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ABSTRACT

Political inclusion is widely believed to improve governance, motivating the creation of elected representatives for highly localized constituencies. This paper studies 1.2 million "hyperlocal" representatives across 150,000 local governments in rural India. Exploiting discontinuities that determine the number and identity of these representatives, we assess how expanded representation affects governance outcomes. We find precisely estimated null effects on core functions, including public project management, intermediation in access to benefit programs, alignment of policy with citizen preferences, equity of benefit allocation, and oversight of public finances. These findings highlight the limits of expanding political representation if representative capacity is weak.

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1 Introduction

Developing countries have increasingly embraced decentralization reforms. A central premise of these initiatives is that expanding local political representation can improve the delivery of public goods by narrowing information and accountability gaps between constituents and decision-makers. This logic has motivated the introduction of a class of elected representatives for highly localized constituencies, sometimes encompassing as few as a hundred people. These "hyperlocal" representatives appear to constitute the world's largest class of elected officials, with at least five million active across many countries. Yet despite their prevalence, there is little evidence on how expanding representation at the hyperlocal margin affects governance.

Many classic theories highlight channels through which expanding political representation could affect governance outcomes. More localized representatives may face stronger accountability through social and electoral sanctions (Seabright, 1996; Boffa et al., 2016), be more accessible to citizens in facilitating access to government-provided benefits (Bussell, 2019), or better understand or align with constituent preferences (Tiebout, 1956; Oates, 1972). Increased proximity between voters and representatives may particularly benefit marginalized communities that would otherwise be politically excluded. At the same time, additional representatives can generate coordination costs, act as additional veto players that stall decision-making (Tsebelis, 2002), or lack the capacity or policy knowledge to influence outcomes (Auerbach et al., 2023). The strength of each channel depends on the scale of representation: for example, coordination costs grow with increased localization, while accountability gains may diminish. The net effect of expansion depends on the current level of representation relative to the optimum implied by these tradeoffs.

This paper studies the largest group of hyperlocal representatives globally: ward members in India's village councils (gram panchayats, or GPs). GPs oversee local public goods provision, assist in national welfare schemes, and implement a major workfare program, among other functions. Each GP includes a single elected head and multiple ward members elected from geographically distinct wards. There are over 2.6 million ward members across India, typically serving constituencies of one hundred to one thousand residents, and their functions are comparable to hyperlocal representatives in Indonesia (Antlöv et al., 2016) or China (Shi, 1999).

Our analysis examines four core tasks of these representatives: (1) supporting project implementation; (2) intermediating citizen access to public benefits; (3) aligning policy with citizen preferences; and (4) exercising oversight over public projects and finances. We assemble a novel dataset of over 1.2 million ward members across 150,000 gram panchayats in 13 Indian states, home to nearly 600 million constituents.

We investigate two margins of representation: "density" (the ratio of representatives to residents) and identity. To study density, we exploit state-specific population thresholds at which the number of hyperlocal representatives increases discretely. For example, in Tamil Nadu, a GP with under 2,000 residents has six ward members, while one just above has nine. We use these thresholds in a regression discontinuity design to estimate the effects of increased density.

Expanding representation often broadens who serves in political office. To study the impact of representative identity, we exploit a rule reserving some ward member positions for members of a marginalized group (Scheduled Castes, or SCs). The number of these reserved seats increases in a stepwise fashion with the SC population share of the GP. We use these thresholds in a regression discontinuity framework to estimate the causal effects of increased hyperlocal representation from marginalized communities.

We first examine how hyperlocal representatives affect the aggregate level of public services. Denser representation could improve service delivery through channels such as intermediation (more intermediaries available to help citizens access public entitlements) or project management (more leaders to identify local needs and oversee implementation). Representative identity may also increase the aggregate level of services: marginalized citizens, who often lack political networks, could see total benefits rise with greater representation of their group, while access for more advantaged citizens is more inelastic to representation composition. Using data on the NREGS workfare program, eight major public programs, and a measure of alignment between local citizen priorities and local public spending, we find no evidence that either greater density or increased representation of marginalized groups improves any of these outcomes. Our estimates are precise, ruling out improvements as small as 0.017 standard deviations on an index of workfare outcomes.

We then turn to the distribution of benefits. With more ward members, each representing a geographically distinct area, benefits may shift toward previously neglected areas; with more SC representation, a greater share may flow towards SC communities (Pande, 2003). We bring several novel data sources to bear on these questions, including geocoded data on the location of tens of millions of NREGS projects, SC receipt of NREGS work and other public benefits, and measures of whether benefits disproportionately flow to underserved areas or those with more SC residents. Across all of these measures, we find no evidence that either increased density or greater SC representation shifts the distributional outcomes.

Finally, we test whether hyperlocal representatives influence accountability and corruption. Hyperlocal representatives may increase oversight, particularly when they come from groups that otherwise lack access to power, but could instead increase corruption by creating additional demands for rents. Using three years of financial audits of GP accounts and nine years of social audits of the NREGS workfare program, we find no effect of either representative density or identity on the incidence and composition of irregularities, with sufficient power to rule out even modest effects.

Our analysis estimates the effects of marginal increases in representative density and representation of marginalized groups. One interpretation is that hyperlocal representatives have limited impacts in this setting. Alternatively, the results could reflect features of our empirical strategy: for example, representation may exhibit diminishing returns, with our estimates dominated by ranges where additional representation has little effect. The final section of the paper conducts seven empirical tests to probe these and other explanations, concluding that our results are best explained by low capacity and power of ward members.

Our paper contributes to the large literature emphasizing how political representation can improve governance through responsiveness, accountability, and alignment with citizen preferences (e.g., Oates, 1972; Seabright, 1996; Besley, 2006; Besley and Coate, 2003; Ashworth, 2012). At the same time, it is unclear whether representation alone—absent direct control over budgets, personnel, or formal policy decisions—is sufficient to generate such improvements (e.g., Mansbridge, 1999; Bardhan and Mookherjee, 2006). Even without such authority, representatives may still matter by providing an institutionalized channel to voice citizen grievances and preferences, legitimizing citizen claims in bureaucratic interactions, or serving as a focal point for oversight when citizens have weak individual incentives to conduct monitoring (Besley and Burgess, 2002; Björkman and Svensson, 2009). Hyperlocal representatives provide a particularly clean setting to examine this question, as they maximize proximity to citizens while holding limited formal authority. Our findings shed light on the conditions under which political inclusion translates into effective governance, offering a cautionary tale particularly relevant for decentralization initiatives. They indicate that simply expanding representation need not guarantee improvements in public service delivery without complementary investments in authority or training.

We also contribute to the literature on how the institutional design of local government shapes service delivery in developing countries. A growing body of work studies how local political institutions—such as democratization (Olken, 2010; Martinez-Bravo et al., 2022), political boundaries (e.g., Narasimhan and Weaver, 2024; Dahis and Szerman, 2025), and the size of municipal legislatures (Britto and Fiorin, 2020; Freire et al., 2023)—affect government spending and public goods provision. Related research has particularly focused on the identity or incentives of heads of local governments, with much of this work examining affirmative action (e.g., Chattopadhyay and Duflo, 2004; Beaman et al., 2009; Sharan and Kumar, 2021). We complement this literature by shifting attention from executive authorities to a ubiquitous but understudied feature of local governance: hyperlocal representatives.

2 Background

Village councils, known as gram panchayats (GPs), have existed in India since precolonial times, but the 73^{rd} Amendment to the Constitution in 1992 established a uniform national framework for decentralization and village-level elections. Similar reforms were introduced around the same time in other large developing countries, including Indonesia and China. The 73^{rd} Amendment devolved 29 areas of responsibility to GPs, including education, sanitation, and drinking water. GPs also implement large-scale public programs, most notably the National Rural Employment Guarantee Scheme (NREGS) workfare program. Beyond these formal responsibilities, political science research highlights the informal influence of GP leaders in facilitating citizen access to government services (Chauchard, 2017; Bussell, 2019): Kruks-Wisner (2018) finds that 62% of respondents seeking welfare assistance turned to members of the village council.

The political leadership of a GP comprises a council head and ward members.[1] GPs are typically divided into five to twenty wards of approximately equal population, with one elected ward member each. Average ward size varies across states, ranging from only 100-200 people in some states to over a thousand in others (Table A1). The political leadership is supported by administrative staff such as a panchayat secretary (Kerala Institute of Local Administration, 2023). In most states, both head and ward member positions are filled via simultaneous, nonpartisan, first-past-the-post elections held every five years. Although there are no formal term limits, India's political reservation system restricts candidacy in some seats to women and/or members of particular caste groups.

One major constraint for research on hyperlocal representatives globally has been the lack of data on this level of government. As described in Appendix B, we overcome this issue by combining five data sources to construct a data set of 2.07 million currently active ward members across 209,531 GPs, representing 78% of all GPs in India; approximately 1.2 million are in our 13 sample states. Reflecting reservation policies, women constitute 50.8% of ward members. Caste composition is broad-based, with 24.6% general caste, 39.3% OBC, 23.4% SC, and 12.7% Scheduled Tribe. Ward members are typically middle-aged and have low levels of formal education: 41.2% are illiterate or have less than primary schooling, raising concerns that education may constrain their capacity.

As described in Ministry of Rural Development training materials, the role of ward

[1] Terminology varies across states (e.g., pradhan, mukhiya, sarpanch). We use "head" and "ward member" throughout.

members is to identify and address problems in their wards, ensure last-mile delivery of government schemes to their constituents, participate in local government decision-making and administration, and oversee specific local government functions, particularly through membership on committees of the gram panchayat (Ministry of Rural Development, 2020). As part of this, they should participate in meetings of the gram panchayat, which are supposed to have a quorum of ward members present and occur at least monthly. Ward members are typically unpaid or receive only a small honorarium so usually have another form of employment (Ministry of Panchayati Raj, 2022).

In practice, member efficacy depends on institutional factors. The head is generally seen as the most powerful figure in GP governance due to their control over finances (Ministry of Rural Development, 2020; Sharan and Kumar, 2021). As training materials describe, ward members still may have a meaningful role by representing citizen interests with the head, assisting in implementation, and monitoring GP activities, but their limited ability to impose formal sanctions may hamper their capabilities. Aside from small-scale qualitative accounts (e.g., HCL Foundation, 2020; Harinath, 2024), there is little rigorous evidence on intra-council power dynamics and how hyperlocal representatives influence local governance.

3 Empirical Strategy

Our main analysis uses two regression discontinuity (RD) designs to study how hyperlocal representation affects public goods delivery. The first design focuses on representative density, measured as the ratio of ward members to constituents. In eight states, statutory rules increase the number of ward members at discrete population thresholds based on GP population in the prior decennial census. Table A1 shows how these rules vary across states: for example, in Punjab, the number of ward members increases from 5 to 7 at a population of 1,000 and from 7 to 9 at 2,000.

These rules generate sharp discontinuities in representative density: GPs just above a threshold have discretely higher representative density than those below. In these states, we estimate the specification:

$$Y_{g,s,t} = \gamma_0 + \gamma_1 \mathbf{1}(p_{g,s} \geq 0) + \gamma_2 f_1(p_{g,s}) + \gamma_3 f_2(p_{g,s}) \cdot \mathbf{1}(p_{g,s} \geq 0) + \epsilon_{g,s} \tag{1}$$

where $Y_{g,s,t}$ is the outcome of interest in GP g in state s at time t. The running variable $p_{g,s}$ is defined as the difference between GP g's population in the relevant census year and the nearest population threshold in its state (e.g., 1,000, 2,000, and higher thresholds in the example of Punjab).[2] GPs are assigned to their nearest threshold, and the running variable is centered at zero for each. This stacks observations across multiple thresholds, so γ_1 represents a weighted average treatment effect across several discontinuities, which we later disaggregate. The functions $f_1(p_{g,s})$ and $f_2(p_{g,s})$ allow for flexible functional forms on either side of the cutoff. Estimation follows Calonico et al. (2014), using a triangular kernel, local linear regression, and MSE-optimal bandwidths as in Calonico et al. (2018). When we observe multiple years of data for the same GP, we pool observations across years and cluster standard errors at the GP level.[3]

The second RD design focuses on the identity of representatives, and specifically on whether ward members belong to Scheduled Castes. Some ward member positions are reserved for SC candidates, meaning that only SCs may contest these seats. In some states, the number of reserved positions are proportional to the SC share of the GP population in the previous census. For a GP with n ward members, this generates stepwise increases in the number of SC ward members at population shares of $(\frac{1}{2n}, \frac{3}{2n}, \ldots)$. For example, in a GP with 11 wards, the number of reserved SC seats increases from zero to one at an SC population share of 1/22, from one to two at 3/22, and so on. Restricting to the seven states that we can verify follow this rule, we estimate the same specification as in equation 1, redefining $p_{g,s}$ as the difference between the GP's SC population share and the nearest discontinuity.

We verify the first stage for both designs — whether these thresholds are followed in practice — using the data on ward members discussed in section 2. For the density RD, column (1) of Table 1 and Panel A of Figure 1 estimate Equation 1 with the actual number of ward members as the outcome: GPs just above the threshold have 1.39 more ward members than those below. Table A2 confirms the first stage for each state individually. For the identity RD, column (1) of Panel B of Table 1 and Panel A of Figure 2 show that crossing the reservation threshold increases the number of SC ward members by 0.40 (SC share of ward members by 4.4 percentage points; column 2). In both designs, the institutional rules generate clear shifts in representation at the relevant thresholds.

Two identification assumptions underpin our designs. First, the running variable should not be manipulable around the threshold. Manipulation is quite unlikely given the integrity of India's census process (Asher and Novosad, 2020; Burlig and Preonas, 2024), and there is no evidence of bunching at either cutoff (Cattaneo et al. (2018), p-values of 0.73 and

[2]For construction of the running variable, we match villages to GPs using the Local Government Directory (Ministry of the Panchayat Raj, 2020) and take village-level population data from the Census of India (2001, 2011).

[3]Outcome data span 2016 to 2025, but ward delimitation typically occurs in the first election cycle after the 2011 census, and boundaries have remained relatively fixed. Our estimates therefore reflect accumulated effects since the initial delimitation.

0.84; Figure A1). Second, covariates should vary smoothly at the threshold. The "Baseline covariates" columns of Table 1 show no significant discontinuities in village characteristics from the prior Census.[4] Appendix B provides more details on the data, while Appendix B.9 and Table A3 conduct robustness checks related to matching across datasets.[5]

4 Results

4.1 Project management, intermediation, and alignment of policy with citizen preferences

This section examines how hyperlocal representatives affect the aggregate level of public services, focusing on two channels through which they are envisioned to operate. First, they may play a role in the implementation of public projects. Local governments in India oversee a wide range of projects such as roads, drinking water, sanitation, and workfare. Ward members are expected to identify and propose projects addressing local needs and to monitor ongoing projects to ensure quality (Ministry of Rural Development, 2020). Second, hyperlocal representatives may serve as intermediaries between citizens and the state, especially for accessing programs that require navigating eligibility rules and application processes (e.g., ration cards, pensions). Many residents of developing countries face significant barriers to accessing such entitlements due to illiteracy, administrative complexity, or limited political networks (Gupta, 2025). Ward members could bridge these gaps by disseminating information, assisting with applications, or advocating for citizen claims (Kruks-Wisner, 2018).

Both of the margins that we study — density and identity — could affect the total level of public benefits through these channels. For density, additional ward members may expand the capacity to propose public projects or oversee their implementation; they could also worsen outcomes by increasing coordination problems or rent-seeking. If intermediation is costly or depends on personal connections between citizens and representatives, greater density expands the stock of intermediaries and could increase aggregate access to benefit programs. For identity, both the placement of local public projects and access to benefit programs can depend on citizens' relationships with political figures. More marginalized citizens, who often lack such networks, could see total benefits rise when representatives

[4] Another concern is that other programs may share the thresholds we use. We searched for and removed one such case (Uttar Pradesh, 1,000-person threshold). Any remaining overlap would bias us toward finding effects, which we do not.

[5] Our main analysis focuses on reduced-form RD estimates. Appendix D.1 shows that the results are unchanged using a fuzzy RD approach instrumenting for the number or identity of ward members.

from their own group are added. If access for more advantaged citizens is relatively inelastic to the composition of representatives, gains for marginalized citizens need not be offset, and aggregate benefits could rise.

We examine these possibilities with two main data sets. To study the impact on project management, we use annual, GP-level administrative data on the NREGS workfare program for 2016 to 2023, covering outcomes such as job cards issued, person-days of work, expenditures, and project completion. The GP leadership proposes projects, hires workers, and oversees implementation, so NREGS serves as a useful lens for broader governance. The program is also one of the GP's most important: NREGS spending is typically comparable to the rest of the local government budget combined.

Our second dataset measures access to individual welfare benefits, where the intermediation channel is most direct. The Mission Antyodaya initiative reports village-level counts of beneficiaries of major government programs in 2019–2020 and 2022–2023. We focus on eight major benefit programs — ration cards providing access to subsidized food, widow and old-age pensions, electricity and LPG connections, maternal cash transfers, housing assistance, piped water, and zero-balance bank accounts — which together cover hundreds of millions of beneficiaries. Appendix B provides additional details on both datasets.

For both datasets, our analysis focuses on a standardized index aggregating the outcomes (Kling et al., 2007). Column (1) of Table 2 estimates effects on the NREGS index for both designs (Panel A for density, Panel B for identity; Panel (b) of Figures 1 and 2 display the corresponding RD plots). The estimates are precise nulls: the upper bounds of the 95% confidence intervals (0.017 and 0.020 SD) rule out even modest gains. Column (2) reports similar nulls on a standardized index of the Mission Antyodaya outcomes (95% CI upper bounds of 0.047 and 0.040). Tables A4 and A5 find the same for each component of the indices, contrasting with cross-sectional evidence from India linking denser representation to better service outcomes (Auerbach, 2019).

Even if aggregate levels do not change, hyperlocal representatives may still improve citizen welfare by shifting the *composition* of spending toward what citizens actually want. Denser representation could improve this alignment by providing better information on citizen preferences or more advocates for these preferences in project selection; greater representation of marginalized groups could ensure that priorities of underrepresented citizens are reflected.

We test how local government projects match citizen priorities using data from Gram Panchayat Development Plan (GPDP) meetings. These are statutorily mandated meetings in which citizens and representatives jointly select development priorities for the coming year. Our data codes them into 34 standardized categories (e.g., drinking water, roads); on average, a meeting designates 9.3 priority areas. We combine this with annual, GP-level administrative data on completed NREGS projects, classified into categories that closely match the GPDP categories. For each GP, we calculate the fraction of NREGS projects completed in the financial year following the GPDP meeting that fall into categories the meeting designated as priorities. Column (3) of Table 2 finds no evidence that expanded representation increases this alignment rate under either design (see Appendix C.1 for details on the construction of this measure): the 95% confidence intervals are [-.009,0.027] and [-0.016,0.027].

4.2 Distribution of benefits

Expanded representation could also affect the distribution of public benefits across areas and groups even if aggregate levels are unchanged. For density, more ward members implies more areas with a dedicated advocate, which could shift benefits toward underserved areas. For identity, more SC ward members may shift the allocation of public goods toward marginalized groups.

We test for distributional effects using two measures. The first is the spatial dispersion of NREGS projects within a GP, constructed from novel data recording the GPS location of 21 million NREGS projects completed between 2016 and 2023 (Ministry of Panchayati Raj, 2025c). We measure dispersion as the mean distance between projects within a GP (in kilometers); greater dispersion would be consistent with benefits reaching more peripheral areas, which tend to be where marginalized groups like SCs reside (Srinivas, 1955; Bharathi et al., 2021). The second measures how equally Mission Antyodaya benefits are distributed across villages within a GP. For each GP and benefit program discussed in the previous section, we compute per-capita benefits at the village level. We then measure the population-weighted coefficient of variation across villages, where lower values indicate that per-capita benefits are more equally distributed across villages. We average this measure across programs (see Appendix C.2 for details). Columns (4) and (5) of Table 2 show no evidence that either increased density or greater SC representation shifts the spatial dispersion of NREGS projects or the equality of benefit allocation across villages.[6]

A more direct test of the identity RD is whether SC representation shifts outcomes specifically toward SCs. Panel C of Table 2 examines a set of outcomes pertaining specifically to SC households. Columns (1) and (3) test whether greater SC representation improves SC outcomes within the NREGS data (index of job cards and person-days of employment for SCs) or the Mission Antyodaya data (index of three outcomes: whether the GP assists with

[6]Translated into a standardized index for comparability with other outcomes, the 95% CIs are [-.060,0.005] and [-.035,0.040] (NREGS), and [-.023,0.052] and [-.085,.019] (benefit programs).

caste certificates, and scholarships and bank loans for marginalized households). Columns (2) and (4) test whether NREGS projects and Mission Antyodaya benefits are disproportionately allocated to higher-SC villages, comparing actual allocations to a benchmark of equal per-capita distribution (details in Appendix C.3). We find no evidence of effects across any of these outcomes (Figure A2; disaggregated outcomes in Table A8).[7]

These findings suggest that the null effects documented in Section 4.1 are not masking distributional shifts, whether measured through spatial dispersion, cross-village allocation, or SC-specific outcomes. However, we cannot test whether expanded representation shifts the targeting of benefits along other dimensions, such as household wealth.

4.3 Accountability and monitoring

A final potential role for hyperlocal representatives is promoting accountability by monitoring more powerful political actors or bureaucrats. Higher representative density could generate more monitors who deter corruption or mismanagement. For identity, SC representatives may have weaker ties to existing power structures and therefore provide a stronger check. At the same time, either margin could expand corruption by adding new actors with demands for rents.

We assess effects on accountability with two sources of audit data. The first is annual GP-level financial audits conducted by the Ministry of Panchayati Raj in fiscal years 2020–2022. District-level audit teams review compliance with financial and procedural norms, classify irregularities into 19 standardized categories, and report violations to executive authorities. The second is NREGS social audits conducted between 2016 and 2024. These are conducted by trained laypersons from outside the GP, who inspect recent works and interview residents to verify payments against official records. While social audits do not capture all forms of corruption (Afridi and Iversen, 2014), they are effective at detecting irregularities (Pande et al., 2025) and may be well-suited to flag the kinds of violations that ward members could plausibly influence, such as non-payment for work. Appendix C.4 provides additional detail and shows that these two independent audit systems exhibit a strong positive relationship in detected violations.

Columns (6) and (7) of Table 2 (Panels (g) and (h) of Figures 1 and 2) report effects on the total violations detected in each audit, with Tables A6 and A7 disaggregating by violation type. Overall, the results suggest that hyperlocal representatives have limited effects on malfeasance, with sufficient precision to rule out even modest effects: translated

[7] The coefficient on the benefits index is negative and marginally significant ($p = 0.041$), but given multiple testing (18 estimates in the table), this may reflect chance, particularly as the estimate is opposite the expected sign.

into a standardized index for comparability to other outcomes, the 95% confidence intervals are [-0.007,0.049] and [-0.044,0.031] (financial audits), and [-0.013,0.027] and [-0.018,0.039] (social audits).

4.4 Interpretation and Mechanisms

Our results point to additional hyperlocal representatives having little effect on core governance functions. One interpretation is that these representatives lack the authority or capacity needed to influence outcomes; alternatively, the nulls could reflect features of our empirical approach. We conduct seven tests to evaluate these explanations.

The first five tests address concerns that our empirical strategy could mask real effects. Tests 1 and 2 examine whether representation exhibits diminishing marginal returns, where additional representatives matter most at low baseline levels; estimates that combine many margins could miss such effects. Test 3 examines whether our null estimates reflect changes in representation that are too small to detect effects. Test 4 examines whether the marginal representatives induced by our discontinuities differ systematically from average ward members. Test 5 examines whether representatives become more effective with experience, such that pooling across their term attenuates estimates. The final two tests provide positive evidence of ward members having limited influence on core governance outcomes.

Test 1: Diminishing Returns (Density) Test 1 evaluates whether representation exhibits diminishing marginal returns by disaggregating our main density estimates. Our main representation density specification stacks multiple discontinuities, aggregating across heterogeneous treatments (e.g., from 5 to 7 members, from 12 to 14, etc.). Here, we instead group GPs based on the number of ward members to the left of their nearest population threshold and re-estimate Equation 1 within each group; for example, GPs with five or six members to the left correspond to discontinuities that increase representation from 5 to 7 or 6 to 9 members. Under diminishing returns, effects should be larger when baseline representation is lower. Panel (a) of Figure 3 plots the estimated coefficients by group for each outcome from Table 2. Contrary to the diminishing-returns hypothesis, estimates are consistently near zero and statistically insignificant.

Test 2: Diminishing Returns (Identity) The second test applies the same logic to the identity design. As in Test 1, we group GPs based on the change in SC representation induced by the nearest discontinuity and re-estimate Equation 1 within each group. For example, the first bin corresponds to an increase from zero to one reserved seats, which we might expect to have the largest effect. However, Panel (b) of Figure 3 shows no evidence of impacts in any bin; Panel (e) of Figure A2 finds the same for SC-focused outcomes.

Test 3: Larger Shifts in Representation Test 3 examines whether our null estimates reflect changes in representation that are too small to detect effects. We re-estimate the density RD restricting to discontinuities that induce larger proportional increases in density: the top 10%, 25%, and 50%, corresponding to density increases of at least 40%, 28.6%, and 18.1%. Panel (c) of Figure 3 again shows null results across all samples, indicating that even sizable increases in representation density do not affect outcomes.[8]

Test 4: Quality of marginal representatives Test 4 considers whether the marginal representatives induced by the density RD differ systematically from the average ward member (e.g., are less educated), which would limit what our estimates reveal about ward members generally. We test this by examining whether average ward member characteristics change discontinuously at the density thresholds; if marginal and incumbent representatives differ, average composition should shift at the cutoff (e.g., if marginal representatives are less educated, average education levels should decline at the cutoff).

Panel A of Table A9 shows little evidence of compositional changes, implying that marginal representatives do not differ significantly in education, age, or gender, though there is a small increase in the likelihood of belonging to the OBC caste category. These results indicate that marginal representatives are broadly similar to the average representative along observable dimensions. This finding is also consistent with Test 1: if marginal representatives were systematically lower quality in outcome-relevant ways, treatment effects should vary with the scale of representation, but they do not.[9]

Test 5: Learning Ward members may become more effective with experience, such that pooling across their term masks stronger effects in later years. Test 5 exploits cross-state variation in election timing and the long NREGS panel to estimate treatment effects on the NREGS index outcome in each year since election. The estimates in Table A10 remain close to zero throughout representatives' terms, providing evidence against either learning or election-cycle effects.

[8]Figure A3 re-estimates each main specification by state. Of these estimates, 6 are statistically significant at the 5% level, consistent with chance given the number of tests (102), and with no systematic state-level patterns.

[9]Consistent with Dal Bó et al. (2017) in Sweden, Panel B of Table A9 finds that more inclusive representation (additional SC representatives) does not reduce the quality of representatives as measured by education, though we lack the detailed measures of that study.

Test 6: Procedural outcomes Test 6 uses two sets of data to examine whether ward members affect procedural outcomes upstream of the substantive outcomes in Sections 4.1-4.3. We first examine policy deliberations using administrative data on over 1.2 million Gram Panchayat Development Plan (GPDP) meetings held between 2018 and 2023. These are statutorily mandated forums in which citizens and representatives set local development priorities for the coming year (see Appendix B.8). If hyperlocal representatives influence governance, their effects should be apparent at this agenda-setting stage.

More representatives should exhibit greater heterogeneity in preferences: if members influence deliberations, this should increase the number of priority areas set. Yet we find no effect on the number of priorities set (column (1) of Table A11), the number of core topics discussed (column (2)), or discussion of any individual topic (Table A12). However, increased representative density significantly increases citizen attendance (columns 3 to 6). These findings suggest that hyperlocal representatives influence participation in deliberative forums but not the policy priorities that emerge, consistent with a limited role in governance.

Appendix D.2 provides complementary evidence from the 2022 Mission Antyodaya survey on local government practices. We examine whether denser representation affects basic transparency: whether the GP publicly posts a list of public works projects, publicly posts a list of public program beneficiaries, and whether these lists are approved in a GP meeting. We find significant positive effects on each, consistent with ward members exerting influence over low-cost actions that do not require formal authority. However, this influence does not extend to the related, substantive outcomes in Sections 4.1-4.3. Consistent with the deliberative outcomes, this pattern supports the interpretation that the limited effects of ward members on those outcomes reflect constraints on their authority rather than complete disengagement.

Test 7: Voter turnout as revealed preference Our final test uses voter turnout as a revealed-preference measure of whether citizens value their hyperlocal representatives. In standard turnout models (Downs, 1957; Riker and Ordeshook, 1968), an individual votes if the cost of voting C is outweighed by expressive benefits D and the expected instrumental benefit pb, where p is the probability of being pivotal and b is the benefit from the preferred candidate winning. In our setting, more ward members reduce ward size and thus raise p for all voters in the GP. If voters value these positions ($b > 0$), turnout should increase; consistent with this logic, Narasimhan and Weaver (2024) find that exogenously smaller GPs have higher turnout when electing GP heads, indicating that voters in this setting do respond to pivotality when the underlying positions are valued.

In our context, pivotality is highly relevant: 27% of contested ward elections in Uttar

Pradesh in 2021 were decided by 10 or fewer votes. Using data from the 2021 Uttar Pradesh elections, Columns (1) and (2) of Table A13 confirm that both eligible voters per ward and margins of victory fall at the discontinuity. However, Column (3) shows no change in turnout. This suggests that b is small, consistent with a low perceived value of these positions.[10]

These tests suggest that the limited estimated effects of hyperlocal representatives reflect constraints inherent to the roles rather than features of the empirical design.[11] While ward members may play roles we do not observe, such as mediating community conflicts, they appear to fall short in fulfilling core governance functions envisioned in official training materials. Unlike GP heads, who control budgets, procurement, and staffing, ward members mostly affect outcomes indirectly, such as advocating on behalf of constituents or pressuring more powerful actors. Their influence might be greater with a direct channel of power, such as the ability to select or remove the GP head, or if given training to remedy poor knowledge of administrative processes (Bamezai et al., 2026). Although these positions increase proximity between citizens and the state, our results indicate that political inclusion alone need not translate into improved governance.

5 Conclusion

Hyperlocal representatives constitute the largest class of elected officials globally, yet credible causal evidence on their effectiveness remains scarce. Studying village councils in rural India, we find little evidence that changing the density or identity of hyperlocal representatives affects public goods provision, benefit distribution, or oversight. The pattern of results suggests that, while increasing representation is normatively appealing, hyperlocal representatives may have limited influence over substantive outcomes, functioning more as symbolic than as decision-making actors.

More broadly, our findings speak to a central question in political economy: when does political representation translate into effective power? Our results suggest that expanding representation without corresponding investments in authority, training, or incentives may yield limited returns, even when representatives are closely embedded in local communities. This is not obvious a priori, as even disempowered representatives could still convey local information to higher-level officials, lend institutional legitimacy to citizen claims in

[10] The null turnout response could also reflect limited voter awareness of pivotality. While the small size of wards makes it easier to observe the number of voters in the constituency or whether the race is competitive, voters may not notice.

[11] Our estimates are most directly informative about the intensive margin of representation and only suggestive on the extensive margin of any hyperlocal representation. However, the mechanism tests indicate that effects are small even at the lowest levels of representation we observe.

interactions with bureaucrats, or serve as focal points for collective political action. Future research should further investigate how institutional design shapes the relationship between representation and governance, identifying conditions under which political inclusion leads to durable improvements in accountability and development.

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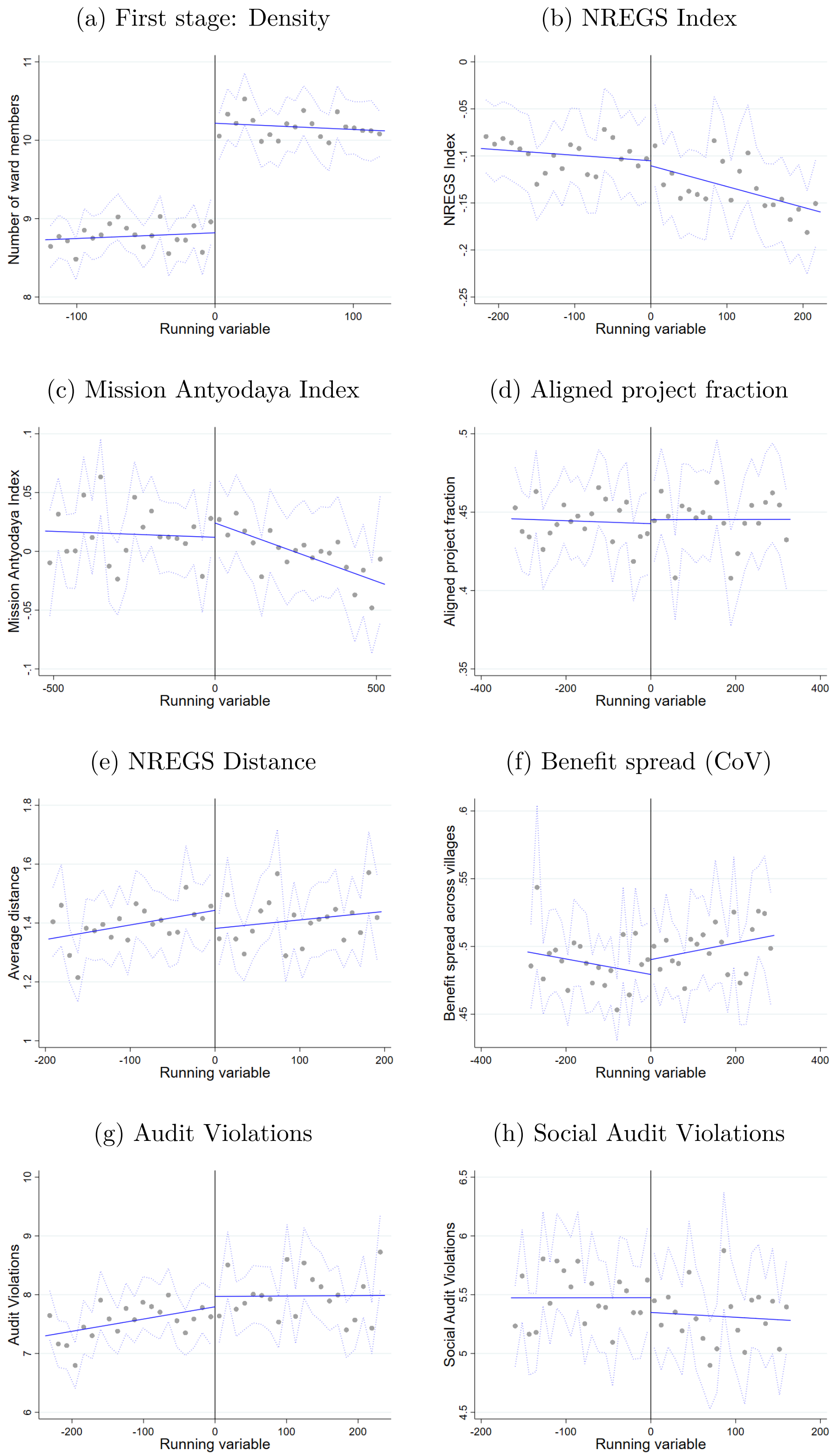
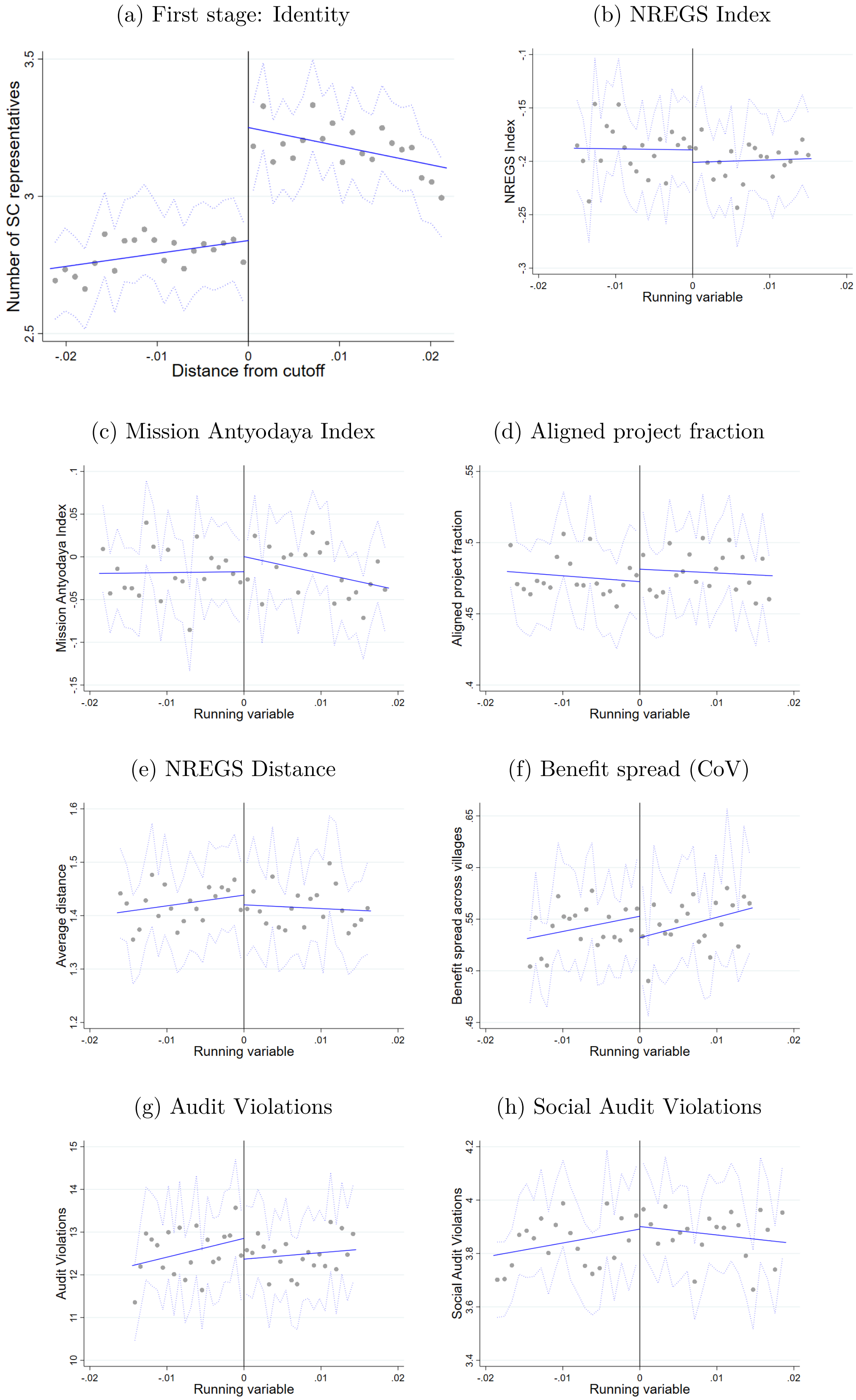
6 Figures and Tables

Figure 1: Density of political representation



Notes: These figures plot reduced-form regression discontinuity relationships for the density of political representation design. Panel (a) shows the first-stage relationship between the number of ward members and the running variable. Panels (b)-(h) show reduced-form effects on a standardized NREGS workfare index, a standardized Mission Antyodaya benefit access index, fraction of NREGS projects aligned with GPDP priorities, mean distance between NREGS projects (in km), a measure of inequality in benefit program access across villages in the GP, total financial audit violations, and total social audit violations, respectively. Each panel plots binned means and local linear fits estimated separately on either side of the cutoff using triangular kernels and MSE-optimal bandwidths.

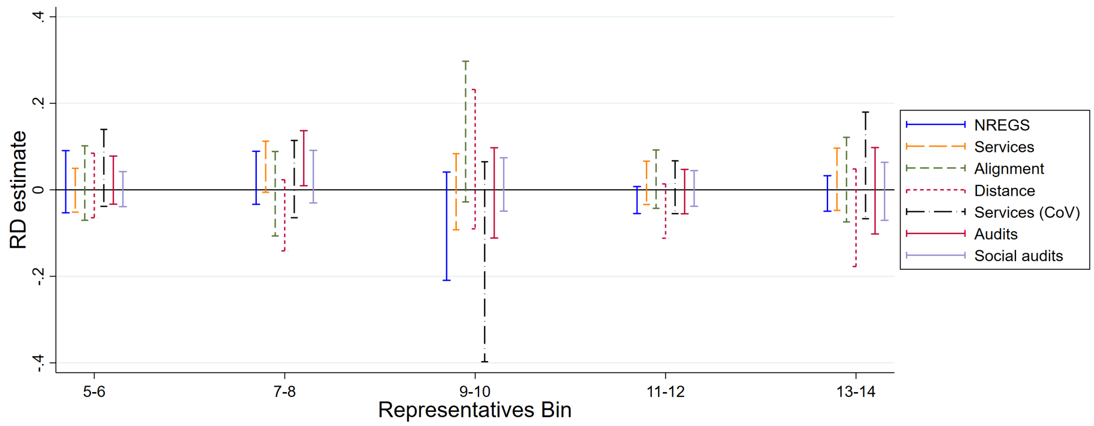
Figure 2: Representation of marginalized groups



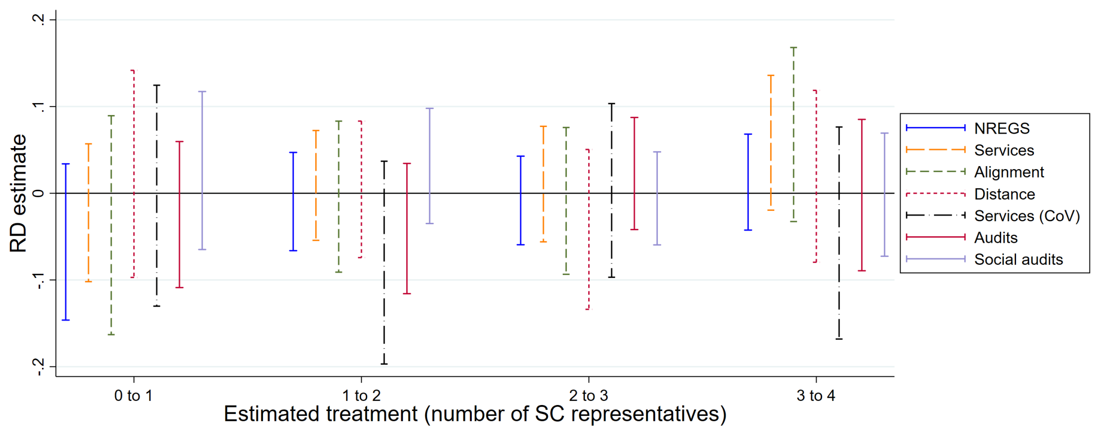
Notes: These figures plot reduced-form regression discontinuity relationships for the caste-based reservation design. Panel (a) shows the first-stage relationship between Scheduled Caste (SC) representation and the running variable. Panels (b)-(h) show reduced-form effects on a standardized NREGS workfare index, a standardized Mission Antyodaya benefit access index, fraction of NREGS projects aligned with GPDP priorities, mean distance between NREGS projects (in km), a measure of inequality in benefit program access across villages in the GP, total financial audit violations, and total social audit violations, respectively. Each panel plots binned means and local linear fits estimated separately on either side of the cutoff using triangular kernels and MSE-optimal bandwidths.

Figure 3: Additional tests of mechanisms

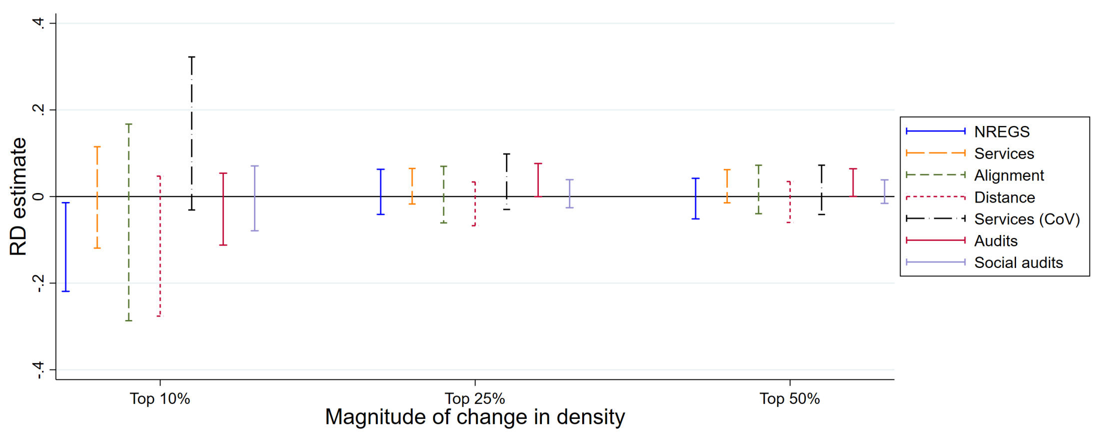
(a) Test 1: diminishing returns to density



(b) Test 2: diminishing returns to identity



(c) Test 3: Focus on larger proportional changes



Notes: These figures plot regression discontinuity estimates and corresponding confidence intervals for mechanism and heterogeneity analyses. Outcomes are standardized indices of NREGS workfare, mean distance between NREGS projects (in km), Mission Antyodaya welfare access, financial audit violations, and social audit violations; for distance and audit outcomes, indices are constructed by normalizing relative to the state-level mean and standard deviation. Panel (a) estimates effects separately by the number of ward members to the left of the nearest population cutoff (5-6, 7-8, 9-10, 11-12, and 13-14). For example, the first category (GPs with five or six members to the left) corresponds to discontinuities that increase representation from 5 to 7 or 6 to 9 members. Panel (b) applies the same approach to caste-based representation, grouping discontinuities by the change in the number of Scheduled Caste (SC) representatives. Panel (c) restricts the sample to discontinuities in the top 10, 25, and 50 percent of changes in the representative-to-population ratio. All estimates use local linear RD specifications with triangular kernels and MSE-optimal bandwidths.

Table 1: First stage and balance of baseline characteristics

Panel A: Additional political representatives

	First stage	Baseline covariates (2011)					
	Total reps	Primary school	Middle school	Paved road	Electrified	SC pop.	Literate pop.
RD_Estimate	1.3937***	0.0086	-0.0013	0.0088	-0.0091	0.0000	-0.0014
	(0.0888)	(0.0058)	(0.0105)	(0.0102)	(0.0111)	(0.0034)	(0.0023)
	[0.000]	[0.142]	[0.899]	[0.388]	[0.411]	[0.997]	[0.532]
Dep var mean	10.194	0.889	0.574	0.744	0.712	0.228	0.599
Bandwidth	172.786	199.634	187.621	171.729	167.581	248.052	252.256
Effective Obs	24214	35669	33578	30808	30074	44095	44793

Panel B: Additional representation of marginalized groups

	First stage		Baseline covariates (2011)					
	SC reps	SC frac	Primary school	Middle school	Paved road	Electrified	SC pop.	Literate pop.
RD_Estimate	0.4049***	0.0449***	-0.0103	-0.0020	-0.0024	0.0068	0.0058	-0.0003
	(0.0545)	(0.0046)	(0.0076)	(0.0094)	(0.0103)	(0.0116)	(0.0038)	(0.0027)
	[0.000]	[0.000]	[0.174]	[0.827]	[0.815]	[0.560]	[0.124]	[0.909]
Dep var mean	3.177	0.295	0.857	0.545	0.723	0.644	0.243	0.571
Bandwidth	0.022	0.023	0.012	0.018	0.013	0.013	0.015	0.013
Effective Obs	45020	44568	25723	39276	28262	29265	32099	28956

Note: This table reports regression discontinuity estimates validating the two empirical designs and testing balance in pre-determined characteristics. Panel A examines population-based thresholds in state Panchayati Raj Acts: the first column reports the first-stage effect on the total number of ward members, and remaining columns test balance in baseline GP characteristics from the 2011 Census. Panel B examines caste-based reservation thresholds: the first two columns report effects on the number and share of SC ward members, followed by balance tests for the same covariates. Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Main outcomes

Panel A: Effect of additional political representatives

	Aggregate outcomes			Distributional outcomes		Monitoring outcomes	
	Workfare index	Benefit programs index	Aligned project fraction	Workfare spread	Benefit spread (CoV)	Audit violations	Social audit violations
RD_Estimate	-0.0128	0.0166	0.0109	-0.0461	0.0071	0.1932	-0.0465
	(0.0141)	(0.0128)	(0.0094)	(0.0324)	(0.0095)	(0.1553)	(0.0987)
	[0.364]	[0.193]	[0.244]	[0.155]	[0.450]	[0.213]	[0.637]
Dep var mean	-0.159	0.007	0.447	1.423	0.493	8.092	4.877
Bandwidth	230.804	412.672	316.652	294.066	320.444	280.514	231.518
Effective Obs	271874	130411	44686	325030	44788	143880	153901

Panel B: Effect of additional representation of marginalized groups

	Aggregate outcomes			Distributional outcomes		Monitoring outcomes	
	Workfare index	Benefit programs index	Aligned project fraction	Workfare spread	Benefit spread (CoV)	Audit violations	Social audit violations
RD_Estimate	-0.0086	0.0064	0.0061	-0.0261	-0.0154	-0.4627	0.0479
	(0.0130)	(0.0166)	(0.0110)	(0.0316)	(0.0151)	(0.3616)	(0.0703)
	[0.510]	[0.700]	[0.576]	[0.409]	[0.309]	[0.201]	[0.496]
Dep var mean	-0.195	-0.017	0.479	1.416	0.543	12.258	3.883
Bandwidth	0.018	0.019	0.016	0.016	0.016	0.015	0.013
Effective Obs	295717	77339	33657	239324	30349	91624	120643

Panel C: Effect of additional representation of marginalized groups on marginalized group outcomes

	NREGS index (SC)	Workfare share (SC)	Benefit programs index (SC)	Benefit programs share (SC)
RD_Estimate	0.0048	0.0051	-0.0292**	0.0071
	(0.0154)	(0.0112)	(0.0143)	(0.0067)
	[0.756]	[0.647]	[0.041]	[0.286]
Dep var mean	-0.128	0.952	-0.005	0.993
Bandwidth	0.015	0.016	0.017	0.015
Effective Obs	184123	15038	35010	29047

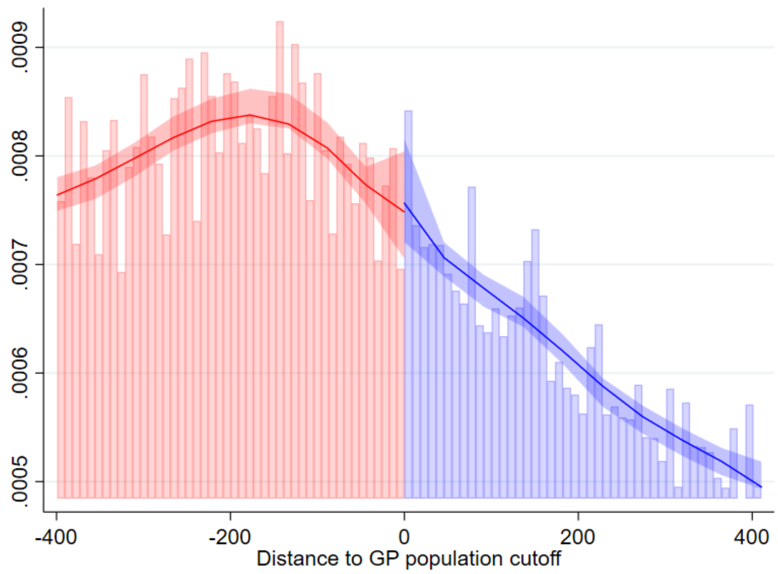
Note: This table reports regression discontinuity estimates of the effects of hyperlocal representation on governance outcomes. Panel A reports effects of increases in the number of ward members, Panel B reports effects of increases in representation from Scheduled Castes (SC), and Panel C reports effects of SC representation on SC-specific outcomes. Aggregate outcomes include a standardized index of NREGS workfare implementation, a standardized index of access to eight benefit programs from Mission Antyodaya, and the fraction of completed NREGS projects aligned with citizen priorities stated at GPDP meetings (see Appendix C.1). Distributional outcomes include the mean distance between NREGS projects within a GP (in km) and the population-weighted coefficient of variation of per-capita Mission Antyodaya benefits across villages (see Appendix C.2). Monitoring outcomes include total violations from financial audits and NREGS social audits. Panel C outcomes are: a standardized index of NREGS outcomes for SC households (job cards and person-days); the ratio of actual to expected NREGS project allocation toward higher-SC villages (see Appendix C.3); a standardized index of SC-targeted Mission Antyodaya benefits (caste-related scholarships, bank loans to marginalized households, and GP assistance with caste certificates); and the analogous ratio for Mission Antyodaya benefits. All outcomes are measured at the gram panchayat level. Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$)

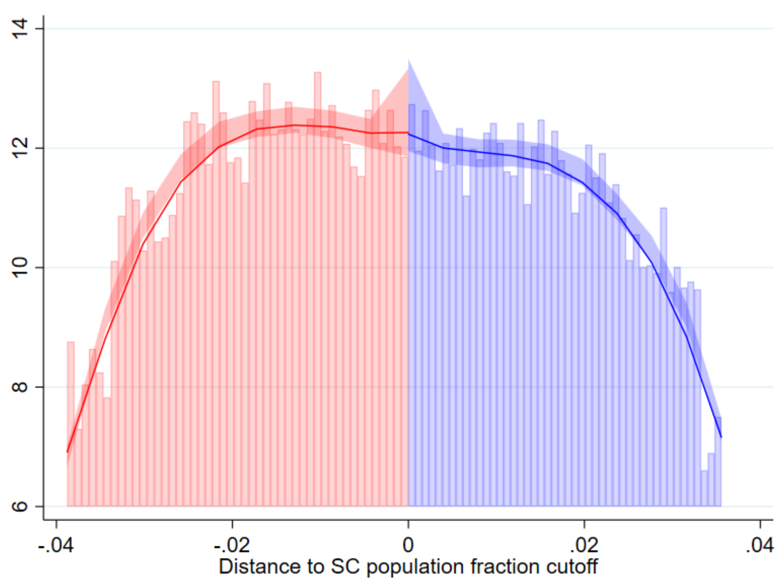
Appendix: For Online Publication

A Appendix Figures and Tables

Figure A1: Distribution of the running variable

(a) Manipulation test (Density)



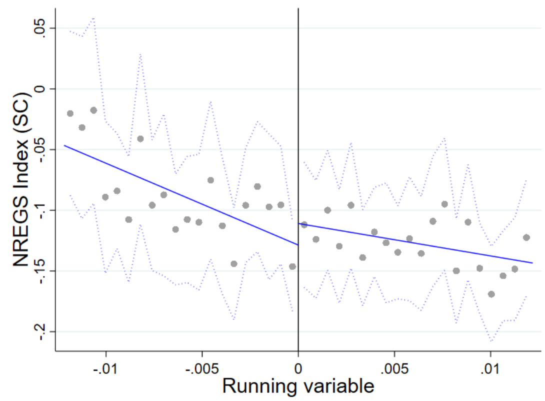


(b) Manipulation test (Representative identity)

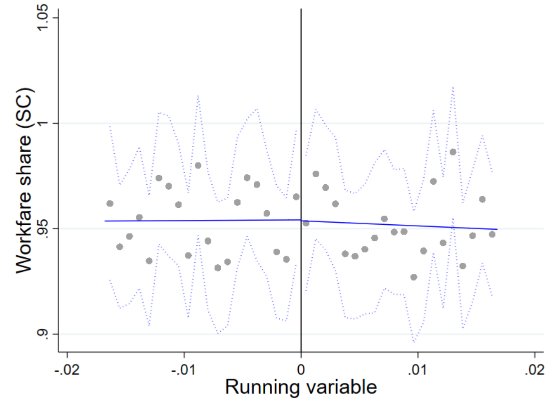
Notes: These figures plot the distribution of the running variables around their respective cutoffs and a non-parametric regression for each half of the distribution in order to test for a discontinuity in the running variables around the threshold (Cattaneo et al., 2020).

Figure A2: Representation of marginalized groups on outcomes for marginalized groups

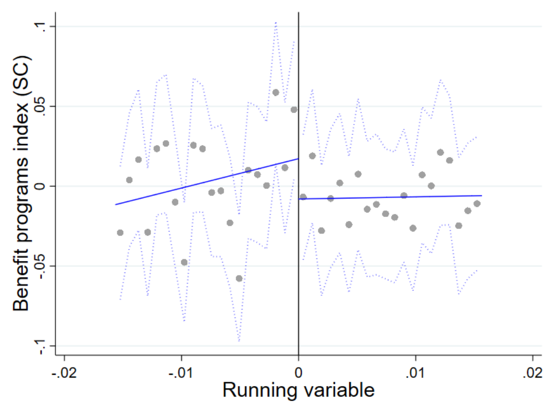
(a) Workfare index (SC)



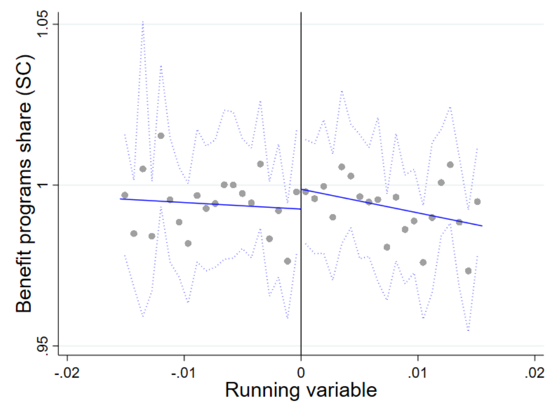
(b) Workfare share (SC)



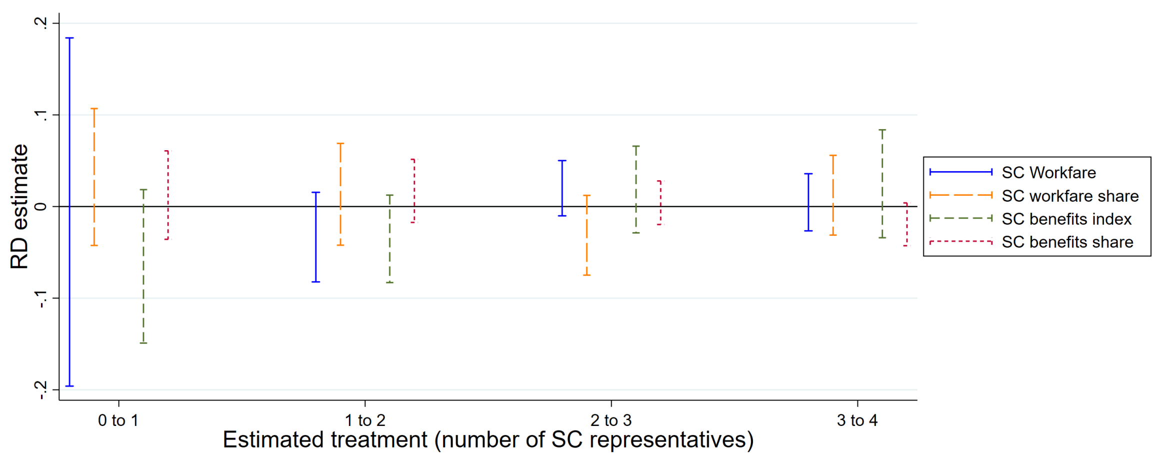
(c) Benefit programs index (SC)



(d) Benefit programs share (SC)



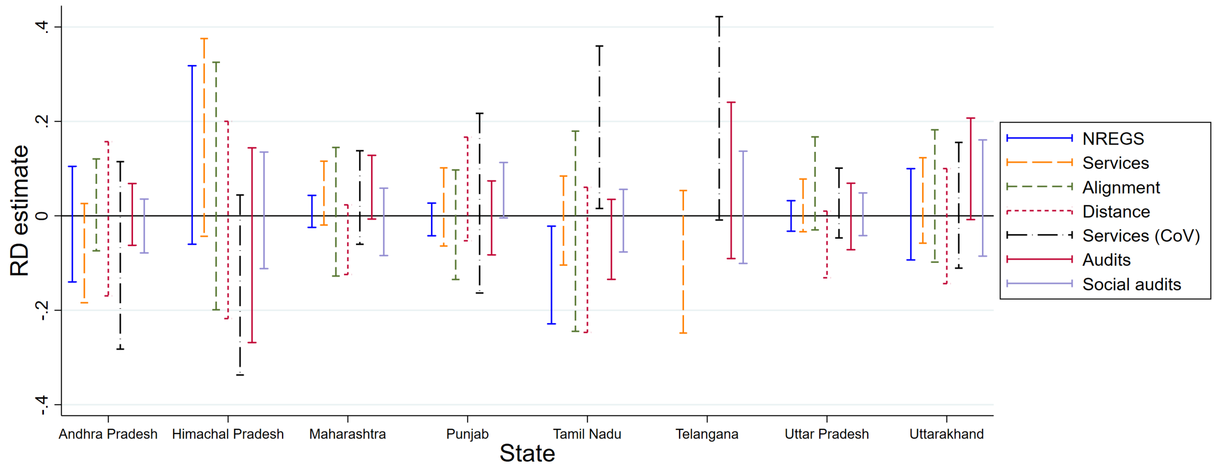
(e) Heterogeneity by number of SC representatives

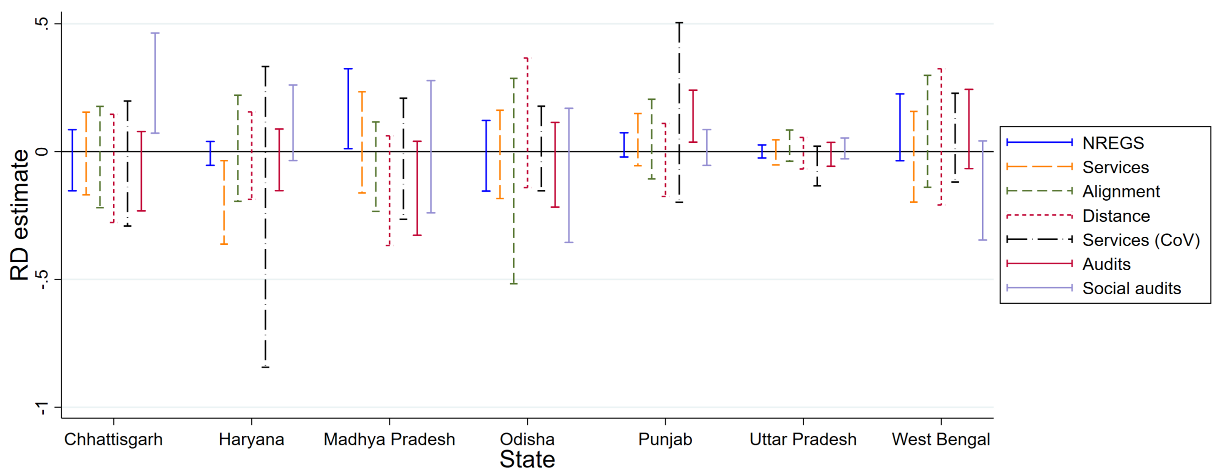


Notes: Panels (a) through (d) plot reduced-form regression discontinuity relationships for the caste-based reservation design. Panel (a) shows the relationship between the running variable and the index of SC-specific NREGS outcomes and Panel (b) shows the relationship with the distribution of NREGS projects across villages relative to their SC population share. Panels (c) and (d) plot the corresponding relationships for the Mission Antyodaya benefit program outcomes. Each panel plots binned means and local linear fits estimated separately on either side of the cutoff using triangular kernels and MSE-optimal bandwidths. Panel (e) plots regression discontinuity estimates and corresponding confidence intervals for an analysis of heterogeneity in the treatment effect of having an additional SC ward member. Outcomes are standardized indices of an index of NREGS outcomes for SC residents, how the share of NREGS projects skews towards villages with a higher fraction of SC residents in the GP, SC-targeted benefits measured in the Mission Antyodaya data (caste-related scholarships, bank loans, caste certificates), and how the share of benefits measured in the Mission Antyodaya data skews towards villages with a higher fraction of SC residents in the GP. It estimates treatment effects separately by the number of SC ward members to the left of the nearest population cutoff.

Figure A3: Estimated effects by states

(a) Representative Density





(b) Representative identity

Notes: These figures plot state-specific regression discontinuity estimates and corresponding confidence intervals, providing state-level analogues to the pooled estimates in Table 2. Outcomes are standardized indices of NREGS workfare, mean distance between NREGS projects, Mission Antyodaya welfare access, financial audit violations, and social audit violations; distance and audit outcomes are normalized using state-level means and standard deviations. Panel (a) reports effects of increases in representative density, and Panel (b) reports effects of increases in representation from marginalized groups. NREGS data are unavailable for Telangana, so NREGS and distance outcomes are not shown for that state. All estimates use local linear RD specifications with triangular kernels and MSE-optimal bandwidths.

State	**Population (2025 est.)**	**Rules relating number of ward members to GP population**	**SC member rule**
Andhra Pradesh	54.0 million [3.75%]	Population below 1500: 9 members Above 1500: Increase by 2 members per 1500 people	No
Chhattisgarh	25.5 million [2.11%]		Yes
Haryana	25.4 million [2.09%]		Yes
Himachal Pradesh	7.9 million [0.55%]	Less than 1750: 5 1750-2749: 7 2750-3749: 9 3750-4749: 11 4750+: 13	No
Madhya Pradesh	72.6 million [6.00%]		Yes
Maharashtra	126.6 million [8.79%]	Less than 1500: 7 Above 1500: Increase by 2 per 1500	No
Odisha	41.9 million [3.47%]		Yes
Punjab	30.8 million [2.14%]	Less than 1000: 5 1000-1999: 7 2000-4999: 9 5000-9999: 11 10000+: 13	Yes
Tamil Nadu	78.1 million [5.42%]	Less than 2000: 6 2000-5999: 9 6000-9999: 12	No
Telangana	40.6 million [2.82%]	Less than 1500: 7 1500-2999: 8 3000-4500: 9	No
Uttar Pradesh	248.4 million [17.25%]	Less than 1000: 9 1000-1999: 11 2000-2999: 13 3000+: 15	Yes
Uttarakhand	12.1 million [0.84%]	Less than 500: 5 500-999: 7 Above 1000: Increase by 2 per 1000	No
West Bengal	91.3 million [7.55%]		Yes

Table A1: Ward Member Rules and Population of Sample States (2025)

Note: This table summarizes state-specific rules governing the number of ward members and caste-based reservation policies across the states in the sample, along with state population sizes. Population figures are 2025 estimates, with each state's share of India's population reported in brackets. Ward member rules describe population thresholds and corresponding changes in the number of ward members at the gram panchayat level. The final column indicates whether Scheduled Caste (SC) reservation rules for ward members apply in the state.

Table A2: First stage by state

Panel A: Additional political representatives								
	Andhra Pradesh	Himachal Pradesh	Maharashtra	Punjab	Tamil Nadu	Telangana	Uttar Pradesh	Uttarakhand
RD_Estimate	0.8863***	1.2897***	1.1925***	1.1944***	2.6694***	1.0268***	1.5458***	0.3704***
	(0.1371)	(0.3426)	(0.1610)	(0.0912)	(0.1087)	(0.1689)	(0.1094)	(0.1226)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.003]
Dep var mean	9.511	7.450	8.411	6.948	9.006	9.442	12.187	7.252
Bandwidth	412.007	186.532	280.064	227.100	431.035	251.470	190.653	125.019
Effective Obs	4025	524	3214	3340	2769	1573	13537	2233

Panel B: Additional representation of marginalized groups							
	Chhattisgarh	Haryana	Madhya Pradesh	Odisha	Punjab	Uttar Pradesh	West Bengal
RD_Estimate	0.0454**	0.0371**	0.0396**	0.0368*	0.0916***	0.0438***	0.0729*
	(0.0201)	(0.0186)	(0.0160)	(0.0192)	(0.0177)	(0.0081)	(0.0404)
	[0.024]	[0.046]	[0.013]	[0.055]	[0.000]	[0.000]	[0.071]
Dep var mean	0.195	0.287	0.187	0.246	0.423	0.315	0.322
Bandwidth	0.020	0.020	0.007	0.012	0.037	0.012	0.011
Effective Obs	1730	1808	1301	1280	3107	15444	581

Note: This table reports regression discontinuity estimates for the first-stage for the two empirical designs. Panel A examines the relationship between the first running variable (based on GP population) and the number of ward members. Panel B examines the relationship between the second running variable (based on SC population share) and the number of SC ward members. Ward member rosters are only observed for a subset of GPs based on data availability. All estimates use local linear RD specifications with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014). Standard errors are clustered at the GP level and reported in parentheses.

Table A3: Match rates

	Match rate		Match probability		
	Full sample	In sample	RD estimate	SE	p-value
Panel A: Additional political representation analysis					
Census	99.90%	100%			
Elections	95.97%	96.62%	.00262	(.00663)	.692
Financial Audits	99.32%	99.54%	-.00033	(.00117)	.78
GPDP Meetings	99.98%	99.98%	-.00011	(.00022)	.61
Mission Antyodaya	94.58%	95.79%	-.00385	(.00249)	.122
NREGS	90.34%	88.62%	-.00079	(.00643)	.902
NREGS (location)	90.32%	88.59%	-.00083	(.00639)	.896
Social audits (NREGS)	87.23%	86.29%	.00568	(.00774)	.463
Ward member roster	93.72%	92.43%	-.00055	(.00418)	.894
Panel B: Additional representation of marginalized groups analysis					
Census	99.88%	99.84%			
Elections	95.97%	96.40%	-.00509	(.00535)	.342
Financial Audits	98.95%	98.93%	-.00258	(.00217)	.235
GPDP Meetings	99.89%	99.91%	.00053	(.00071)	.458
Mission Antyodaya	97.94%	98.36%	.00302	(.00208)	.147
NREGS	93.98%	93.39%	.01055	(.00568)	.063
NREGS (location)	93.94%	93.34%	.00988	(.00573)	.085
Social audits (NREGS)	72.44%	81.19%	.01158	(.00951)	.223
Ward member roster	93.91%	97.55%	-.00397	(.00348)	.253

Panel A examines match rates for the analysis of additional political representation, while Panel B examines match rates for the analysis of additional representation from marginalized groups. Column (1) reports the match rate between the local government directory data and the listed data set for all gram panchayats in the sample for each panel (8 states in Panel A and 7 in Panel B). Column (2) reports the match rate for gram panchayats within the typical RD bandwidth for each panel. Columns (3) through (5) test for differential matching around the discontinuity. Column (3) reports regression discontinuity estimates for an indicator equal to one if the gram panchayat is successfully matched to the listed data set, with standard errors in column (4). Column (5) reports p-values for the corresponding RD estimates.

Table A4: Effects on NREGS

Panel A: Effect of additional political representatives						
	Workfare index	Job cards	Person days	Labor expenditure	Material expenditure	Total assets
RD_Estimate	-0.0128	-0.0028	-0.0681	-13.5101	-2.5287	-0.1288
	(0.0141)	(0.0019)	(0.0980)	(17.1945)	(8.3085)	(0.3433)
	[0.364]	[0.137]	[0.487]	[0.432]	[0.761]	[0.708]
Dep var mean	-0.159	0.165	3.342	577.851	235.319	15.060
Bandwidth	230.804	269.474	230.046	188.469	193.668	223.196
Effective Obs	271874	227472	195375	152163	156024	263886
Panel B: Effect of additional representation of marginalized groups						
	Workfare index	Job cards	Person days	Labor expenditure	Material expenditure	Total assets
RD_Estimate	-0.0086	0.0003	-0.0372	-8.1008	1.7739	-0.7506
	(0.0130)	(0.0018)	(0.0586)	(13.5789)	(6.5257)	(1.6209)
	[0.510]	[0.872]	[0.525]	[0.551]	[0.786]	[0.643]
Dep var mean	-0.195	0.139	2.762	560.890	227.979	29.621
Bandwidth	0.018	0.016	0.021	0.016	0.017	0.016
Effective Obs	295717	185160	249450	184117	205167	251344

Note: This table reports regression discontinuity estimates of the effects of hyperlocal representation on NREGS workfare outcomes. Panel A reports effects of increases in the number of ward members, and Panel B reports effects of increases in Scheduled Caste (SC) representation. Columns (1) examines effects on a standardized index of the other variables. The outcome in column (2) is the per capita number of job cards, while column (3) is per capita days of NREGS work provided in the GP. Columns (4) and (5) measure total expenditure on labor and materials, while column (6) measures the total assets created by the NREGS program. Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Effects on program access

	Programs index	Jan Dhan	Ration card	LPG	Pension	Saubhagya (elec.)	PMMVY	Housing benefits	Piped water
Panel A: Effect of additional political representatives									
RD_Estimate	0.0166	0.0006	0.0051	0.0041	0.0037	0.0037	0.0002	0.0036	0.0091
	(0.0128)	(0.0041)	(0.0055)	(0.0037)	(0.0042)	(0.0036)	(0.0003)	(0.0050)	(0.0076)
	[0.193]	[0.874]	[0.347]	[0.273]	[0.375]	[0.311]	[0.573]	[0.474]	[0.231]
Dep var mean	0.007	0.351	0.500	0.242	0.263	0.152	0.015	0.223	0.292
Bandwidth	412.672	292.371	262.609	292.269	206.196	250.324	300.504	260.397	178.566
Effective Obs	130411	95653	86562	95653	68713	82899	98159	85947	59681
Panel B: Effect of additional representation of marginalized groups									
	Programs index	Jan Dhan	Ration card	LPG	Pension	Saubhagya (elec.)	PMMVY	Housing benefits	Piped water
RD_Estimate	0.0064	0.0020	-0.0024	0.0030	-0.0093*	0.0072	-0.0005	0.0006	-0.0061
	(0.0166)	(0.0051)	(0.0074)	(0.0052)	(0.0048)	(0.0044)	(0.0004)	(0.0060)	(0.0070)
	[0.700]	[0.687]	[0.744]	[0.568]	[0.053]	[0.102]	[0.214]	[0.917]	[0.386]
Dep var mean	-0.017	0.363	0.528	0.296	0.254	0.191	0.015	0.239	0.166
Bandwidth	0.019	0.016	0.012	0.012	0.010	0.014	0.015	0.016	0.010
Effective Obs	77339	64772	48593	49050	41185	58401	60998	63745	41944

Note: This table reports regression discontinuity estimates of the effects of hyperlocal representation on access to public programs. Panel A reports effects of increases in the number of ward members, and Panel B reports effects of increases in Scheduled Caste (SC) representation. Columns (1) examines effects on a standardized index of the other variables. The outcome in column (2) is the number of zero-balance bank accounts under the Jan Dhan initiative, while column (3) is Below Poverty Line (BPL) ration cards providing access to subsidized food grains. Columns (4) and (5) measure liquefied petroleum gas (LPG) connections (Pradhan Mantri Ujjwala scheme) and pensions for the elderly, widows, and the disabled (National Social Assistance Programme), while column (6) measures household electricity connections under the Saubhagya scheme (Pradhan Mantri Sahaj Bijli Har Ghar Yojana). Columns (7) and (8) look at a Rs. 5000 cash transfer for pregnant women and mothers (Pradhan Mantri Matru Vandana Yojana) and housing subsidies or waitlist status under Pradhan Mantri Awaas Yojana or state schemes. Column (9) examines publicly provided piped water connections (Jal Jeevan Mission). Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Effects on audit violations

Panel A: Effect of additional political representatives					
	Tot viol.	No viol.	Rule viol.	Non-prod of records	Irregularities
RD_Estimate	0.1932	0.0010	0.0522*	0.0163	0.0026
	(0.1553)	(0.0013)	(0.0284)	(0.0328)	(0.0204)
	[0.213]	[0.443]	[0.066]	[0.620]	[0.898]
Dep var mean	8.092	0.017	1.179	1.125	0.428
Bandwidth	280.514	436.395	281.728	208.784	199.349
Effective Obs	143880	214210	144300	108268	103818
Panel B: Effect of additional representation of marginalized groups					
	Tot viol.	No viol.	Rule viol.	Non-prod of records	Irregularities
RD_Estimate	-0.4627	0.0020	0.0093	0.0117	0.0024
	(0.3616)	(0.0027)	(0.0436)	(0.0385)	(0.0160)
	[0.201]	[0.461]	[0.830]	[0.762]	[0.883]
Dep var mean	12.258	0.031	1.421	1.476	0.182
Bandwidth	0.015	0.013	0.014	0.015	0.013
Effective Obs	91624	77968	87298	90205	82194

Note: This table reports regression discontinuity estimates of the effects of hyperlocal representation on financial misconduct, as measured by audits of the gram panchayat. Panel A reports effects of increases in the number of ward members, and Panel B reports effects of increases in Scheduled Caste (SC) representation. Columns (1) examines effects on the total number of detected violations, while column 2 looks at whether any violations were detected. Columns (3) to (5) measure the total number of the three most common violation types: procedural rule violations, refusals to provide records (a potential indicator of an attempt to avoid revealing corruption), and financial irregularities. Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Effects on NREGS social audits violations

Panel A: Effect of additional political representatives					
	Total issues	Financial misapprop.	Financial deviation	Process violation	Citizen grievances
RD_Estimate	-0.0465	-0.0249	-0.0157	-0.0177	0.0014
	(0.0987)	(0.0343)	(0.0244)	(0.0677)	(0.0147)
	[0.637]	[0.468]	[0.521]	[0.794]	[0.922]
Dep var mean	4.877	0.633	0.828	2.685	0.659
Bandwidth	231.518	210.625	261.345	206.702	328.285
Effective Obs	153901	140157	173116	137534	216259
Panel B: Effect of additional representation of marginalized groups					
	Total issues	Financial misapprop.	Financial deviation	Process violation	Citizen grievances
RD_Estimate	0.0479	-0.0024	-0.0169	0.0500	0.0269
	(0.0703)	(0.0072)	(0.0149)	(0.0523)	(0.0207)
	[0.496]	[0.742]	[0.255]	[0.339]	[0.195]
Dep var mean	3.883	0.105	0.445	2.546	0.739
Bandwidth	0.013	0.013	0.010	0.012	0.014
Effective Obs	120643	120725	95416	110964	129107

Note: This table reports regression discontinuity estimates of the effects of hyperlocal representation on financial misconduct, as measured by audits of the gram panchayat. Panel A reports effects of increases in the number of ward members, and Panel B reports effects of increases in Scheduled Caste (SC) representation. Columns (1) examines effects on the total number of detected violations. Columns (2) to (5) measure the total number of the violation types most tightly linked to corruption: financial misappropriation, deviations in financial records, process violations, and citizen grievances (often about theft of wage payments). Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Effects on SC-focused disaggregated outcomes

Panel A: Effect of additional representation of marginalized groups					
	NREGS outcomes		Welfare outcomes		
	Jobcards (SC)	Person days (SC)	Scholarships	Loans	Caste cert.
RD_Estimate	0.0025	0.0194	-0.0045	-0.0002	-0.0259**
	(0.0056)	(0.1297)	(0.0040)	(0.0027)	(0.0124)
	[0.656]	[0.881]	[0.259]	[0.935]	[0.036]
Dep var mean	0.192	3.659	0.102	0.052	0.489
Bandwidth	0.015	0.016	0.017	0.017	0.016
Effective Obs	179906	184914	33998	34946	30981

Note: This table reports regression discontinuity estimates of the effects of increases in Scheduled Caste (SC) representation on SC-focused outcomes. Columns (1) and (2) look at the per capita NREGS job card and days worked outcomes only among SC households. Columns (3) to (5) focus on individual benefit programs measured in the Mission Antyodaya data: minority focused scholarships, minority focused loans, and whether the local GP assists in providing caste certificates. Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: Characteristics of elected members

Panel A: Effect of additional political representatives

				Caste		
	Education	Age	Female	SC	OBC	General
RD_Estimate	-0.0069	-0.2657	-0.0031	-0.0074	0.0173*	-0.0127
	(0.0236)	(0.2215)	(0.0067)	(0.0066)	(0.0093)	(0.0101)
	[0.770]	[0.230]	[0.640]	[0.266]	[0.062]	[0.209]
Dep var mean	1.500	41.069	0.458	0.269	0.365	0.324
Bandwidth	313.793	200.779	203.523	238.877	198.311	197.699
Effective Obs	30074	12898	20714	24087	20234	20139

Panel B: Effect of additional representation of marginalized groups

				Caste		
	Education	Age	Female	SC	OBC	General
RD_Estimate	-0.0016	-0.1213	-0.0048	0.0448***	-0.0154**	-0.0213***
	(0.0257)	(0.1900)	(0.0048)	(0.0046)	(0.0076)	(0.0070)
	[0.950]	[0.523]	[0.312]	[0.000]	[0.041]	[0.002]
Dep var mean	1.435	41.063	0.436	0.294	0.405	0.228
Bandwidth	0.012	0.013	0.014	0.023	0.013	0.014
Effective Obs	23017	17239	26087	44007	24942	26309

Note: This table reports estimates of how the changes in representation induced by the regression discontinuity designs we study affect the average characteristics of elected ward members. Panel A reports effects of increases in the number of ward members, while Panel B reports effects of increases in representation from Scheduled Castes (SC). Column 1 reports effects on the average education levels of elected members. The education variable ranges from 0 (illiterate) to 7 (PhD), using data from the Uttar Pradesh 2021 elections and rosters of ward members from Gram Manchitra. Column 2 reports effects on the average age of ward members, while column 3 repots the fraction who are female. Columns 4 to 6 examine the fraction of ward members who are from different caste groups (Scheduled Caste, Other Backwards Classes, General Caste). All outcomes are measured at the gram panchayat level. Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10: Effects on NREGS by year since election

Panel A: Effect of additional political representatives					
	0 Years	1 Years	2 Years	3 Years	4 Years
RD_Estimate	-0.0117	-0.0246*	-0.0112	-0.0190	-0.0156
	(0.0212)	(0.0141)	(0.0145)	(0.0181)	(0.0247)
	[0.579]	[0.080]	[0.441]	[0.293]	[0.528]
Dep var mean	-0.141	-0.135	-0.156	-0.187	-0.002
Bandwidth	208.190	260.475	231.220	235.293	224.730
Effective Obs	28151	48657	48439	33159	34859
Panel B: Effect of additional representation of marginalized groups					
	0 Years	1 Years	2 Years	3 Years	4 Years
RD_Estimate	0.0128	0.0049	0.0076	-0.0013	-0.0044
	(0.0138)	(0.0102)	(0.0100)	(0.0130)	(0.0134)
	[0.351]	[0.632]	[0.450]	[0.919]	[0.741]
Dep var mean	-0.399	-0.294	-0.354	-0.401	-0.404
Bandwidth	0.016	0.016	0.014	0.017	0.016
Effective Obs	27310	47174	42050	27422	26504

Note: This table reports regression discontinuity estimates of the effects of hyperlocal representation on NREGS outcomes by time since the most recent gram panchayat election. Columns report effects estimated separately by year since election. Panel A reports effects of increases in the number of ward members, and Panel B reports effects of increases in Scheduled Caste (SC) representation. The outcome is the standardized NREGS workfare index. All estimates use local linear RD specifications with triangular kernels and MSE-optimal bandwidths. Standard errors are clustered at the gram panchayat level and reported in parentheses; p-values are reported in brackets. Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Gram Panchayat Development Plan Meetings

	Outcomes		Attendance			
Panel A: Effect of additional political representatives						
	Total focus areas	Topics covered	Total	Women	SC	ST
RD_Estimate	0.0343	0.0015	6.4002**	1.7752*	0.2495	1.1817**
	(0.0573)	(0.0105)	(3.0253)	(1.0702)	(0.8714)	(0.5527)
	[0.550]	[0.885]	[0.034]	[0.097]	[0.775]	[0.033]
Dep var mean	2.680	5.499	141.032	40.428	36.806	9.307
Bandwidth	345.840	456.027	299.797	277.411	308.574	359.635
Effective Obs	284539	366421	248436	231619	255995	294832
Panel B: Effect of additional representation of marginalized groups						
	Outcomes		Attendance			
	Total focus areas	Topics covered	Total	Women	SC	ST
RD_Estimate	0.0499	0.0195	4.9846	-1.3997	0.7466	-0.5621
	(0.0837)	(0.0171)	(7.0340)	(2.2278)	(1.8384)	(0.9163)
	[0.551]	[0.252]	[0.479]	[0.530]	[0.685]	[0.540]
Dep var mean	2.864	5.459	202.999	61.269	55.642	11.725
Bandwidth	0.018	0.015	0.012	0.022	0.016	0.028
Effective Obs	187774	156210	130534	238523	168570	294925

Note: This table reports regression discontinuity estimates of the effects of hyperlocal representation on Gram Panchayat Development Plan meetings. Panel A reports effects of increases in the number of ward members, and Panel B reports effects of increases in Scheduled Caste (SC) representation. Columns (1) examines effects on the total number of major topics covered at the meetings (out of 7), while Table A12 shows effects on each of the topics individually. Column 2 looks at the total number of development priorities set for the upcoming year. Columns (3) to (6) measure attendance at the meetings for all persons, women, scheduled castes, and scheduled tribes respectively. Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12: Gram Panchayat Development Plan Meeting Topics Covered

	Poverty issues	Current funds	Future resources	Mission Antyod.	MA gaps	GPDP passed	COVID-19
Panel A: Effect of additional political representatives							
RD_Estimate	0.0012	-0.0027	-0.0004	0.0000	0.0029	-0.0001	-0.0008
	(0.0046)	(0.0029)	(0.0021)	(0.0029)	(0.0027)	(0.0012)	(0.0035)
	[0.796]	[0.353]	[0.845]	[0.991]	[0.282]	[0.966]	[0.819]
Dep var mean	0.379	0.882	0.953	0.851	0.872	0.981	0.932
Bandwidth	272.219	304.742	318.348	496.578	388.278	355.119	266.425
Effective Obs	227695	253146	263543	395620	316686	291982	138487
Panel B: Effect of additional representation of marginalized groups							
	Poverty issues	Current funds	Future resources	Mission Antyod.	MA gaps	GPDP passed	COVID-19
RD_Estimate	0.0087*	0.0023	-0.0005	0.0051	0.0020	0.0026	-0.0016
	(0.0052)	(0.0035)	(0.0030)	(0.0049)	(0.0038)	(0.0020)	(0.0050)
	[0.095]	[0.502]	[0.871]	[0.303]	[0.597]	[0.193]	[0.745]
Dep var mean	0.344	0.885	0.952	0.847	0.875	0.977	0.917
Bandwidth	0.019	0.016	0.015	0.014	0.014	0.018	0.013
Effective Obs	198004	167609	159030	147555	152549	194933	89785

Note: This table reports regression discontinuity estimates of the effects of hyperlocal representation on the topics discussed at Gram Panchayat Development Plan meetings. Panel A reports effects of increases in the number of ward members, and Panel B reports effects of increases in Scheduled Caste (SC) representation. The topics discussed are: (1) Presentation by self-help group members on poverty issues and poverty reduction plans; (2) Review of current year spending of local government funds; (3) Discussion on resources and plans for next year; (4) Presentation and validation of the data collected by the most recent round of the Mission Antyodaya initiative (see section B.2); (5) Discussion of the gaps revelead by the Mission Antyodaya data and proposals to fill those gaps; (6) whether the meeting passed a resolution on the GPDP priorities for the following year); and (7) Discussion of COVID-19 appropriate behavior (in 2020, 2021, 2022). Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A13: Pivotality and Electoral outcomes

	(1) Total ward pop.	(2) Margin of victory	(3) Turnout
RD_Estimate	-25.014***	-4.340***	-0.790
	(2.71)	(1.01)	(0.80)
	[0.000]	[0.000]	[0.323]
Dep var mean	142.537	21.547	72.879
Bandwidth	199.469	225.991	168.148
Effective Obs	23087	24237	19546

* p<0.10, ** p<0.05, *** p<0.01

Note: This table tests whether voters value hyperlocal representatives using turnout as a revealed-preference measure. Estimates are from regression discontinuity designs based on population thresholds that determine ward size in the 2021 ward member elections in Uttar Pradesh. Columns (1) and (2) show that adding a ward member reduces the number of eligible voters per ward and the margin of victory, increasing the probability that an individual voter is pivotal. Column (4) shows no corresponding increase in voter turnout. Within standard turnout models, this null response implies that voters place little instrumental value on these representatives. Robust standard errors in parentheses; p-values in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B Data appendix

This appendix provides additional details on the construction of each of the main datasets used in the paper, including issues related to data collection and matching for specific states.

B.1 Census and Local Government Directory

We use data from the Local Government Directory (LGD) and the Census of India to construct population measures and characteristics of gram panchayats. The LGD is a centralized administrative registry maintained by the Ministry of Panchayati Raj that assigns unique codes to local government entities and tracks their names, boundaries, and component villages (Ministry of the Panchayat Raj, 2020). It also provides a standardized mapping of gram panchayats to higher administrative units-including blocks, districts, and states-and has become the backbone for linking administrative datasets collected by different ministries.

Population and demographic characteristics are drawn from the 2001 and 2011 Censuses of India, which provide decennial, village-level data on total population (including by broad caste category) and other characteristics, such as the presence of infrastructure (Census of India, 2001, 2011). We link Census villages to gram panchayats using the LGD, which contains census codes for component villages. Villages are then aggregated to their corresponding gram panchayats using the Local Government Directory to construct the regression discontinuity running variables, including total gram panchayat population and the fraction belonging to Scheduled Castes. Census 2011 data are used for all states except Tamil Nadu, where the running variables are based on Census 2001 data.

B.2 Mission Antyodaya

The Mission Antyodaya initiative provides village-level administrative data on the delivery of individual public benefits across rural India (Ministry of Rural Development, 2019, 2022). Launched by the Government of India, these data were collected by the central government in fiscal years 2019–20 and 2022–23 to provide inter-census snapshots of rural demographics, infrastructure, services, and welfare coverage. Mission Antyodaya indicators are drawn primarily from departmental administrative records rather than household surveys; for example, information on Below Poverty Line (BPL) ration cards is sourced from the Department of Food and Civil Supplies. The data are reported by local officials using standardized templates and subsequently validated and aggregated at higher administrative levels. As a result, Mission Antyodaya provides comprehensive and nationally comparable measures of program coverage.

Our analysis follows Narasimhan and Weaver (2024) in examining beneficiary counts for eight major benefit programs: (1) zero-balance bank accounts under the Jan Dhan initiative; (2) Below Poverty Line (BPL) ration cards providing access to subsidized food grains; (3) liquefied petroleum gas (LPG) connections under the Pradhan Mantri Ujjwala scheme; (4) pensions for the elderly, widows, and the disabled under the National Social Assistance Programme; (5) household electricity connections under the Saubhagya scheme (Pradhan Mantri Sahaj Bijli Har Ghar Yojana); (6) a Rs. 5000 cash transfer for pregnant women and mothers under the Pradhan Mantri Matru Vandana Yojana; (7) housing subsidies or waitlist status under the Pradhan Mantri Awaas Yojana or related state schemes; and (8) access to publicly provided piped water under the Jal Jeevan Mission.

B.3 National Rural Employment Guarantee Scheme work records

We obtain annual, gram panchayat–level administrative data on the implementation of the National Rural Employment Guarantee Scheme from the Ministry of Rural Development for the period 2016 to 2023 (Ministry of Rural Development, 2016-2023). These data are drawn from the official NREGS Management Information System and include information on project completion, person-days of employment generated, wage disbursements, and expenditures on labor and materials, among other outcomes. We downloaded the data between May and July 2024. Data for Telangana are not observed, as they were not available at the time of download and have since been removed from the system. For this reason, Table A3 reports match rates excluding Telangana.

B.4 National Rural Employment Guarantee Scheme work location records (Gram Manchitra)

The Gram Manchitra portal is a web-based geographic information system (GIS) platform developed by the National Informatics Centre under the Ministry of Panchayati Raj (Ministry of Panchayati Raj, 2025c). The portal integrates multiple geospatial datasets to provide a unified view of village infrastructure. A key feature of the platform is the tracking of geo-tagged development works across schemes, including NREGS projects. Through integration with mobile data capture tools, assets constructed under government programs are mapped with GPS coordinates and photographs at multiple stages of implementation (before, during, and after work). These geo-referenced project points are displayed on Gram Manchitra's interactive map interface. For our analysis, the Gram Manchitra data provide a location-specific record of completed NREGS projects across states, allowing us to construct measures of spatial dispersion and equity in project placement within a gram panchayat.

We acquired the full Gram Manchitra data for sample states in September 2025, containing the GPS locations of 21 million completed NREGS projects between 2016 and 2023. A comparison with other NREGS administrative data indicates that this represents approximately 53.0 percent of all completed NREGS projects in the sample states over this period. The Gram Manchitra data do not include unique project identifiers that would allow project-level matching to other NREGS datasets, and we therefore cannot directly observe which types of projects are geo-tagged. However, it is unlikely that missing geo-tagged data are systematically related to the number of ward councilors, as shown in Table A3.[12]

[12] Columns (3) to (5) of Table A3 examine whether the running variables predict the presence of any geo-located data for a given gram panchayat. We also test whether the discontinuity is related to the fraction of completed projects with geo-coded data, calculated by comparing the number of geo-tagged projects in Gram Manchitra to the total number of completed projects reported in the other NREGS data. We find no statistically significant relationship for either design (b = 0.0030, se = 0.0091, p = 0.745; b = 0.0057, se = 0.0092, p = 0.539).

B.5 Social Audits

We use data from NREGS social audits conducted between 2016 and 2024 across all states of India (Ministry of Rural Development, 2025). These data were downloaded from the NREGS social audits website between November and December 2025. NREGS social audits are a large-scale, citizen-led monitoring mechanism designed to improve accountability in program implementation. Audits are conducted by lightly trained lay auditors, primarily women affiliated with government-organized self-help groups, who are deployed to gram panchayats outside their home areas to reduce local capture. Audit teams typically spend approximately one week in each gram panchayat verifying whether beneficiaries listed in official records exist and actually worked, whether wages were paid as recorded, whether assets were constructed, and whether procedural rules were followed. The audit process culminates in a public meeting (gram sabha), where findings are presented to citizens, local implementers, and elected officials, and grievances are publicly discussed. Final audit reports are submitted to the state Social Audit Unit, digitized, and used to track issue resolution and, in some cases, trigger administrative action.

Experimental evidence from Bihar shows that social audits are effective at detecting implementation irregularities and can meaningfully alter subsequent program behavior, particularly for violations that are easily observable by citizens, such as non-payment for work or beneficiary-level kickbacks (Pande et al., 2025). In our context, social audit data therefore provide a program-specific and policy-relevant measure of monitoring and governance quality that captures forms of misconduct that hyperlocal representatives could plausibly observe and raise with higher-level authorities.

B.6 Financial audits

For most gram panchayats across India, financial audits are conducted annually by the Ministry of Panchayati Raj (Ministry of Panchayati Raj, 2024). We use audit data for fiscal years 2020–2022, downloaded from the Ministry's website in May 2024. Appendix C.4 documents a strong relationship between violations detected in these financial audits and findings from NREGS social audits. Because NREGS social audit results have been independently validated using household survey data in prior work (e.g., Pande et al., 2025), this correspondence suggests that the financial audits also contain a meaningful signal component.

Financial audits are conducted annually by state Local Fund Audit departments or equivalent authorities, operating under standards and procedures issued by the Comptroller and Auditor General of India (Comptroller and Auditor General of India, 2011). The objective of the audit is to assess whether gram panchayat accounts are free from material misstatement due to fraud or error. Auditors examine annual receipt and payment accounts and supporting records maintained under the Model Accounting System for Panchayati Raj Institutions, including cash books, bank reconciliation statements, vouchers, muster rolls, asset registers, and inventory records. The audit process places particular emphasis on areas prone to misappropriation, such as procurement, grants, labor payments, and works execution.

Audit teams consist of professionally trained, district-level officials who conduct record-based verification, supplemented when necessary by physical inspection of works and assets. Detected issues are classified into standardized categories, such as procedural violations and financial misappropriation, and submitted to higher administrative authorities, where they may trigger corrective actions or or disciplinary measures. The financial audit data provide a professionally generated audit measure that complements social audits by capturing forms of misconduct that are less visible to citizens.

B.7 Ward members

There is no exhaustive roster of ward members across India, so we combine information from five data sources of varying quality and detail. The first two sources are election data from the State Election Commissions of Uttar Pradesh and West Bengal, which provide information on ward members elected in the most recent gram panchayat elections (Uttar Pradesh State Election Committee, 2021; West Bengal State Election Commission, 2023). Two additional sources come from independent initiatives within the Ministry of Panchayati Raj: the Gram Manchitra mapping platform (Ministry of Panchayati Raj, 2025b) and the eGramSwaraj database, a gram panchayat–level accounting system that also reports ward member names and gender (Ministry of Panchayati Raj, 2025a). While these two databases are intended to cover all of India, coverage is incomplete for some gram panchayats. The final source, the Mahaegram initiative, provides current ward member rosters for the state of Maharashtra (Government of Maharashtra, 2025).

Table A14 summarizes these data sources and their respective strengths and limitations. The primary challenge across all sources is incomplete coverage, either because data are missing entirely for some gram panchayats or because rosters do not include the full set of ward members (for example, reporting data for three members when population thresholds imply eleven). In addition, the latter two sources do not report ward member caste, which limits their usefulness for verifying the first stage in the second research design.

When a gram panchayat (GP) appears in multiple sources, we retain the source with the most complete roster. To calculate the total number of ward members in a GP, we select the roster that contains the largest number of ward members, conditional on observing at least five members.[13] For the sample states, the retained data source is Gram Manchitra for 68,647 GPs, eGramSwaraj for 41,272 GPs, one of the State Election Commissions for 37,925 GPs, and Mahaegram for 4,509 GPs.

For estimating ward member demographics in Section 2, we prioritize the source with the most detailed information, ranking sources as follows: State Election Commissions, Gram Manchitra, eGramSwaraj, and Mahaegram. We do not impose a minimum roster size restriction for this exercise. This procedure yields an all-India dataset in which the primary data source is the State Election Commissions for 54,942 GPs, Gram Manchitra for 74,585 GPs, eGramSwaraj for 69,577 GPs, and Mahaegram for 11,129 GPs. The resulting data cover 2.07 million unique ward members across 210,233 GPs, representing 78 percent of all GPs in India. Gender is observed for 95.5 percent of members, caste and education for 69.0 percent, and age for 28.6 percent.

[13] We estimate the first-stage relationship between the running variable and the number of ward members using GPs for which we observe at least five members, as rosters are likely to be incomplete below this threshold. The estimated relationship is similar without this restriction (estimate of 1.30 [se = 0.095] instead of 1.39), but somewhat smaller, likely due to increased measurement error from including GPs with missing roster data.

Table A14: Summary of Data Sources on Ward Members

Dataset	Coverage	Key Variables	Strengths	Limitations
State Election Commission	Uttar Pradesh, West Bengal	Age, Gender, Caste, Name; Education and Turnout (UP only)	Complete rosters of elected ward members; Contains electoral outcomes	Only available for two states
Gram Manchitra	All-India	Age, Gender, Caste, Name, Term start and end date	Relatively complete rosters for observed GPs	Missing rosters for many GPs
eGramSwaraj	All-India	Name, Gender	Coverage of most GPs across India	Rosters are frequently incomplete
Mahaegram	Maharashtra	Name, Term start and end date	Better coverage for Maharashtra than other data	Only for one state, rosters are frequently incomplete

Reflecting reservation policies, women constitute 50.8 percent of ward members. Caste composition is broad-based, with 24.6 percent general caste, 39.3 percent Other Backward Classes, 23.4 percent Scheduled Castes, and 12.7 percent Scheduled Tribes. Ward members are typically middle-aged, with a median age of 40 (10th to 90th percentiles: 26 to 58), and have low levels of formal education: 41.2 percent are illiterate or have less than primary schooling, while only 2.2 percent hold a college degree or higher. Overall, these data indicate that ward members are demographically representative of marginalized groups, although limited formal education may constrain their capacity.

To estimate the total number of ward members nationwide, we assume that the average number of ward members per gram panchayat is similar in the 22 percent of GPs for which roster data are unavailable. This implies a total of approximately 2.63 million ward members in India. Other large countries with comparable hyperlocal elected bodies include China, Indonesia, and the Philippines. China has approximately 650,000 village committees, typically comprising three to seven members, implying between 1.9 and 4.4 million council members. Indonesia has 74,961 desa with five to nine members each, implying between 0.37 and 0.67 million members. The Philippines has approximately 42,000 barangays with about seven councilors each, implying roughly 0.3 million barangay councilors. Even taking conservative estimates, these four countries alone account for at least 5.2 million hyperlocal representatives, suggesting that the global total is likely substantially larger.

B.8 Gram Panchayat Development Plan meetings

To assess whether ward members play a meaningful role in shaping local government deliberative processes, we use data from the Ministry of Panchayati Raj on meetings convened to formulate annual Gram Panchayat Development Plans (GPDPs) (Ministry of Panchayati Raj, 2025d). In these meetings, citizens and elected representatives discuss and decide on development priorities for the upcoming year. Such meetings constitute an important channel through which citizens can influence local government decisions and public goods provision (Rao and Sanyal, 2010; Ban et al., 2012; Das, 2015). For example, Das (2015) estimates that a one standard deviation increase in the relative attendance of women compared to men at a gram sabha increases the provision of sanitation and health facilities by 0.5 and 0.6 standard deviations, respectively. We observe five years of GPDP data from 2018-19 to 2022-23, covering 1.2 million meetings across 264,834 unique gram panchayats (including 744,711 meetings in our sample states).

These data record attendance at each meeting, with additional detail on the number of attendees from traditionally disadvantaged groups, including Scheduled Castes, Scheduled Tribes, and women. The data also indicate whether meetings discussed a set of topics designated as priorities for the GPDP process: (1) presentation and validation of data collected under the most recent Mission Antyodaya initiative (see Section B.2); (2) discussion of gaps identified by the Mission Antyodaya data and proposals to address them; (3) presentations by self-help group members on poverty-related issues and poverty reduction plans; (4) review of current-year local government expenditures; (5) discussion of resources and plans for the following year; (6) discussion of COVID-19 appropriate behavior (in 2020, 2021, and 2022); and (7) passage of a resolution on GPDP priorities for the subsequent year. In addition, for the years 2020 to 2022, we observe the sectoral focus areas prioritized in the GPDP for the following year (e.g. Roads, Sanitation), which we analyze in Test 6 in Section 4.4.

B.9 Matching

Most of the datasets used in the paper include unique identifiers from the Ministry of Panchayati Raj's Local Government Directory, enabling near-perfect matches. For data without standardized identifiers, we combine fuzzy name-matching algorithms with manual verification, following Narasimhan and Weaver (2024).

Details on matching success rates are reported in Table A3. Match rates are high for most datasets across both panels, with 11 of 18 exceeding 95 percent and 16 of 18 exceeding 90 percent. The two cases with lower match rates correspond to the NREGS social audit data, with match rates of 86.3% in Panel A and 81.0% in Panel B within the sample bandwidth. These lower rates arise because social audits have not been fully rolled out in a small number of states, rather than due to difficulties matching records across datasets. Among gram panchayats where social audits were conducted, the match rate exceeds 98 percent.[14] Importantly, we find no systematic relationship between the discontinuities we study and the probability of matching, indicating that whether a gram panchayat receives a social audit is unrelated to the number or caste composition of its ward members.[15]

[14]States in our sample where fewer than 75 percent of gram panchayats have conducted social audits include Chhattisgarh (53%), Haryana (60%), Madhya Pradesh (38%), Odisha (25%), and West Bengal (50%).

[15]While NREGS and NREGS (location) have an associated p-value of 0.063 and 0.085 in Panel B, this is consistent with chance given the number of regressions run.

C Construction and validation of outcome measures

C.1 Constructing the Measure of Alignment Between Citizen Preferences and Spending

A natural question is whether expanded representation causes public spending to better reflect what citizens actually want. Even if the total quantity of public goods does not change, representatives could improve governance by shifting the composition of spending toward citizen priorities. This appendix describes how we construct a measure of the alignment between citizen preferences and what projects the local government completes.

Measuring citizen preferences. We use data from Gram Panchayat Development Plan (GPDP) meetings, described in Appendix B.8. During these meetings, citizens can voice their preferences, and the GP sets a list of development priorities for the coming year, called the Gram Panchayat Development Plan (GPDP). For the years 2020-21 and 2021-2022, we observe the list of focus areas in the GPDP for the following year, which are coded into standardized categories such as drinking water, or roads. On average, a meeting sets 9.3 priority areas, where the five most common priorities (in order) are drinking water, education, agriculture, roads, and health. We use the priority areas set at GPDP meetings as a proxy for citizen preferences over development spending. As we will discuss below, this is less ideal than a private survey of citizen preferences, but it is the only available data for which we observe preferences across tens of thousands of GPs.[16]

Measuring project completion. NREGS projects are classified into categories, which happen to closely match the GPDP priority categories (e.g., drinking water, road connectivity). For each GP, we observe the number of projects completed in each category in the closest financial year following their GPDP meeting. We do not include projects involving improvement of an individual's own land, as that does not map cleanly to a particular priority category.

Constructing the alignment measure. We combine the GPDP priority data with the data on the types of NREGS projects completed in each GP to see how well they are aligned. For each GP, we calculate the fraction of NREGS projects completed in the financial year following a GPDP meeting that fall into the categories the meeting designated as priorities (e.g., the 2021-22 priorities for meetings that took place in 2020-21). This alignment rate averages approximately 45%. We then test whether this measure is responsive to either the number of ward members (using the density RD) or the fraction that are SC (using the identity RD). If expanded representation causes project selection to better reflect citizen preferences, we would expect the alignment rate to increase at the discontinuity.

This measure has several limitations that are worth noting. First, the GPDP priorities may not perfectly capture citizen preferences, as meeting attendance is not universal and more influential community members may have an outsized voice. Second, some GPDP

[16] The preferences set at the meetings could themselves be a function of the number or identity of ward members. However, Test 6 shows that having more ward members does not lead to a greater number of priority areas, which provides some reassurance that the priorities are not a function of the treatment.

priorities cover areas outside the scope of NREGS (e.g., education), while some NREGS projects may address needs that were not raised at the meeting. These factors would attenuate our estimates toward zero, though we note that the average alignment rate of 45% suggests that the two classification systems do capture meaningful overlap.

C.2 Measuring the Dispersion of Benefits Across Villages

If hyperlocal representatives advocate for the areas they serve, expanding the density of representation could lead to a more equal distribution of benefits across villages within a GP. This appendix describes how we construct a measure of the dispersion of Mission Antyodaya benefits across villages to test this prediction.

For each GP g with K villages, we observe the number of beneficiaries b_v for a given benefit in village v, along with village population p_v from the Census. We compute per-capita benefits in each village as $r_v = b_v/p_v$. Our measure of dispersion is the population-weighted coefficient of variation of per-capita benefits across villages within the GP:

$$CV_{g,b} = \frac{\sqrt{\sum_{v=1}^{K} \frac{p_v}{p_g}(r_v - \bar{r}_g)^2}}{\bar{r}_g}, \tag{2}$$

where $\bar{r}_g = \sum_{v=1}^{K} \frac{p_v}{p_g} r_v$ is the population-weighted mean of per-capita benefits across villages. We weight by village population so that variation driven by very small villages does not dominate the measure. A $CV_{g,b}$ of zero indicates that every village receives the same per-capita benefits; higher values indicate more unequal allocation. If expanded representation causes benefits to be spread more evenly across villages, we would expect the CV to fall at the discontinuity.

We compute $CV_{g,b}$ separately for each of the eight Mission Antyodaya benefit programs described in Appendix B.2. For our outcome variable, we take the average across programs, $\overline{CV}_g$. Averaging across programs is appropriate if we expect the treatment to affect dispersion similarly across benefit types, and has the additional advantage of reducing noise from any individual program.

This measure complements the spatial dispersion measure used for NREGS projects in the main text, which is based on the mean geographic distance between projects within a GP. The CV-based measure captures inequality in per-capita benefit allocation across administrative units (villages), whereas the distance-based measure captures geographic spread. The geographical spread NREGS measure is advantageous because it is a more granular measure of dispersion, whereas this only has more aggregated data at the village level; thus while the NREGS measure captures changes in both across- and within-village allocations, the method in this appendix only captures changes in across-village allocations. However due to the lack of individual benefits data that is disaggregated at a level below the village, it is not possible to capture within-village changes.

C.3 Measuring the Distribution of Benefits Across SCs and non-SCs

In section 4, we discuss how both the density and the identity of hyperlocal representatives affects the distribution of benefits among citizens, with a particular focus on how this impacts

SCs. In an ideal world, our benefits data would be measured at the household or group level, so that we could directly observe the number of individuals from a given group in a GP receiving each of the benefits in the Mission Antyodaya data. We could then see, for example, if SCs benefit differentially from these changes, and in particular from expanded representation from their own group. That would address the concern that we may not observe changes on aggregate because there is a reallocation from non-SCs to SCs without overall level changes.

While that is not possible with our data, a number of our outcome variables are measured at the village level rather than the GP level. For these outcomes, we can study the next best thing — whether expanded representation shifts benefits toward villages with a higher fraction of SC residents within the GP, consistent with shifts in benefits towards SCs. This appendix describes the construction of the distributional measure we use to test this.

Our objective is to study the relative distribution of benefits between SC and non-SC households. We observe total benefits at the village level, along with the SC population share in each village from the Census. We therefore use this information to construct a proxy for the share of benefits accruing to SC households. Suppose there are K villages in a gram panchayat (GP). Each village v has total population p_v and Scheduled Caste (SC) population p_v^{SC}. For a given benefit b, village v receives b_v units, and total GP-level benefits are $b_g = \sum_{v=1}^{K} b_v$. Since we only observe benefits at the village level, if we assume benefits are distributed uniformly within villages, we can approximate the total benefits accruing to SC households as

$$A_{g,b} = \sum_{v=1}^{K} b_v \frac{p_v^{\mathrm{SC}}}{p_v}. \tag{3}$$

We then compare this to a benchmark of equal per-capita allocation of benefits across villages within the GP. Under this benchmark, the expected benefits accruing to SC households are

$$E_{g,b} = \sum_{v=1}^{K} b_g \cdot \frac{p_v}{p_g} \cdot \frac{p_v^{\mathrm{SC}}}{p_v} = b_g \frac{\sum_{v=1}^{K} p_v^{\mathrm{SC}}}{p_g}. \tag{4}$$

We define our main measure as the ratio of actual to expected benefits, $s_{g,b} = \frac{A_{g,b}}{E_{g,b}}$. This measure compares the share of benefits accruing to SC households under the observed allocation to the share they would receive under equal per-capita allocation. Values of $s_{g,b} > 1$ indicate that benefits are disproportionately allocated toward villages with higher SC population shares, while values below one indicate the opposite. We are interested in whether additional SC representation increases $s_{g,b}$, i.e., shifts benefits towards villages with more SC households.

We apply this approach to two outcome domains. The first is the presence of NREGS projects. Here the benefit b_v is the number of NREGS projects that fall within the village boundary. To construct this, we take GIS data on village boundaries from the Survey of India, as provided by SHRUG (Asher et al., 2021). We intersect the geo-coded NREGS project locations from the Gram Manchitra data (described in Appendix B.4) with those boundaries to measure how many projects fall within a given village. We then compute $s_{g,b}$ to test whether NREGS projects are disproportionately located in villages with higher SC population shares.[17]

[17] Jeong et al. (2023) use individual-level NREGS records from one state, finding that election to local office

The second set of outcomes is the village-level Mission Antyodaya benefit data (described in Appendix B.2). We compute $s_{g,b}$ separately for each of the eight benefit programs and take the average across programs, $\bar{s}_g$, as our outcome variable.

An important limitation of this approach is that because the measure is constructed from village-level data, it captures reallocation of benefits across villages within a GP but does not capture within-village redistribution. If treatment affects the distribution of benefits within villages without changing their allocation across villages, this measure will not detect such effects. While we cannot rule out such within-village changes, we think this provides the best available evidence on the distributional margin given data constraints.

C.4 Cross-check of the audits data

Section 4.3 uses two data sources to measure monitoring and corruption. The first is financial audits conducted by the Ministry of Panchayati Raj, which review gram panchayat finances and are carried out by professional, district-level audit teams. The second consists of NREGS social audits, in which trained lay auditors from outside the village inspect recent works and interview residents to verify payments. While the financial audit data are novel, prior work shows that NREGS social audits are effective at detecting and deterring corruption (Pande et al., 2025).

To assess the informational content of these audits, we examine whether gram panchayats with higher detected violations in one system also exhibit higher violations in the other. Although the audits cover different functions and need not be strongly correlated, a positive relationship would indicate that both capture underlying governance quality and corruption. Using three years of overlapping data, Columns (1) and (2) of Table A15 regress financial audit violations on social audit violations in the same year, clustering standard errors at the gram panchayat level. Column (1) includes block and year fixed effects, while Column (2) includes gram panchayat and year fixed effects. The latter specification is particularly stringent, as it exploits within-gram panchayat variation to test whether years with more social audit violations also exhibit more financial audit violations. In both specifications, the estimated relationship is highly statistically significant, with t-statistics of 12.4 and 12.0.

Columns (3) and (4) repeat the analysis for violations closely linked to corruption. In financial audits, these include "Variation in accounts," "Irregularities," and "Non-production of records," while in the social audits they include violations labeled "Financial misappropriation" and "Financial deviations." We again find a strong positive relationship across the two audit systems, with t-statistics of 10.2 and 6.6 respectively. Together, these results suggest that both audit systems capture meaningful variation in GP governance quality.

While both audit systems appear to contain meaningful signal, they are imperfect measures of corruption. They are unlikely to detect all forms of corruption, particularly those that are less visible to locally trained lay auditors or hard to detect in formal records, such as collusion between contractors and officials or manipulation of procurement processes. If ward members are better positioned to influence these less observable forms of malfeasance

increases NREGS self-dealing. This suggests reallocation by winner identity but not necessarily aggregate effects large enough to detect here. Because we look at a larger number of states and due to changes by the government in access to individual-level NREGS data, it was difficult to get individual-level data for our paper.

than the types of violations that audits typically flag (e.g., non-payment of wages, ghost beneficiaries), our estimates would understate their true effects on corruption. We view this as unlikely given the limited formal authority of ward members, but cannot rule it out. Our estimates should therefore be interpreted as effects on detected violations, which may understate the true extent of irregularities.

Table A15: Relationship between detected financial and NREGS audit violations

	(1) Violations (finan.)	(2) Violations (finan.)	(3) Corruption (finan.)	(4) Corruption (finan.)
Violations (NREGS)	0.0860***	0.0810***	–	–
	(0.0076)	(0.0072)		
Corruption (NREGS)	–	–	0.0340***	0.0334***
			(0.0029)	(0.0034)
Block FEs	Yes	No	Yes	No
GP FEs	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
Dep var mean	10.1039	10.3170	2.6026	2.6057
Observations	187,330	137,417	187,330	137,417

Note: This table reports a regression of violations detected in the financial audits data on violations detected in the NREGS social audits data. The sample is from the years 2021-2023, with one observation for each GP and year in which there are audits from both data sets. Columns (1) and (2) report the total violations detected in each dataset, while columns (3) and (4) focus on violations more closely tied to corruption. Columns (1) and (3) include block fixed effects, while columns (2) and (4) include GP fixed effects. All columns include year fixed effects Standard errors are clustered at the GP level (parentheses). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D Additional analysis

D.1 Fuzzy RD Analysis

Section 3 shows that while the first-stage relationships are strong, their magnitudes indicate incomplete compliance with the underlying institutional rules. For example, the coefficient on the number of Scheduled Caste (SC) representatives is not equal to one, even though an additional seat should be reserved for and filled by an SC representative. This could reflect imperfect compliance by state election commissions, cases in which a seat is reserved but no candidate stands for office, or incomplete data on ward members (for example, if some members are not recorded). Given these complications, we present the reduced-form RD relationships in the main text, which provide interpretable intent-to-treat effects.

An alternative approach would be to estimate a fuzzy RD design using the discontinuities as an instrument for the number or identity of ward members. A key challenge with this approach is that administrative data on ward members are incomplete for many gram panchayats across states and years. A standard fuzzy RD would therefore require restricting the sample to gram panchayats with complete ward member data, reducing sample size and precision. Instead, we implement a fuzzy RD design using an RD analogue of two-sample instrumental variables (Angrist and Krueger, 1992, 1994; Inoue and Solon, 2010), in which the outcome and treatment discontinuities are estimated separately and then combined using a local Wald estimator (Hahn et al., 2001). Intuitively, this approach rescales the reduced-form RD estimates in Table 2 by the corresponding first-stage estimates in Table 1. Tables A16 and A17 report the resulting estimates. Because the first stage is very strong, these estimates are effectively rescaled versions of the reduced-form results and do not alter the substantive conclusions of the paper.

Table A16: Fuzzy RD for density of representation

	Aggregate outcomes			Distributional outcomes		Monitoring outcomes	
	(1) Workfare index	(2) Benefit programs index	(3) Aligned project fraction	(4) Workfare project dispersion	(5) Benefit spread (CoV)	(6) Audit violations	(7) Social audit violations
Fuzzy RD	-0.009	0.012	0.008	-0.033	0.005	0.139	-0.033
	(0.010)	(0.009)	(0.007)	(0.023)	(0.007)	(0.112)	(0.071)
	[0.364]	[0.195]	[0.246]	[0.153]	[0.450]	[0.215]	[0.637]
Dep var mean	-0.135	0.013	0.445	1.413	0.487	7.839	4.878
Effective Obs (RF)	271874	130411	44686	325030	44788	143880	153901
Effective Obs (FS)	24214	24214	24214	24214	24214	24214	24214
First-stage F	246.53	246.53	246.53	246.53	246.53	246.53	246.53

Note: This table reports fuzzy regression discontinuity estimates of the effects of additional hyperlocal representation on main governance outcomes. We report a Wald estimator of the ratio of the reduced form and first stage estimates. Outcomes are the same as in table 2. All outcomes are measured at the gram panchayat level and standardized where indicated. Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Table notes report the relevant first stage F-statistic as well as the effective sample size for the reduced form RD and first stage RD. Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A17: Fuzzy RD for density of representation

	Aggregate outcomes			Distributional outcomes		Monitoring outcomes	
	(1) Workfare index	(2) Benefit programs index	(3) Aligned project fraction	(4) Workfare project dispersion	(5) Benefit spread (CoV)	(6) Audit violations	(7) Social audit violations
Fuzzy RD	-0.021	0.016	0.015	-0.064	-0.038	-1.143	0.118
	(0.032)	(0.041)	(0.027)	(0.078)	(0.037)	(0.880)	(0.174)
	[0.508]	[0.700]	[0.577]	[0.406]	[0.304]	[0.194]	[0.498]
Dep var mean	-0.190	-0.016	0.477	1.419	0.541	12.304	3.869
Effective Obs (RF)	295717	77405	33657	239324	30178	91624	120643
Effective Obs (FS)	45020	45020	45020	45020	45020	45020	45020
First-stage F	55.14	55.14	55.14	55.14	55.14	55.14	55.14

Note: This table reports fuzzy regression discontinuity estimates of the effects of additional hyperlocal representation on main governance outcomes. We report a Wald estimator of the ratio of the reduced form and first stage estimates. Outcomes are the same as in table 2. All outcomes are measured at the gram panchayat level and standardized where indicated. Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Table notes report the relevant first stage F-statistic as well as the effective sample size for the reduced form RD and first stage RD. Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D.2 Extent of ward member influence

Our main results point to ward members not meaningfully affecting the core governance outcomes studied in Sections 4.1 through 4.3. A natural question is whether there are any dimensions of governance over which ward members can exert influence, even if these do not translate into those outcomes. Identifying such margins is useful for understanding the boundaries of ward member influence: if ward members can affect low-stakes procedural outcomes but not substantive policy, this supports the interpretation that their limited effects reflect constraints on authority rather than complete disengagement.

The 2022 round of the Mission Antyodaya data includes several variables related to GP governance practices that are well suited to this question. We examine five outcomes: (1) whether a list of beneficiaries of public programs is publicly posted at the GP office; (2) whether this list has been approved in a GP meeting; (3) whether a list of public works projects is publicly posted at the GP office; (4) the total number of standing committees in the GP; and (5) the total number of meetings held by all standing committees in the previous year. The first three capture whether the GP engages in basic transparency practices that ward members could plausibly advocate for without needing formal authority over budgets or staffing. The last two capture whether expanded representation affects the internal institutional structure of the GP.

Panel A of Table A18 shows that increased representative density has a modest but statistically significant effect on the first three outcomes. GPs with more ward members are more likely to publicly post beneficiary lists, to have these lists approved in a meeting, and to publicly post lists of public works projects ($p < 0.005$). These are the types of low-cost procedural actions that ward members could push for through verbal advocacy in meetings or informal pressure on the GP head or secretary, without requiring greater political power or authority. The fact that we detect effects here, but not on the substantive outcomes in Sections 4.1 through 4.3, is consistent with ward members having some voice within the GP but lacking the authority to translate this into changes in actual service delivery or corruption.

We also find that greater density increases the number of standing committees in the GP (column 4). However, there is no corresponding increase in the total number of standing committee meetings (column 5). To put this in context, the average GP has 5.07 standing committees that collectively meet only 8.1 times over the course of a year. The additional committees created by denser representation appear to be largely inactive, either not meeting at all or displacing meetings that would otherwise have occurred of existing committees. This pattern reinforces the broader finding that expanded representation can influence institutional form without substantive function.[18]

D.3 Understanding the Optimal Level of Representation

There are many theoretical channels relating the density of political representation to governance outcomes. On one hand, more localized representatives may better understand or

[18]Panel B shows no significant effects of SC representation on the transparency outcomes, though we do find a significant increase in total standing committee meetings. Again, given that we don't observe changes in substantive outcomes, and particularly those related to SCs, we interpret this as demonstrating the limits of ward member power.

Table A18: Gram Panchayat Administrative Outcomes

Panel A: Effect of additional political representatives					
	Beneficiary List		Works list	Standing Committees	
	Publicly posted	Approved in meeting	Publicly posted	Total number	Total meetings
RD_Estimate	0.0280***	0.0274***	0.0352***	0.2010**	-0.4468
	(0.0072)	(0.0087)	(0.0092)	(0.1008)	(0.3311)
	[0.000]	[0.002]	[0.000]	[0.046]	[0.177]
Dep var mean	0.729	0.747	0.642	5.081	8.098
Bandwidth	482.904	299.089	329.903	327.438	475.992
Effective Obs	73224	47887	52146	51132	71343
Panel B: Effect of additional representation of marginalized groups					
	Beneficiary List		Works list	Standing Committees	
	Publicly posted	Approved in meeting	Publicly posted	Total number	Total meetings
RD_Estimate	0.0001	-0.0019	0.0051	0.1511	1.1023**
	(0.0116)	(0.0114)	(0.0121)	(0.1483)	(0.5212)
	[0.996]	[0.870]	[0.674]	[0.308]	[0.034]
Dep var mean	0.710	0.718	0.606	5.201	9.466
Bandwidth	0.014	0.014	0.015	0.011	0.012
Effective Obs	28269	29090	30902	22216	24538

Note: This table reports regression discontinuity estimates of the effects of hyperlocal representation on main governance outcomes. Panel A reports effects of increases in the number of ward members, while Panel B reports effects of increases in representation from Scheduled Castes (SC). Columns (1) and (2) measure whether the GP publicly posts a list of beneficiaries of public programs and whether this list was approved in a GP meeting, respectively. Column (3) measures whether the GP publicly posts a list of public works projects. Column (4) reports the total number of standing committees in the GP, while column (5) reports the total number of meetings held by all standing committees in the previous year. All outcomes are measured at the gram panchayat level and standardized where indicated. Estimates use local linear RD with triangular kernels and MSE-optimal bandwidths (Calonico et al., 2014, 2018). Standard errors are clustered at the GP level (parentheses); p-values are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

align with constituent preferences (Tiebout, 1956; Oates, 1972), face stronger accountability through social and electoral sanctions (Seabright, 1996; Boffa et al., 2016), or be more accessible to citizens in facilitating access to government-provided benefits (Bussell, 2019). On the other hand, additional representatives may increase coordination costs, act as additional veto players that stall decision-making (Tsebelis, 2002), or lack the capacity or policy knowledge to influence outcomes (Auerbach et al., 2023).

The magnitude and sign of each of these mechanisms will vary at different levels of representation density. For example, consider direct democracy, where each individual is their own representative. At this level, going from slightly less dense representation to this level of density likely imposes quite high coordination costs relative to only marginal informational gains from further reducing constituency size. As a result, this is unlikely to be the optimal level of density. At the same time, in low density jurisdictions, the coordination costs of increasing density may be relatively low relative to the informational gains. The balance of these forces implies there is a non-monotonic relationship between density of representation and policy outcomes. For a particular outcome, there is likely an interior optimum: some level of representation that best balances the informational and accountability gains of more localized constituencies against the costs of fragmented decision-making.

The existence of an interior optimum has implications for interpreting our null results. One possibility is that GPs in our sample are already near this optimum, such that marginal increases in the number of ward members generates little benefit. Under this interpretation, our estimates are close to zero not because ward members lack capacity, but because the current scale of representation is approximately right. An alternative explanation, which we emphasize in the main text, is that ward members lack the formal authority, training, or incentives needed to influence governance outcomes, such that changes in representation density have little effect regardless of the starting point.

These two explanations generate different predictions about how treatment effects should vary with baseline representation density. The optimality interpretation predicts that effects should vary systematically: GPs with low baseline density (few ward members per capita) may be below the optimum, where additional representation improves outcomes, while GPs with high baseline density may be above the optimum, where additional representation worsens outcomes through coordination costs or other channels. Regardless of where these GPs lie relative to the optimum, we would expect heterogeneity in treatment effects related to baseline representative density. In contrast, the capacity explanation predicts that treatment effects should be uniformly close to zero across a range of baseline densities.

Our design, which stacks many discontinuities across a wide range of baseline representation levels, allows us to distinguish between these two explanations. We divide the sample based on the baseline density of representation to the left of the nearest population threshold. Specifically, for each GP, we compute the ratio of the number of ward members (to the left of the threshold) to the GP population. We then split the sample into quintiles of this baseline density measure and re-estimate Equation 1 within each quintile for each of the five main outcomes. If the optimality explanation holds, we should observe a pattern in which effects vary with baseline density.

Figure A4 plots the estimated treatment effects by quintile of baseline representation density for each of the five main outcomes. The estimates are consistently close to zero across all quintiles, with no evidence of the systematic variation that the optimality interpretation would predict. In particular, we do not observe positive effects at low baseline densities or negative effects at high baseline densities, nor any evidence of a crossing point.

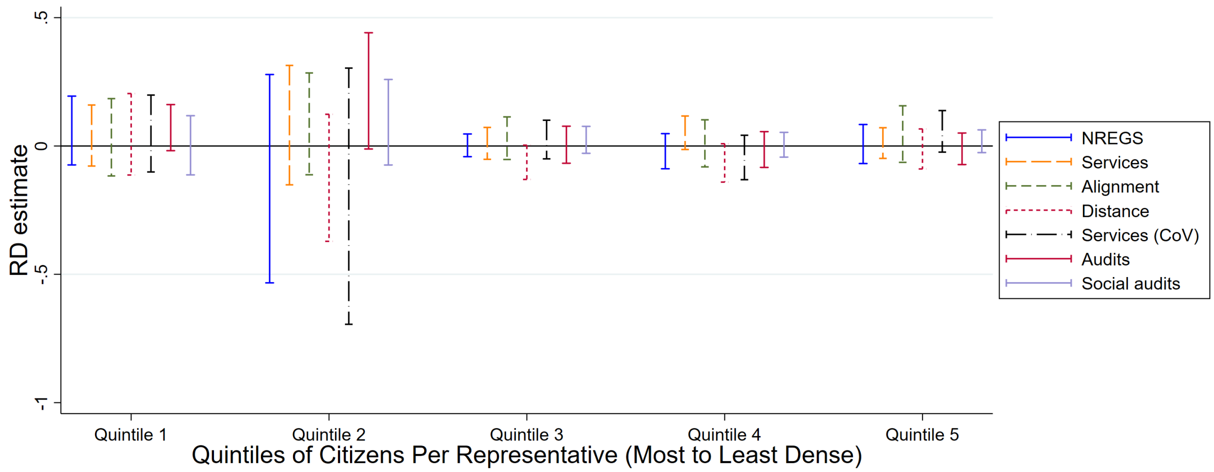
To be more precise about the range of variation we are exploiting, the bottom quintile of baseline density corresponds to GPs with an average of one ward member per 104 residents, while the top quintile corresponds to an average of one ward member per 421 residents. This represents substantial variation in the scale of representation: GPs in the top quintile have roughly four times the representative density of those in the bottom quintile. The range that we observe here likely covers at least the upper end of the policy-relevant range of density (i.e., it is difficult to imagine density greater than one representative per 100 citizens); whereas even greater levels of representative density like full direct democracy (i.e., one representative per citizen) are a useful thought exercise, they are unlikely to ever be implemented. Thus if there were an inverse-U relationship between representation density and governance outcomes over a policy relevant range of density, we would expect this range to be wide enough to detect it.

This test is related to but distinct from Test 1 in Section 4.4, which disaggregates effects by the number of ward members to the left of the nearest threshold. Test 1 was designed to look for diminishing marginal returns, i.e., whether treatment effects are larger when baseline representation is lower. The analysis here is designed to look for a non-monotonic pattern, i.e., whether treatment effects change sign as baseline density increases. Both tests point in the same direction: treatment effects do not vary meaningfully with the baseline level of representation.

We interpret the absence of systematic heterogeneity across baseline density levels as evidence against the optimality explanation and in favor of the capacity explanation. If the current scale of representation happened to be near the optimum, it would be a considerable coincidence for this to hold across the wide range of GP sizes and state-level institutional rules in our sample. The more parsimonious explanation is that ward members lack the formal authority, training, or incentives needed to influence governance outcomes, such that changes in representation density have little effect regardless of the starting point. This interpretation is also consistent with the other mechanism tests in Section 4.4, such as that voters do not appear to value these positions (Test 7).

We note two caveats. First, our test has limited power to detect non-monotonic patterns if the optimum lies outside the range of baseline densities in our sample. If all GPs in our sample are far above (or far below) the optimum, treatment effects would be uniformly signed (though we could still capture heterogeneity in the treatment effects). However, the range of representation densities across states and thresholds in our data is wide and likely covers most of the policy-relevant levels of decentralization. Even if there is a point theoretically at which the relationship could become non-monotonic, it may not be within the range that will ever actually be implemented. Second, the optimality and capacity explanations are not mutually exclusive: it is possible that GPs are near the optimum precisely because ward members lack capacity, such that neither adding nor removing them matters much. In this case, the policy implication is the same: expanding representation at this margin is unlikely to improve governance without complementary investments in authority or training.

Figure A4: Treatment effects by baseline representation density



Notes: These figures plot regression discontinuity estimates and corresponding confidence intervals for an investigation of whether the effects of representative density depend on the baseline level of density. Outcomes are standardized indices of NREGS workfare, Mission Antyodaya welfare access, alignment between citizen preferences and NREGS project composition, mean distance between NREGS projects (in km), inequality in benefit allocation across villages within a GP, financial audit violations, and social audit violations; for the outcomes for which we do not use the standardized index in the main text, indices are constructed by normalizing relative to the state-level mean and standard deviation. Quintile 1 represents the densest levels of representation at baseline (average of one representative per 104 citizens), while quintile 5 represents the least dense level of representation at baseline (average of one representative per 421 citizens). All estimates use local linear RD specifications with triangular kernels and MSE-optimal bandwidths.