional documents.

completed 3. Whether the required documents are submitted for the interview.

3.4.3 Results

The Job Ad generated thousands of clicks, of which 3,124 applicants met the selection criteria and were directed to one of the two job description pages. 80% of these applicants show interest in the job and are then directed to fill out an online form. 50% of the applicants fill out the online form, but only 5% submit the required documents over WhatsApp.

I test for discrimination against minority employers on three different stages of application. I first look at whether applicants assigned to see a job description page with a minority

employer are less likely to show interest in filling out the form compared to those who see the job description page with a non-minority employer (Table 3.1). I do not find any evidence of discrimination at this stage as shown in Table 3.1 (column 1). Inters tingly, there is a great deal of heterogeneity in treatment effects across time. This suggests the possibility of spillover effects using this randomization strategy.

Table 3.1: Interested in Applying for the Job

	Interested in Applying			
(lr)2-5	(1)	(2)	(3)	(4)
	All Days	**Day1**	**Day2**	**Day3**
Minority Employer	-0.005	-0.086**	0.008	0.005
	(0.015)	(0.037)	(0.022)	(0.026)
Observations	3124	463	1364	1041
Control Mean	.79	.84	.76	.76
Controls	None	None	None	None

This table delineates the impact of assigning candidates to see a job description page with a minority employer on their interest in applying for the job. The dependent variable in all columns is whether the candidates want to apply for the job. Column 1 presents results based on the complete sample. Column 2 restricts the sample to day 1, column 2 to day 3, and column 4 to day 3. Standard errors are robust to heteroskedasticity and reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

I now turn to the second stage of application: completion of the online form. Here, we find that candidates assigned to a minority employer are 2 percentage points (pp) less likely to complete the application form (3.2, col 1). Furthermore, we find some suggestive evidence that social image concerns might be an important underlying motivation for discriminating against minority employers. Applicants assigned to minority employers who are asked to provide references in the application form are 4 p.p. (p=0.26) less like to complete the form compared to their non-minority counterparts.

Finally, candidates who completed the online form were asked to submit a few documents which would determine if they will be considered for a job interview. Candidates assigned to a minority employer are 3 p.p. (26 %, p=0.02) less likely to submit required documents (Table

Table 3.2: Do applicants complete the online application form?

(lr)2-3	Complete Online Application Form	
	(1)	(2)
	Complete Form	**Complete Form**
Minority Employer	-0.028	-0.002
	(0.020)	(0.027)
References Section		-0.222***
		(0.026)
Minority*References		-0.042
		(0.038)
Observations	2461	2461
Control Mean	.5	.5
Controls	None	None

This table delineates the impact of assigning candidates to see a job description page with a minority employer on their likelihood of application completion. The dependent variable in both columns is whether candidates complete the online application form. Column 1 presents the results for the main treatment: a job description page with minority sounding firm name. Column 2 presents the differential effects of a minority-sounding firm name and the introduction of the references section in the application form. Standard errors are robust to heteroskedasticity and reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

3.3). This suggests that as the cost of applying increases, we find greater discrimination against minority employers.

Attention Discrimination

I test for attention discrimination by checking whether the information acquisition behavior of applicants (about their potential employers) varies depending on the social identity of employers. I measure this by monitoring whether applicants click on the link in the job description page that says, *"click here to learn more about the firm XYZ"*.

The results of this test are presented in table **??**. I find that applicants assigned to minority employers are 9 p.p. (64 %) less likely to click on the link about their potential employers compared to candidates assigned to non-minority employers. This shows that applicants are

Table 3.3: Do applicants submit required documents?

	Submit Documents	
(lr)2-3	(1)	(2)
	Submit Documents	**Submit Documents**
Minority Employer	-0.034**	-0.021
	(0.016)	(0.020)
References Section		0.037*
		(0.022)
Minority*References		-0.033
		(0.032)
Observations	1527	1527
Control Mean	.12	.12
Controls	None	None

This table delineates the impact of assigning candidates to see a job description page with a minority employer on their likelihood of submitting required documents. The dependent variable in both columns is whether candidates submit the required documents. Column 1 presents the results for the main treatment: a job description page with minority sounding firm name. Column 2 presents the differential effects of a minority-sounding firm name and the introduction of the references section in the application form. Standard errors are robust to heteroskedasticity and reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

less 'attentive' when assigned to see a job from a minority employer. Alternatively, one could argue that the applicants are less serious about applying for a job with a minority employer and, therefore, do not want to invest more time in acquiring additional information about them.

3.5 Conclusions

Minority employers can face considerable discrimination from the potential workers they want to hire. Preliminary results of a pilot study show that applicants are 3 p.p. (26%) less likely to apply for jobs advertised by minority employers. I also find strong evidence for 'attention discrimination' against minority employers

Table 3.4: Do applicants acquire additional information about employers?

	Clicked			
(lr)2-5	(1)	(2)	(3)	(4)
	All Days	**Day1**	**Day2**	**Day3**
Minority Employer	-0.092***	-0.035	-0.096***	-0.113***
	(0.010)	(0.028)	(0.016)	(0.018)
Observations	3124	463	1364	1041
Control Mean	.14	.12	.15	.15
Controls	None	None	None	None

This table delineates the impact of assigning candidates to see a job description page with a minority employer on their likelihood of clicking on a link to acquire additional information about their potential employers. The dependent variable in all columns is whether the candidates click on the link about their employers. Column 1 presents results based on the complete sample. Column 2 restricts the sample to day 1, column 2 to day 3, and column 4 to day 3. Standard errors are robust to heteroskedasticity and reported in parentheses $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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