The Limits of Electoral Gender Quotas in Rural Local Bodies

Varun Karekurve-Ramachandra* Gaurav Sood†

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Abstract

We study whether women win unreserved seats at higher rates when those seats were previously reserved. Our data cover over 25,000 local government bodies across four election cycles spanning sixteen years in rural India, where seats are randomly reserved for women. In Rajasthan, prior reservation has null to negative effects on women's subsequent electoral success. In Uttar Pradesh, effects are positive but small: 1–4 percentage points on a base of 15–19%, an order of magnitude below the roughly 15 percentage point effects documented in urban settings. Minimum detectable effects of 1.7–4.0 percentage points confirm these are not power failures. Cumulative exposure over multiple cycles does not compound: in UP, all two-way interactions between quota assignments across cycles are zero or negative, and the one significant three-way interaction does not survive restriction to districts with cleaner identification. Phone audits of roughly 1,000 randomly sampled representatives reveal a mechanism: male relatives answered 85% of calls to quota-seat officeholders, and only 9% of calls reached the elected woman herself, compared with 76% in open seats. Women elected under quotas are also younger, less educated, and less wealthy than their open-seat counterparts. Quotas achieve *de jure* representation but, where male relatives govern by proxy, do not create the conditions for women to retain power independently.

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*Assistant Professor, Department of Political Science and International Relations, University of Southern California, Email: varun.rama@usc.edu

†Independent Researcher, Email: gsood07@gmail.com

1 Introduction

Women's representation in elected office matters for both intrinsic and instrumental reasons. Equal citizenship is difficult to reconcile with decision-making bodies in which half the population is largely absent. The instrumental case is equally strong: India's political reservations for women show that female leaders shift spending toward public goods preferred by women (Chattopadhyay and Duflo, 2004), that exposure to women leaders narrows gender gaps in aspirations by 20–32% and eliminates gender gaps in educational attainment (Beaman et al., 2012), and that women's political voice strengthens institutional responsiveness, including greater reporting of crimes against women (Iyer et al., 2012). Despite this evidence, women remain sharply underrepresented in elected bodies worldwide. Many countries have adopted electoral quotas in response, betting that temporary mandates will prove self-sustaining: exposure to female leaders shifts voter attitudes, women acquire political skills and networks, and a pipeline of viable candidates emerges that no longer depends on the quota.

We test this expectation in rural India, where a constitutional mandate reserves at least one third of local government leadership positions for women. Using sixteen years of electoral data from over 25,000 Gram Panchayats across four election cycles in Rajasthan and Uttar Pradesh, we ask whether seats previously reserved for women are more likely to elect women once the reservation is lifted.

We find that prior reservation has limited effects on women's subsequent electoral success. In Rajasthan, short-run estimates are zero or negative across three election transitions. In Uttar Pradesh, effects are positive but small: 1–4 percentage points on a base of 15–19%, an order of magnitude below the roughly 18 percentage point effect Bhavnani (2009) documented in urban Mumbai. Our samples are large enough that minimum detectable effects at 80% power range from 1.7 to 4.0 percentage points, ruling out urban-scale impacts. Cumulative exposure over multiple election cycles does not compound. In Rajasthan, short-run effects are already null, leaving little for repeated exposure to build on. In UP, where short-run effects are positive and the data are

most informative, all pairwise interactions between quota assignments across different cycles are zero or negative.

We document two features of the institutional environment that help explain these results. First, male relatives perform the representative function in the vast majority of quota seats. In a phone audit of roughly 1,000 randomly sampled Sarpanches in Rajasthan, only 9% of calls to quota-seat representatives reached the elected officeholder; 85% were answered by male family members. In open seats, 76% of calls reached the representative directly. This proxy governance severs the causal chain at its first link: if citizens interact with male relatives rather than the female officeholder, voters cannot update their beliefs about women's leadership, and women in office cannot accumulate the political capital or visibility that would let them or others compete without a quota.

Second, women elected under quotas are less professionalized than open-seat winners. In Rajasthan, quota-seat winners are younger, have 11 percentage points lower graduation rates (12% vs. 24%), own fewer assets, and are far more likely to report being unemployed (61% vs. 10%). These gaps may reinforce rather than challenge voter stereotypes about women's political competence, further weakening the exposure channel.

A suggestive piece of evidence on scope conditions comes from extending the phone audit to urban Jaipur, where 31.7% of calls to quota-seat representatives reached the elected officeholder, compared with 9% in rural areas. Proxy governance is less prevalent in urban settings, though it remains the norm even there. This gradient is consistent with the gap between our rural results and the large urban effects reported by Bhavnani (2009), and suggests that the conditions under which quotas generate lasting representation—women actually governing, a professionalized female candidate pool, voter exposure to female leadership—are more likely to be met in urban settings where household structures and labor markets differ.

Our contribution is threefold. First, we provide the largest and longest test of whether electoral quotas produce lasting gains in women's representation, covering over 25,000 electoral bodies across sixteen years. Previous studies of lasting electoral effects examined single cities

or single transitions (Bhavnani, 2009; Clayton and Tang, 2018). Our data span two states, four election cycles each, and allow us to compare the effects of single and repeated quota exposure. Second, our phone audit provides direct evidence of male proxy governance at scale, revealing how formal inclusion can coexist with effective exclusion from power (Valdini, 2019; Brulé et al., 2025; Meguid et al., 2025). Third, the contrast between our rural results and the large urban effects in Bhavnani (2009) helps delineate the scope conditions under which quotas can and cannot generate self-sustaining representation.

The implication is not that quotas are ineffective. Quotas guarantee descriptive representation, and compliance with India's mandate has placed nearly a million women in elected local office. The implication is that quotas alone are insufficient to generate self-sustaining female representation in settings where household-level power structures prevent women from exercising the authority that office confers. Complementary interventions that address proxy governance directly may be necessary for quotas to fulfill their transformative potential.

2 Theoretical Framework

For quotas to generate lasting change, they must shift either voter behavior or the pool of women willing and able to contest elections.

On the demand side, quotas can change voter behavior through two routes. The first operates through statistical discrimination. Voters uncertain about women's competence as leaders may use gender as a proxy for quality. Observing a woman govern provides information that updates these beliefs. If the female officeholder delivers public goods, manages budgets competently, or proves responsive to constituents, voters learn that women can govern effectively and become more willing to support female candidates in future open elections. Beaman et al. (2009) provide direct evidence: two cycles of exposure to female leaders in Indian village councils reduces the component of gender bias attributable to beliefs about competence. The content of this updating may include policy shifts — Chattopadhyay and Duflo (2004) show that female leaders redirect

spending toward drinking water and road infrastructure — but the mechanism is voter learning, not the policy change itself. What matters is that voters observe the outcome, attribute it to the female leader, and revise their priors about women's capacity to govern.

The second demand-side route operates through taste-based discrimination. Voters may hold an intrinsic preference for male leaders independent of beliefs about competence — a distaste for female authority rather than a doubt about female ability. Exposure to women in office could erode this preference through familiarity or norm change. However, the empirical evidence for this channel is weak. Beaman et al. (2009) find that even after two election cycles of exposure, taste-based prejudice against women persists. This channel is theoretically possible but should not be expected to do heavy lifting.

On the supply side, quotas may expand the pool of women willing and able to contest elections. Women who serve under quotas accumulate political skills, constituent relationships, name recognition, and access to party networks, all of which lower the barriers to contesting future elections without a mandate (Barnes and Holman, 2020; Goyal, 2025). Their presence in office may also signal to other women that political careers are feasible, encouraging new entrants (Beaman et al., 2012; Prillaman, 2023). This channel operates through both direct experience — the officeholder herself becomes a stronger future candidate — and demonstration effects, as other women see a viable path.

All three channels share a precondition: women elected under quotas must exercise genuine political authority. If voters interact with a male proxy rather than the female officeholder, the informational basis for belief updating is absent — voters cannot revise their priors about women's competence because they have not observed a woman governing. If the officeholder does not participate in decision-making, she cannot accumulate the political capital that would make her a viable candidate in an open election. And if other women observe that the quota-seat holder is a figurehead, the demonstration effect points away from entry rather than toward it. Proxy governance severs all three channels simultaneously.

Two features of the institutional environment can produce exactly this failure. The first is male proxy governance itself. In settings where women's political agency is curtailed by household power structures, male relatives may perform the representative function while women hold office in name (Tate, 2004; Dahlerup, 2008; Valdini, 2019; Meguid et al., 2025).[1] The elected woman is the *de jure* officeholder; a husband, father-in-law, or son is the *de facto* decision-maker. Male proxies may still deliver beneficial policies, but voters attribute these outcomes to the male relative, not the female officeholder, preventing the belief updating that the demand-side channels require.

The second is weak candidate professionalization. The pool of women who contest under quotas may differ systematically from the pool of candidates in open seats. If quota-elected women are less educated, less experienced, and less economically independent than their open-seat counterparts, they may lack the resources and autonomy to resist proxy governance, and voters may perceive them as less competent (Weeks and Baldez, 2015; Allen et al., 2016). These perceptions reinforce rather than challenge existing stereotypes about women's political capacity (Hansen, 1997; Atkeson, 2003). The two failure conditions compound: less professionalized candidates are more susceptible to proxy governance, and proxy governance prevents candidates from acquiring the experience that would professionalize them.

2.1 Predictions

These arguments generate four testable predictions.

First, if the demand- and supply-side channels operate, reserving a seat for a woman in one election should increase the probability that a woman wins or contests that seat in the next open election. If proxy governance and weak candidate professionalization dominate, this effect should be small.

Second, if quotas work through gradual erosion of discrimination and incremental pipeline expansion, the effect should grow with repeated exposure. Two cycles of reservation should

[1] See pp. 368–376 in Thakur 2015 for an example from the Indian state of Bihar.

produce a larger effect than one. If proxy governance persists across cycles, cumulative exposure should add little.

Third, if the barriers to quota effectiveness are features of underdeveloped, patriarchal settings, quota effects should be larger in more developed GPs — closer to towns, with better infrastructure, and higher female literacy. This prediction also speaks to the gap between the large effects documented in urban Mumbai (Bhavnani, 2009) and the effects we estimate in rural settings.

Fourth, if proxy governance is widespread, a direct measure of who exercises political authority should reveal that citizens overwhelmingly interact with male relatives rather than the female officeholder when contacting quota-seat representatives. The contrast with open seats, where the officeholder is typically male and performs the representative function directly, provides a benchmark.

We test these predictions using electoral panel data, a phone audit of elected representatives, and candidate-level administrative records from two Indian states.

3 Institutional Background

India's rural local governance operates through a three-tier system of Panchayati Raj Institutions (Figure 1). At the top is the Zilla Parishad, the elected body at the administrative district level. Below it is the Panchayat Samiti (PS), which operates at the block level. At the base is the Gram Panchayat (GP), typically comprising one or more villages, and the unit at which quotas are assigned. Our study focuses on the GP level.

Each GP is governed by an elected body of ward members headed by the Sarpanch (president). The Sarpanch wields significant executive power: overseeing village development, managing service delivery, presiding over meetings of the Gram Sabha (the assembly of all registered voters in the GP), and identifying beneficiaries for government welfare schemes.[2] Elections are non-

[2]For details on governance responsibilities, see https://www.pria.org/panchayathub/panchayat_text_view.php.

partisan and held every five years, a feature that distinguishes this setting from the partisan urban contexts (such as Mumbai municipal wards) where scholars have documented large and lasting effects of quotas (Bhavnani, 2009).

India's 73rd Constitutional Amendment (1992) mandates that at least one third of Sarpanch positions be reserved for women. Many states, including Rajasthan and Uttar Pradesh, have raised this threshold to 50%. In both states, gender quotas are implemented alongside caste-based reservations for Scheduled Castes (SC), Scheduled Tribes (ST), and Other Backward Classes (OBC), producing a cross-classified assignment in which some seats are reserved on both gender and caste dimensions. Our analysis focuses on the gender dimension: we compare GPs where the Sarpanch seat was reserved for women (quota seats) with GPs where the seat was not reserved for women (non-quota seats). In both categories, caste reservations may still apply.

Rajasthan and Uttar Pradesh together account for roughly 20% of India's population. Rajasthan is India's largest state by area with over 70 million residents; Uttar Pradesh is the most populous with over 200 million. In both states, more than 70% of the population is rural (Figure 2). Table A1 reports the number of GPs, Panchayat Samitis, and districts in each state across the election cycles in our data.

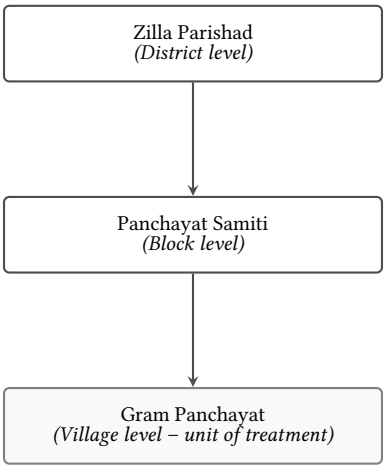

Figure 1: Local Governance Structure in Rajasthan and Uttar Pradesh

Notes: The Zilla Parishad operates at the administrative district level. Panchayat Samitis are intermediate bodies at the block level. Gram Panchayats are collections of one or more villages and the unit at which gender quotas are assigned.

Figure 2: Study States: Rajasthan and Uttar Pradesh

Notes: Highlighted states are the study sites. Rajasthan (darker grey, west): India's largest state by area (342,239 km^2), population 68.5 million (Census 2011), approximately 75% rural, with 11,300 Gram Panchayats. Uttar Pradesh (lighter grey, east): India's most populous state, population 199.8 million (Census 2011), approximately 77% rural, with 58,000 Gram Panchayats.

4 Data and Research Design

4.1 Data

We assemble four data sources for this study.

Electoral data. We scraped GP-level election records from the Rajasthan and Uttar Pradesh State Election Commissions for four cycles each: 2005, 2010, 2015, and 2020 in Rajasthan, and 2005, 2010, 2015, and 2021 in UP. For each GP-election, we observe the reservation status of the Sarpanch seat (reserved for women, or open) and the sex of the elected Sarpanch. In Rajasthan 2015, the winner's sex was missing from official records and was manually coded from names. For Rajasthan 2020, we additionally collected candidate-level affidavit data (age, education, assets) from the SEC. Table A1 summarizes the electoral data. Between 2005 and 2020, the number of GPs in Rajasthan grew from roughly 9,200 to 11,500 (↑ 25%), reflecting delimitation driven by population growth and urbanization. The number of Panchayat Samitis increased from 237 to 352 (↑ 49%), while the number of districts was nearly constant, with only Pratapgarh added in 2008. In UP, the number of GPs, blocks, and districts remained approximately constant across cycles. As a validation check, we replicate the UP short-run results using independently collected data from Weaver (2022).

Linking GPs across election cycles. Constructing a panel of GPs across elections requires matching administrative units whose names vary over time due to transliteration inconsistencies, OCR errors in digitized records, and administrative renaming. We proceed in three steps. First, we standardize raw names within each election year using transliteration normalization (via the `stringi` package) and manual correction of OCR artifacts (e.g., "JAW AJA" → Jawaja, "BANSW ARA" → Banswara). Second, we construct validated crosswalks at each level of the administrative hierarchy. For districts, we manually map every raw election district name to its canonical SHRUG or LGD equivalent, handling spelling variants (Chittorgarh/Chittaurgarh), abbreviations (S. Madhopur/Sawai Madhopur), OCR errors, and post-2010 renamings (Allahabad →

Prayagraj, Faizabad → Ayodhya). For sub-district units (Panchayat Samitis in Rajasthan, blocks in UP), we build analogous crosswalks to LGD block codes, achieving 100% coverage of election units. In UP, this includes tracking blocks that moved between districts when Shamli, Sambhal, Hapur, and Amethi were carved out after 2010. Third, we match GPs across cycles by joining on normalized district-samiti-GP name strings, and manually verify a random sample of matches for accuracy.

Village-level covariates. We link GPs to pre-treatment village characteristics via two additional data sources. The Local Government Directory (LGD), the Indian government's authoritative registry of rural administrative units, provides the mapping between GPs and census villages for 2020.[3] We match election GPs to LGD GPs within each block using a combination of exact matching on normalized GP names and fuzzy string matching (Jaro-Winkler distance < 0.20) for remaining unmatched units. We use this mapping, together with the GP-to-LGD crosswalks described above, to join election data to village-level attributes from the Socioeconomic High-resolution Rural-Urban Geographic Platform for India (SHRUG 2.0; Asher et al., 2021), which harmonizes data from the 1991 and 2001 Census Abstracts and Village Directories (Office of the Registrar General and Census Commissioner, 2011; Chandramouli and Registrar General of India, 2011). We aggregate village-level covariates—population, female population, SC/ST composition, medical facilities, schools, water infrastructure, banking, power supply, and road access—to the GP level and use these for balance tests and covariate-adjusted specifications.

Phone audit data. In 2024, we conducted a phone survey of Sarpanches elected in the 2020 Rajasthan elections, sampling separately from quota and open seats. We report these results separately in Section 7.1.

[3] https://lgdirectory.gov.in/

4.2 Research Design

Our estimand is the effect of reserving the Sarpanch position for women in one election on the probability that a woman wins in the subsequent election, conditional on the seat being open. We begin with the short-run analysis, where identification is most straightforward, then extend to cumulative exposure.

Short-run specification

For each consecutive pair of elections, we estimate:

$$\text{FemaleWinner}_{g,t+1} = \beta \cdot \text{Quota}_{g,t} + \lambda_d + \varepsilon_{g,t+1}, \tag{1}$$

where g indexes GPs, $\text{FemaleWinner}_{g,t+1}$ is an indicator for a woman winning the open seat in the next election, $\text{Quota}_{g,t}$ indicates whether GP g was reserved for women in the current election, and λ_d denotes district-samiti (Rajasthan) or district-block (UP) fixed effects. The sample is restricted to GPs whose seat is open in $t+1$. We report all specifications with and without fixed effects and with and without pre-treatment village characteristics from the 2001 Census Village Directory, accessed via SHRUG 2.0 (Asher et al., 2021). The coefficient β is the intent-to-treat effect: the impact of having been reserved on the probability that a woman wins in the next open election. Our primary outcome is whether a woman wins. We also examine whether a woman contests, which speaks directly to the supply-side channel: if quotas expand the pipeline of female candidates, this should be visible in candidacy rates even if it does not translate into wins.

Note that this estimand captures the full causal chain from reservation to subsequent electoral outcomes, including any behavioral responses to the anticipated political environment. One might worry that candidates anticipate whether their seat will be open in the next cycle and adjust their behavior accordingly. A female Sarpanch who expects her seat to become unreserved may invest differently in building a political base than one who expects continued reservation. This concern exists under any assignment mechanism but is strongest when future open status

is more predictable. It is not a threat to identification so long as anticipation is symmetric: the assignment rules are public information, and candidates in both reserved and open GPs can update their expectations about future reservation status.

Identification requires that quota assignment be independent of potential outcomes. Prior work treats assignment in these states as random or as-if random. In Rajasthan, assignment is reported to follow a lottery within each Panchayat Samiti (Chattopadhyay and Duflo, 2004; Dunning and Nilekani, 2013; Brulé et al., 2025). In Uttar Pradesh, assignment is described as following an administrative rule based on population size within blocks. We assess these claims directly.

In Rajasthan, covariate balance is excellent across all three short-run panels. Joint likelihood ratio tests yield $p = 0.30$ (2005→2010), $p = 0.07$ (2010→2015), and $p = 0.21$ (2015→2020); randomization inference omnibus tests are nearly identical (Table B6). One individual covariate in the first panel reaches conventional significance (FW Centres $p = 0.02$), consistent with chance given fifteen tests. Prior female winner rates do not differ between quota and open GPs ($p = 0.50$), and caste composition is balanced (χ^2 $p = 0.62$).

In UP, the picture is more mixed. Joint balance tests reject for all three panels ($p < 0.01$; Table B7), with statistically significant differences on population, female population, and infrastructure variables. For the 2005→2010 and 2010→2015 panels, these rejections reflect the large sample sizes ($N \approx$ 10,000–18,500) rather than substantive imbalance: standardized differences remain below 0.15, under the conventional threshold of 0.20. However, the 2015→2021 panel shows larger imbalances, driven primarily by population (standardized difference 0.41), with several infrastructure variables also exceeding 0.20 (handpumps, mud roads, power supply). OBC and SC/ST reservation shares also differ between quota and open GPs, consistent with the rotation pattern (Table B5). We address these imbalances in two ways. First, district-block fixed effects absorb any confounds that operate at the level at which rotation is administered. Second, we control directly for the GP-level characteristics that are imbalanced: population, SC share, literacy, female population share, and an infrastructure index (sum of education, medical, power,

and banking facilities) from the 2001 Census via SHRUG. Estimates are stable across all four specifications: no controls, fixed effects only, SHRUG covariates only, and both together.

Placebo tests provide a further check. If quota assignment is effectively random with respect to the outcome, future treatment should not predict past outcomes. We regress the outcome in the prior election on quota status in the current election. Across all six panels and both fixed effects specifications, every placebo coefficient is zero to two decimal places and statistically insignificant (Table D1).

Rotation and its implications

A complication arises from quota rotation. Both states shift quota assignments across election cycles: a GP reserved in one election is less likely to be reserved in the next (Tables B1 and B2). In Rajasthan, the earliest transition (2005–2010) shows no rotation ($\hat{\beta} \approx 0.01$, $p > 0.3$), consistent with independent lottery draws. The chi-square test confirms independence ($\chi^2 = 1.41$, $p = 0.23$; Table B3). Subsequent transitions show strong rotation ($\hat{\beta} = -0.40$ to -0.29, $p < 0.01$), indicating a policy shift after 2010. In UP, even the first transition shows modest rotation ($\hat{\beta} = -0.02$, $p < 0.01$), with stronger rotation in later periods. The chi-square test rejects independence for all transitions except 2005–2021 (Table B4). A four-way balanced panel restricted to GPs observed in all cycles yields nearly identical rotation coefficients (Appendix Table B2).

Rotation matters for identification because we observe outcomes only for GPs whose seats are open in the next election. Since rotation links past quota status to future open status, conditioning on the seat being open is conditioning on a post-treatment variable. This is harmless if rotation is purely mechanical, determined by prior quota status alone and orthogonal to GP characteristics that predict female electoral success. It becomes a threat if rotation is confounded. If, for instance, larger or more developed GPs are systematically rotated out of reservation earlier, conditioning on open status could induce collider bias.

The balance evidence bears on this question. In Rajasthan, covariate balance remains strong even in the later panels where rotation is heaviest, suggesting that rotation does not operate

through observable GP characteristics. In UP, the small standardized differences and stable estimates across specifications with and without controls point to the same conclusion. To assess this more directly, we conduct chi-square tests of independence between current and prior quota assignment at the district level, identifying districts where rotation appears consistent with independent draws and districts where it does not. We then restrict the sample to districts where the chi-square test does not reject independence and re-estimate Equation 1. Results are similar on these subsets (Tables B3 and B4), providing further evidence that rotation-induced selection is not driving our findings.

Cumulative exposure

The short-run specification asks whether a single cycle of reservation increases women's chances of winning in the next open election. A natural extension is whether repeated exposure compounds: does a GP reserved in two or three prior cycles show larger effects than one reserved in only one? We estimate effects of quota assignment in 2005 on outcomes in 2015 or 2020/2021, restricting to GPs open in the outcome year. We also estimate a treatment intensity specification that includes quota assignment indicators for multiple prior cycles and their interactions, recovering the marginal effect of each additional cycle of reservation. Identification here rests on the same assignment mechanism as the short-run analysis, but is complicated by the fact that cumulative exposure is partly a function of rotation patterns across intervening cycles. Because rotation is non-random in many districts after 2010, we also restrict the cumulative analysis to districts where chi-square tests do not reject independence of quota assignment across cycles. This subsetting limits the Rajasthan analysis to outcomes through 2015 but provides a sample where the variation in cumulative exposure is more plausibly exogenous. We treat the cumulative results throughout as informative about dose-response but less definitive than the short-run estimates.

Power and replication

Minimum detectable effects at 80% power are approximately 2.8–3.9 percentage points in Rajasthan (28–37% of the control mean) and 1.7–1.9 percentage points in UP (10–12% of the control mean). For comparison, Bhavnani (2009) found roughly an 18 percentage point effect in urban Mumbai (Appendix Table C1). As a final validation, we replicate the UP short-run results using independently collected data from Weaver (2022). The point estimates are consistent across both datasets.

5 Results

5.1 Short-run Effects

Table 1 reports estimates of Equation 1 for each short-run panel, with and without district-samiti (Rajasthan) or district-block (UP) fixed effects. The outcome is whether a woman wins an open GP seat in the next election; the treatment is quota assignment in the current election.[4]

In Rajasthan, the 2005→2010 panel yields an estimate of 1 percentage point ($p > 0.10$). The 2010→2015 panel yields −2 percentage points without fixed effects and −3 percentage points with fixed effects (both $p < 0.05$). The 2015→2020 panel yields a precise zero. In Uttar Pradesh, effects are consistently positive and statistically significant: 2 percentage points for 2005→2010 ($p < 0.01$), 1 percentage point for 2010→2015 ($p < 0.01$ without FE, $p < 0.05$ with FE), and 4 percentage points for 2015→2021 ($p < 0.01$).

The negative Rajasthan estimate for 2010→2015 warrants comment. This is the first transition where strong rotation appears ($\hat{\beta} = -0.41$ for lagged quota predicting current quota; Table B1). When we restrict to the eight districts (of 33) where within-district chi-squared tests fail to reject independence of quota assignment, the estimate flips to +2 percentage points (Table E2), though

[4]Gender quotas are implemented alongside caste quotas. In seats with both caste and gender quotas, only women from the reserved caste group may contest. In seats with only a caste quota (no gender quota), both men and women from the reserved caste group may contest. Our results indicate that when a seat loses its gender quota, women are not substantially winning or contesting regardless of caste category.

the restricted sample ($N = 777$) lacks power. The negative full-sample estimate thus likely reflects compositional differences between quota and open GPs introduced by non-random rotation, not a genuine backlash effect of quotas.

The UP effects, while statistically significant, are small in absolute terms. The control mean (proportion of women winning open seats without prior reservation) ranges from 15% to 19% across panels, so a 2–4 percentage point increase represents a 10–25% gain relative to the baseline. These magnitudes are an order of magnitude smaller than the roughly 18 percentage point effect reported by Bhavnani (2009) in urban Mumbai. Our null and small results are not driven by lack of power: minimum detectable effects range from 1.7 to 4.0 percentage points at 80% power (Appendix Table C1), and we would detect a Bhavnani-sized effect with over 99% power in every panel.

Table 1: Short-run Effects

	Rajasthan						Uttar Pradesh					
	05→10		10→15		15→20		05→10		10→15		15→21	
	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE
	[i]	[ii]	[iii]	[iv]	[v]	[vi]	[vii]	[viii]	[ix]	[x]	[xi]	[xii]
Intercept	0.09***		0.09***		0.12***		0.14***		0.16***		0.19***	
	(0.00)		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)	
Quota_{t-1}	0.01	0.01	-0.02**	-0.03**	0.00	0.00	0.02***	0.02***	0.01***	0.01**	0.04***	0.04***
	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
R^2	0.00	0.10	0.00	0.15	0.00	0.12	0.00	0.06	0.00	0.05	0.00	0.05
Observations	4,062	4,062	3,867	3,867	3,962	3,962	26,444	26,418	22,709	22,706	27,919	27,919
(District, Samiti) FE		✓		✓		✓		✓		✓		✓

Notes: ***p<0.01; **p<0.05; *p<0.1. Outcome: woman elected in open seat. Sample restricted to GPs where seat was not reserved for women in outcome year. Heteroskedasticity-robust standard errors.

5.2 Cumulative Exposure

If quotas generate lasting change through gradual erosion of discrimination and incremental pipeline expansion, the effects should grow with repeated exposure. We test this using the full interaction model, regressing whether a woman wins an open seat in the final election (2020 in Rajasthan, 2021 in UP) on quota status in each prior election and all two- and three-way interactions. Table 2 reports the results.

In Rajasthan, the individual treatment indicators for 2005 and 2015 are not significant. The 2010 quota indicator is positive and significant (5 percentage points without FE, $p < 0.10$; 7 percentage points with FE, $p < 0.01$). Among the interactions, $\text{Quota}_{2005} \times \text{Quota}_{2015}$ is positive but not significant (6 pp without FE, 3 pp with FE), $\text{Quota}_{2005} \times \text{Quota}_{2010}$ is small and insignificant (+4 pp without FE, +2 pp with FE), and $\text{Quota}_{2010} \times \text{Quota}_{2015}$ is negative and insignificant (-1 to -4 pp). The three-way interaction is -5 to -2 percentage points and not significant. The sum of all seven coefficients, representing the cumulative effect for GPs reserved in all three prior elections, is approximately 10 percentage points with fixed effects, but the linear combination is not statistically significant. There is no evidence that repeated reservation in Rajasthan builds toward a self-sustaining increase in female representation.

In Uttar Pradesh, the coefficient structure undermines a simple dose-response interpretation. Only the most recent quota (2015) has a meaningful individual effect (5 pp with FE, $p < 0.01$). The 2005 indicator contributes 2 percentage points ($p < 0.10$), and the 2010 indicator is zero. More importantly, all three two-way interactions are negative or zero: $\text{Quota}_{2005} \times \text{Quota}_{2010}$ is -0.02, $\text{Quota}_{2005} \times \text{Quota}_{2015}$ is -0.02, and $\text{Quota}_{2010} \times \text{Quota}_{2015}$ is -0.00. This means that a GP reserved in any two prior periods does no better than one reserved only in the most recent period. Being reserved in both 2005 and 2010 but not 2015 yields a cumulative effect of approximately zero ($0.02 + 0.00 - 0.02$). Being reserved in 2005 and 2015 but not 2010 yields approximately 5 percentage points ($0.02 + 0.05 - 0.02$), which is simply the 2015 effect. Double exposure adds nothing over single-period exposure.

The three-way interaction is large and significant (9 pp with FE, $p < 0.01$), and the total cumulative effect for always-reserved GPs sums to approximately 12 percentage points with FE. But this result is driven entirely by the three-way term overcoming the negative two-way interactions. The pattern is not one of gradual compounding, where two cycles of exposure have marginal value over one, but rather a discontinuous jump that appears only for the small group of GPs reserved in all three prior elections. This is difficult to interpret as a general dose-response effect and may instead reflect unobserved characteristics of always-reserved GPs.

Consistent with this concern, when we restrict to districts where quota assignment passes within-district independence tests for both the 2005→2010 and 2010→2015 transitions, the cumulative exposure estimates collapse entirely (Appendix Table E3). Quota_{2005}, Quota_{2010}, and their interaction are all zero to two decimal places in both Rajasthan ($N = 603$) and UP ($N = 1{,}772$). These restricted samples are small, but the complete absence of any signal, not even a directionally positive point estimate, is notable.

In sum, the cumulative exposure evidence does not support the prediction that sustained quota exposure generates compounding gains in women's representation. In Rajasthan, the cumulative effect is not significant. In UP, the full-sample result depends on a single three-way interaction that rescues otherwise null or negative two-way interactions, and the result disappears in the cleaner restricted sample.

Table 2: Cumulative Exposure Effects

	Rajasthan		Uttar Pradesh	
	No FE [i]	FE [ii]	No FE [iii]	FE [iv]
Intercept	0.09***		0.17***	
	(0.02)		(0.00)	
Quota_{2005}	-0.02	0.00	0.02	0.02*
	(0.04)	(0.03)	(0.01)	(0.01)
Quota_{2010}	0.05*	0.07***	0.00	0.00
	(0.03)	(0.02)	(0.01)	(0.01)
Quota_{2015}	0.02	0.04	0.04***	0.05***
	(0.03)	(0.02)	(0.01)	(0.01)
$\text{Quota}_{2005} \times \text{Quota}_{2010}$	0.04	0.02	-0.02	-0.02
	(0.05)	(0.04)	(0.02)	(0.02)
$\text{Quota}_{2005} \times \text{Quota}_{2015}$	0.06	0.03	-0.01	-0.02
	(0.04)	(0.04)	(0.02)	(0.02)
$\text{Quota}_{2010} \times \text{Quota}_{2015}$	-0.01	-0.04	0.00	-0.00
	(0.04)	(0.03)	(0.02)	(0.02)
$\text{Quota}_{2005} \times \text{Quota}_{2010} \times \text{Quota}_{2015}$	-0.05	-0.02	0.08***	0.09***
	(0.06)	(0.05)	(0.03)	(0.03)
R^2	0.00	0.15	0.00	0.06
Observations	2,632	2,632	14,139	14,139
(District, Samiti) FE		✓		✓

Notes: ***p<0.01; **p<0.05; *p<0.1. The dependent variable is whether a woman was elected in an open seat (2020 for Rajasthan, 2021 for UP). Sample restricted to GPs where seat was not reserved for women in outcome year. Treatment variables indicate quota status in each prior election year. Heteroskedasticity-robust standard errors.

5.3 Candidacy Effects

Quotas might affect electoral dynamics through candidacy rather than winning (O'Connell, 2020; Auerbach and Ziegfeld, 2016). Even if more women do not win once quotas are withdrawn, quotas might encourage more women to contest when the seat becomes open. We test this using candidate-level data from the 2020 Rajasthan elections, estimating the full interaction model on four outcomes: the proportion of candidates who are women, the number of women candidates, whether at least one woman ran, and women's vote share among the top two candidates.

Table H1 reports the results. Across all four outcomes, we find no statistically significant candidacy effects. Point estimates on the individual quota indicators are small and negative (1–3 percentage points for the proportion outcome), though some two-way interactions are marginally positive ($\text{Quota}_{2005} \times \text{Quota}_{2010}$: 5 percentage points, $p < 0.10$ without FE; $\text{Quota}_{2005} \times \text{Quota}_{2015}$: 5 percentage points, $p < 0.05$ without FE). These interaction effects do not survive the inclusion of fixed effects. On average, the top two candidates in the 2020 Rajasthan election capture 70% of total votes (70% in open seats), so the vote share analysis among the top two provides a meaningful test.

6 Robustness

Covariate adjustment. We re-estimate the short-run models on the SHRUG-matched subsample, controlling for log population, SC share, and literacy rate from the 2001 Census, aggregated to the GP level. Estimates are virtually unchanged (Appendix Table E1). The Rajasthan 2005→2010 coefficient remains zero; the 2010→2015 coefficient remains −3 percentage points ($p < 0.01$ without FE, $p < 0.05$ with FE). UP coefficients match the main specification: 1 percentage point for 2005→2010 ($p < 0.05$), 1 percentage point for 2010→2015 (not significant), and 4 percentage points for 2015→2021 ($p < 0.01$).

Random-rotation district restriction. We restrict to districts where within-district chi-squared tests fail to reject independence of consecutive quota assignments ($p > 0.05$). Table E2 reports the results. In Rajasthan, all 32 districts pass for the 2005→2010 transition (the coefficient is unchanged at 1 percentage point), 8 of 33 pass for 2010→2015, and 12 of 33 for 2015→2020. The restricted-sample estimates are positive (2 percentage points) but not significant, consistent with both no effect and a small positive effect attenuated by low power. In UP, 48 of 63 districts pass for 2005→2010, and the estimate remains 2 percentage points ($p < 0.01$). For 2010→2015, only 14 of 54 districts pass, and the estimate is zero. No UP districts pass for 2015→2021, so that panel cannot be estimated in this restricted sample. The cumulative exposure analog, restricted to districts where independence holds for both the 2005→2010 and 2010→2015 transitions, yields null effects in both states (Appendix Table E3): all coefficients and the interaction term are zero, though the samples are small (Rajasthan $N = 603$, UP $N = 1{,}772$).

Placebo tests. Table D1 confirms that future quota status has no predictive power for past outcomes: across all six panels and both FE specifications, every coefficient is zero to two decimal places and not significant.

Replication with independent data. We replicate the UP analyses using independently collected data from Weaver (2022), which covers 2010, 2015, and 2020 elections with substantially larger samples ($N \approx 37{,}000$–$39{,}000$). The short-run estimates (Table I2) are 2 percentage points for 2010→2015 ($p < 0.01$) and 4 percentage points for 2015→2020 ($p < 0.01$), matching our main UP results. The cumulative exposure interaction model (Table I3) shows a significant 2015 quota effect (4 percentage points, $p < 0.01$) with a zero interaction term, consistent with our finding that the most recent quota drives the result in UP.

7 Why Women Do Not Win or Run

The results establish that gender quotas have limited effects on women's chances of winning or contesting open seats after quota removal, and that cumulative exposure does not compound. We now turn to the institutional environment that explains these patterns.

7.1 Male Proxy Governance

The channels through which quotas are theorized to generate lasting representation all require that women elected under quotas actually exercise political authority. To measure who exercises day-to-day authority, we conducted a phone audit exploiting a feature of Indian local politics: elected representatives extensively use mobile phones for citizen engagement, a practice documented in media accounts (Rigillo, 2018) and confirmed in our interviews with political staff.[5] We scraped official electoral records from the Rajasthan State Election Commission containing the self-reported mobile phone numbers of all 2020 winners.[6] We block-sampled 500 Sarpanches from seats reserved for women across districts and contacted them between July and August 2024, with the average call lasting about one minute.

Panel A of Table 3 reports the results. Of 500 sampled, 377 (75%) answered. Of those who answered, only 35 (9.3%) were the elected representatives themselves. Male relatives answered in 319 cases (84.6%): spouses accounted for 173 (45.9%), sons for 46 (12.2%), and other males for 100 (26.5%). Six calls (1.6%) were answered by a female relative, and 17 (4.5%) could not be classified. When we requested to speak with the actual representative, 283 of 331 non-representative respondents (85.5%) refused to transfer the call. Reasons included the representative being unavailable, claims by the respondent that they were the "sarpanch pati" (husband of the sarpanch, i.e., the de facto representative), assertions that the representative was illiterate, or disconnection without explanation.

[5] Author's phone interview with former secretarial staff (name and affiliation withheld on request), December 20, 2024.

[6] The study protocol was reviewed by the IRB under protocol number UP-24-00498.

Two cases illustrate the pattern. In one, the elected representative initially answered but transferred the call to her spouse when we began asking about panchayat governance. In another, a father-in-law answered and insisted that he was the "Pratinidhi" (representative).

To benchmark these patterns, we contacted 507 randomly sampled representatives from open seats using the same protocol. The response rate was 79% (400 of 507). In open seats, 76.2% of answered calls reached the elected representative directly (Panel B of Table 3). A citizen calling a quota-seat representative speaks with the actual officeholder less than one in ten times; a citizen calling an open-seat representative does so three quarters of the time.

Among the 42 women in our open-seat sample who won without a gender quota, the pattern resembles quota seats rather than open seats overall: of 32 who answered, only 5 (16%) were the elected representatives; male relatives intercepted 26 calls, and 1 could not be classified. In three cases, male family members explicitly claimed to manage panchayat affairs on behalf of the female member. Male proxy governance thus appears to be a feature of female-held seats generally, not of quota seats specifically, though quotas massively increase the number of seats in which it operates.

If male relatives perform the representative function in the vast majority of quota seats, voters cannot observe female leadership, women in office cannot accumulate governing experience, and other potential female candidates observe family power rather than female political competence. The 2008 Panchayati Raj Ministry report finds that over 89% of women representatives did not contest another election after their term (Ministry of Panchayati Raj, 2008, pp. 26, 63), consistent with officeholders who never governed in practice having no basis on which to build a political career. These findings complement work by Cheema et al. (2023); Khan (2021); Prillaman (2023) and Amar et al. (2024) on male household members as constraints on women's political participation.

A concern is that gender gaps in mobile phone ownership, rather than proxy governance, drive the results. Rural phone ownership rates differ substantially by sex: 81% for men aged 15–49 versus 57% for women (Ministry of Statistics and Programme Implementation, 2025, Fig. 2.2,

Table 3: Phone Audit Response Distribution

Panel A: Gender-Quota Seats

Response Category	N (%)
Initial Contact (N = 500)	
Answered	377 (75.4)
No Answer	123 (24.6)
Among Answered (N = 377)	
Elected Representative	35 (9.3)
Male Relative	319 (84.6)
Spouse	173 (45.9)
Son	46 (12.2)
Other Male	100 (26.5)
Female Relative	6 (1.6)
Unknown/Blank	17 (4.5)
Call Transfer Requested (N = 331)	
Transferred to Member	3 (0.9)
Refused to Transfer	283 (85.5)

Panel B: Open Seats

Response Category	N (%)
Initial Contact (N = 507)	
Answered	400 (78.9)
No Answer	107 (21.1)
Among Answered (N = 400)	
Elected Representative	305 (76.2)
Male Relative	66 (16.5)
Female Relative	10 (2.5)
Unknown/Blank	16 (4)

Notes: Phone audit of elected Sarpanches in Rajasthan. We attempted to contact each representative up to three times between 10:00 am and 6:30 pm, conducting the conversation in Hindi. In addition to noting the initial point of contact, we asked respondents about their previous electoral experience, family involvement in politics, regularity of gram sabha meetings, and attendance at these meetings. The survey script is provided in Appendix Section G.1.

Panel A: Block-sampled 500 quota-seat winners across Rajasthan, contacted July–August 2024 (average call duration: one minute).

Panel B: Block-sampled 500 open-seat winners across Rajasthan, contacted August–December 2024.

p. 7). Two features of our setting mitigate this concern. First, the phone numbers we call are self-reported by candidates in officially sworn affidavits filed under Section 125A of India's Representation of the People Act, 1951, which prohibits false or concealed information under penalty of imprisonment (Government of India, 1951, p. 52). Phone ownership among political elites filing such affidavits likely exceeds population rates. Second, the contrast between quota seats (9% representative answers) and open seats (76% representative answers) is too large to be explained by differential phone ownership alone. What we cannot rule out is that women politicians are systematically less likely than men to answer calls from unknown numbers. Evidence on this specific behavior among political elites does not exist to our knowledge. Phone interception also captures only one dimension of proxy governance; male control likely extends to other spheres of political work, including gram sabha meetings, interactions with block officials, and disbursement decisions, that our audit cannot observe.

7.2 Candidate Characteristics

Proxy governance is not the only barrier. The composition of the candidate pool under quotas may itself weaken the exposure mechanism. If quota-elected women are less professionalized than open-seat winners on dimensions that voters associate with competence, even voters who do interact with the officeholder may not update positively (Hansen, 1997; Atkeson, 2003; Weeks and Baldez, 2015).

Table 4 compares observable characteristics of winners and candidates in quota versus open seats in the 2020 Rajasthan elections, using data from officially sworn candidate affidavits. Winners in open seats are 1.5 years older than winners in quota seats ($p < 0.01$), have 11 percentage points higher graduation rates (24% vs. 12%, $p < 0.01$), own more assets (log assets 12.92 vs. 12.55, $p < 0.01$), and are far less likely to report being unemployed (10% vs. 61%, $p < 0.01$). These patterns hold across the full candidate pool: all-candidate comparisons show nearly identical gaps. Similar patterns appear in UP (Table I1): open-seat winners are older, more educated, and (where available) wealthier than quota-seat winners, with the education gap narrowing slightly over time.

Table 4: Candidate Characteristics: Quota vs. Open Seats (Rajasthan 2020)

	Winners			All Candidates		
Variable	Open	Quota	Diff.	Open	Quota	Diff.
Age	42.69	41.16	1.54***	41.50	39.82	1.68***
Total Children	2.05	2.14	-0.09***	1.90	2.03	-0.13***
Graduation Status	0.24	0.12	0.11***	0.22	0.13	0.09***
Unemployed	0.10	0.61	-0.51***	0.11	0.61	-0.51***
Assets (log)	12.92	12.55	0.37***	12.33	12.08	0.26***

Notes: ***p<0.01; **p<0.05; *p<0.1. T-tests comparing characteristics in open vs quota seats. Data from Rajasthan 2020 panchayat elections. Assets winsorized at 10%.

The unemployment gap deserves a caveat: self-reported unemployment is shaped by gender norms, with men potentially less likely to classify themselves as unemployed even when not formally employed. Still, the composite picture across age, education, and assets points in the same direction: women elected under quotas are less professionalized on observable dimensions.

Among the 35 quota-elected representatives who answered our phone calls (out of 377), respondents are younger (mean age 37.4) and more likely to be employed than the broader quota-winner population (Table G1). This is consistent with women who actually exercise political authority being a positively selected subset of those who hold office under quotas.

These patterns align with the 2008 Panchayati Raj Ministry report (Ministry of Panchayati Raj, 2008, pp. 26, 63) and earlier studies (Chattopadhyay and Duflo, 2004; Ban and Rao, 2008; Raabe et al., 2009, Table VII). Gajwani and Zhang (2015, Tables 5–6) find comparable education levels but document knowledge disparities and less contact with higher-level officials among quota-elected women. The candidate characteristics we observe may also reflect the broader pool of women willing to contest under quotas, potentially leading to lower average qualifications as the pool expands (Bhavnani et al., 2024).

The two findings reinforce each other. Less professionalized candidates are more susceptible to proxy governance: a woman with less education, fewer assets, and less labor market experience is less likely to resist her husband or father-in-law taking over the representative function. And proxy governance prevents the officeholder from acquiring the experience that would professionalize her, trapping quota-seat women in a cycle where the preconditions for lasting change are never established.

7.3 Does Development Moderate the Quota Effect?

A natural explanation for the contrast between our findings and the large effects reported by Bhavnani (2009) in urban Mumbai is that development moderates the quota effect. In more urbanized, better-connected settings, the barriers we document may be weaker: voters are more likely to have encountered women in professional roles, the female candidate pool may be deeper and more professionalized, and household structures may be less patriarchal. If this account is correct, quota effects should increase along the development gradient within our sample.

We test this by interacting the quota treatment with three GP-level markers of development drawn from the 2001 Census Village Directory (via SHRUG): proximity to the nearest town (above

vs. below median distance), an infrastructure index (whether the sum of education, medical, power, and banking facilities exceeds the median), and female literacy rate (above vs. below median).

Table F1 reports the results for all six short-run panels. None of the 18 interaction terms (3 moderators × 6 panels) is significant at the 5% level. The few marginally significant coefficients do not exhibit a consistent pattern across panels or states. In Rajasthan, proximity to town is associated with lower female representation in 2015→2020 (−4 to −5 pp, $p < 0.05$), but this is a level effect on the outcome, not an interaction with treatment. The interactions themselves are uniformly small and insignificant. In UP, being near a town is associated with slightly lower baseline female representation in 2005→2010 (−3 pp, $p < 0.01$), but the quota-by-town interaction is again zero.

This null rules out one natural interpretation of the gap between our results and Bhavnani (2009): that quotas fail only in the most remote and patriarchal GPs and would succeed in better-resourced ones. Within the rural setting, more developed GPs show no greater gains from quota exposure than less developed ones. The gap with Bhavnani's Mumbai result may instead reflect a qualitative difference between urban and rural settings, in labor markets, household structures, or the density of female public-sphere participation, that is not captured by a within-rural development gradient. Combined with the proxy governance and candidate professionalization evidence, the pattern suggests that the barriers to women's post-quota political success are structural features of rural political households, not resource constraints that vary smoothly with infrastructure or connectivity.

8 Conclusion

Across four election cycles in Rajasthan and four in Uttar Pradesh, we find that reserving a GP seat for women in one election has limited effects on the probability that a woman wins or runs for that seat in subsequent open elections. Short-run effects range from zero to 4 percentage

points, precise enough to rule out the roughly 18 percentage point effects reported in urban Mumbai by Bhavnani (2009). Cumulative exposure over multiple cycles does not compound, and the one significant cumulative effect (in UP) does not survive restriction to districts with cleaner identification. Candidacy effects are null.

Our phone audit and candidate data point to why. In rural Rajasthan, only 9% of calls to quota-seat Sarpanches reached the elected representative; 85% were answered by male family members. Without direct citizen interaction with female leaders, the channels through which quotas are theorized to build lasting representation have nothing to work with. Women elected under quotas are also less professionalized than open-seat winners: younger, less educated, less wealthy, and far more likely to report being unemployed. These gaps compound the proxy governance problem, as less professionalized candidates are more susceptible to male relatives taking over the representative function.

We cannot adjudicate between all competing explanations for these results. Quotas may delegitimize women politicians, whom voters perceive as beneficiaries of a mandate rather than competitive candidates (Krook, 2006, p. 111). Taste-based discrimination may prevent women from winning open seats regardless of their performance (Desai et al., 2024). Our data are consistent with these accounts but do not isolate them from the proxy governance and candidate professionalization channels.

A suggestive piece of evidence on scope conditions comes from extending the phone audit to urban Jaipur, where 31.7% of calls to quota-seat representatives reached the elected officeholder, compared with 9.3% in rural areas (Table G2a). Proxy governance is less prevalent in urban settings, though it remains the norm even there. This gradient is consistent with the gap between our rural results and Bhavnani (2009)'s large urban effects, and suggests that the conditions under which quotas generate lasting representation are more likely to be met where household structures, labor markets, and women's public-sphere participation differ from rural India.

The policy implication is not that quotas are ineffective. Quotas guarantee descriptive representation, and compliance with India's mandate has placed nearly a million women in elected

local office. The implication is that quotas alone are insufficient to create the conditions for women to retain political power independently. The binding constraint is not the absence of a legal mandate but the persistence of household-level power structures that quotas do not reach. Complementary interventions that address proxy governance directly, whether through transparency requirements, training programs that build women's independent political networks, or enforcement mechanisms that ensure elected women exercise their authority, may be necessary for quotas to fulfill their transformative potential.

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Supplementary Information for "The Limits of Electoral Gender Quotas in Rural Local Bodies"

A Data and Research Design

Table A1: Summary of Gram Panchayats by Election Year

	2005	2010	2015	2020
Panel A: Rajasthan				
Gram Panchayats	9,178	9,166	9,862	11,314
Panchayat Samitis	237	249	300	352
Districts	32	33	33	33
Women Reserved (%)	33.5	47.6	48.4	48.6
Panel B: Uttar Pradesh				
Gram Panchayats	51,737	41,227	58,994	49,772
Panchayat Samitis	846	844	823	728
Districts	70	72	75	67
Women Reserved (%)	45.3	33.1	33.8	33.7

Note:
GP counts represent the total number of Gram Panchayats for which we have electoral data. The number of GPs increased between 2010 and 2020 due to delimitation (redistricting). The 2020 column for UP contains 2021 election data.

Table A2: Women's Representation: State vs Local Government

Level of Government	Total Representatives	Women	% Women
State Legislatures (1950–2024)			
All State Legislators	$\approx$ 40,000	$\approx$ 2,000	5.0
Local Bodies (Current)			
Gram Panchayats	$\approx$ 250,000	—	—
Total Elected Representatives	$>$ 2,000,000	$\approx$ 1,000,000	$\approx$ 50.0

Note:
State legislature data compiled from historical records of all state elections since 1950. Local body data from the Ministry of Panchayati Raj and Local Government Directory (2024). The contrast illustrates how gender quotas have dramatically increased women's descriptive representation at the local level compared to higher levels where quotas are not mandated.

B Identification

Our identification strategy relies on the rotation mandate: once a GP is reserved for women, it cannot be reserved again in the subsequent election. We present four types of evidence on identification: treatment rotation regressions, chi-squared tests of independence, balance tests on prior electoral characteristics, and balance tests on pre-treatment census covariates.

Table B1: Treatment Rotation: Quota Assignment Correlation Across Election Cycles (2-Way Panel)

	Rajasthan						Uttar Pradesh					
	05→10		10→15		15→20		05→10		10→15		15→21	
	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE
	[i]	[ii]	[iii]	[iv]	[v]	[vi]	[vii]	[viii]	[ix]	[x]	[xi]	[xii]
Intercept	0.47***		0.67***		0.62***		0.34***		0.38***		0.43***	
	(0.00)		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)	
Quota_{t-1}	0.01	0.01	-0.41***	-0.41***	-0.29***	-0.29***	-0.02***	-0.03***	-0.11***	-0.11***	-0.28***	-0.28***
	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.03)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
R^2	0.00	0.01	0.16	0.17	0.08	0.10	0.00	0.01	0.01	0.01	0.08	0.08
Observations	7,667	7,667	7,447	7,447	7,882	7,882	39,510	39,471	34,556	34,553	41,924	41,924
(District, Samiti) FE		✓		✓		✓		✓		✓		✓

Notes: ***p<0.01; **p<0.05; *p<0.1. Outcome: quota status in election t. Treatment: quota status in election $t-1$. Sample includes all GPs (not restricted to open seats). A negative coefficient indicates rotation away from previously reserved GPs, consistent with the rotation mandate. Heteroskedasticity-robust standard errors.

Table B2: Treatment Rotation: Quota Assignment Correlation Across Election Cycles (4-Way Panel)

	Rajasthan						Uttar Pradesh					
	05→10		10→15		15→20		05→10		10→15		15→21	
	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE
	[i]	[ii]	[iii]	[iv]	[v]	[vi]	[vii]	[viii]	[ix]	[x]	[xi]	[xii]
Intercept	0.46***		0.68***		0.64***		0.34***		0.38***		0.44***	
	(0.00)		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)	
Quota_{t-1}	0.02	0.02	-0.42***	-0.43***	-0.32***	-0.32***	-0.02***	-0.02**	-0.11***	-0.11***	-0.28***	-0.29***
	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.03)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
R^2	0.00	0.03	0.18	0.19	0.10	0.12	0.00	0.02	0.01	0.02	0.08	0.09
Observations	5,334	5,334	5,334	5,334	5,334	5,334	21,536	21,536	21,536	21,536	21,536	21,536
(District, Samiti) FE		✓		✓		✓		✓		✓		✓

Notes: ***p<0.01; **p<0.05; *p<0.1. Outcome: quota status in election t. Treatment: quota status in election $t-1$. Sample includes GPs present in all four election years (4-way panel). A negative coefficient indicates rotation away from previously reserved GPs, consistent with the rotation mandate. Heteroskedasticity-robust standard errors.

Tables B1 and B2 show autocorrelation in treatment assignment across consecutive elections. Table B1 uses 2-way panels (sample varies by transition); Table B2 restricts to GPs present in all

four election years (5,334 in Rajasthan; 21,536 in UP). In Rajasthan, the 2005-2010 coefficient is near zero ($\hat{\beta} = 0.01$), while 2010-2015 and 2015-2020 show strong negative coefficients ($\hat{\beta} = -0.42$ and -0.32, respectively). In UP, all transitions show negative coefficients ranging from -0.02 to -0.29. Negative coefficients indicate that GPs reserved in one election are less likely to be reserved in the next.

Tables B3 and B4 test independence of quota assignment across election pairs. In Rajasthan, the 2005-2010 transition shows no significant dependence ($\chi^2 = 1.80$, $p = 0.18$), but subsequent adjacent elections show strong dependence consistent with rotation (2010-2015: $\chi^2 = 946.85$, $p < 0.01$; 2015-2020: $\chi^2 = 536.04$, $p < 0.01$). Non-adjacent elections are independent (2005-2015: $\chi^2 = 0.08$, $p = 0.77$; 2005-2020: $\chi^2 = 0.15$, $p = 0.70$). In UP, adjacent elections show similar patterns, with 2005-2021 approaching independence ($\chi^2 = 2.51$, $p = 0.11$).

Table B3: Chi-Squared Tests for Treatment Rotation (Rajasthan)

Comparison	Chi-Squared	P-Value
2005-2010	1.80	0.18
2010-2015	946.85	0.00
2015-2020	536.04	0.00
2005-2015	0.08	0.77
2005-2020	0.15	0.70
2010-2020	111.30	0.00

Table B4: Chi-Squared Tests for Treatment Rotation (UP)

Comparison	Chi-Squared	P-Value
2005-2010	9.56	0.00
2010-2015	253.99	0.00
2015-2021	1741.14	0.00
2005-2015	429.88	0.00
2005-2021	2.51	0.11
2010-2021	57.72	0.00

Table B5 tests whether quota assignment is balanced on prior electoral characteristics across all election transitions. In Rajasthan, the 2005→2010 transition shows balance on prior female winners (37% vs 36%, $p = 0.51$), but later transitions show imbalance consistent with rotation—reserved GPs were less likely to have had prior female winners. In UP, reserved GPs had slightly

fewer prior female winners and differed on caste reservation status across all transitions. We control for these variables in our main specifications.

Table B5: Electoral Balance Tests for Quota Assignment Across Election Cycles

	Rajasthan									Uttar Pradesh								
	05→10			10→15			15→20			05→10			10→15			15→21		
Variable	Q	O	p	Q	O	p	Q	O	p	Q	O	p	Q	O	p	Q	O	p
Prior female winner	0.37	0.36	0.50	0.33	0.70	0.00	0.38	0.64	0.00	0.51	0.53	0.00	0.36	0.45	0.00	0.29	0.52	0.00
OBC reservation	0.15	0.15	0.39	0.17	0.14	0.00	0.16	0.13	0.00	0.21	0.30	0.00	0.26	0.29	0.00	0.17	0.32	0.00
SC/ST reservation	0.35	0.35	0.84	0.36	0.36	1.00	0.40	0.37	0.00	0.22	0.21	0.00	0.15	0.25	0.00	0.27	0.18	0.00
N	7,667			7,447			7,882			39,510			34,556			41,924		

Notes: Balance tests for quota assignment on prior electoral characteristics across all election transitions. Q = Quota (GPs reserved for women in the later year); O = Open (GPs not reserved). Prior female winner = proportion with female sarpanch/pradhan in prior election. p-values from t-tests. Full electoral panel (not SHRUG-filtered).

Tables B6 and B7 test balance on pre-treatment covariates from the 2001 Census. In Rajasthan, joint tests fail to reject balance (joint p-values: 0.30, 0.07, 0.21 across transitions). In UP, joint tests reject balance ($p < 0.01$), driven by population-related variables—reserved GPs tend to be smaller. We address this by including district-block fixed effects and controlling for GP-level characteristics from the 2001 Census via SHRUG.

Table B6: Rajasthan: Balance Tests for Quota Assignment

	2005→2010			2010→2015			2015→2020		
Variable	Quota	Open	p	Quota	Open	p	Quota	Open	p
Population	4081.54	4102.18	0.62	4055.26	4114.16	0.15	4099.52	4089.92	0.82
Female Share	0.48	0.48	0.18	0.48	0.48	0.65	0.48	0.48	0.20
SC Share	0.09	0.09	0.64	0.09	0.09	0.01	0.09	0.09	0.52
ST Share	0.07	0.07	0.76	0.07	0.07	0.51	0.07	0.07	0.87
Medical fac./1k	0.30	0.30	0.95	0.31	0.30	0.24	0.30	0.31	0.18
Mat. homes/1k	0.01	0.01	0.37	0.01	0.01	0.67	0.01	0.01	0.93
FW Centres/1k	0.01	0.01	0.04	0.01	0.01	0.41	0.01	0.01	0.34
Prim. sch./1k	1.11	1.10	0.63	1.09	1.12	0.08	1.12	1.10	0.08
Mid. sch./1k	0.39	0.38	0.06	0.38	0.38	0.74	0.38	0.38	0.86
Handpumps/1k	1.12	1.16	0.18	1.12	1.16	0.24	1.16	1.10	0.04
Tap water/1k	1.65	1.68	0.60	1.62	1.71	0.06	1.67	1.63	0.23
Wells/1k	1.09	1.12	0.27	1.09	1.11	0.33	1.10	1.07	0.21
Banking/1k	0.05	0.05	0.76	0.05	0.05	0.40	0.05	0.05	0.25
Power/1k	0.82	0.83	0.80	0.82	0.83	0.75	0.83	0.82	0.35
Mud road/1k	0.64	0.65	0.68	0.64	0.64	0.78	0.64	0.64	0.94
N	5189			4794			4700		
Joint p (LR)	0.30			0.07			0.21		
Joint p (RI)	0.30			0.07			0.21		

Notes: Balance tests for quota assignment across all Rajasthan panels. "Quota" = GPs reserved for women in base year; "Open" = GPs not reserved. Covariates from 2001 Census Village Directory (SHRUG). p-values from t-tests. Joint p (LR) from likelihood ratio test of logistic regression. Joint p (RI) from randomization inference with 1,000 permutations.

Table B7: Uttar Pradesh: Balance Tests for Quota Assignment

	2005→2010			2010→2015			2015→2021		
Variable	Quota	Open	p	Quota	Open	p	Quota	Open	p
Population	2269.56	2397.73	0.00	2329.09	2353.25	0.34	2786.09	2131.22	0.00
Female Share	0.48	0.47	0.00	0.47	0.47	0.17	0.47	0.47	0.32
SC Share	0.11	0.11	0.00	0.12	0.11	0.00	0.11	0.11	0.01
ST Share	0.00	0.00	0.48	0.00	0.00	0.03	0.00	0.00	0.93
Medical fac./1k	0.21	0.22	0.06	0.21	0.21	0.79	0.20	0.22	0.00
Mat. homes/1k	0.01	0.01	0.73	0.01	0.01	0.45	0.02	0.01	0.03
FW Centres/1k	0.07	0.07	0.05	0.07	0.07	0.62	0.07	0.07	0.48
Prim. sch./1k	0.72	0.73	0.37	0.74	0.74	0.91	0.68	0.76	0.00
Mid. sch./1k	0.16	0.16	0.77	0.16	0.16	0.47	0.16	0.16	0.25
Handpumps/1k	0.88	0.85	0.03	0.85	0.84	0.57	0.74	0.89	0.00
Tap water/1k	0.39	0.39	0.96	0.36	0.34	0.13	0.32	0.36	0.01
Wells/1k	0.64	0.64	0.73	0.61	0.62	0.61	0.57	0.66	0.00
Banking/1k	0.03	0.03	0.07	0.04	0.03	0.02	0.04	0.03	0.00
Power/1k	0.64	0.62	0.07	0.58	0.58	0.56	0.53	0.62	0.00
Mud road/1k	0.60	0.57	0.02	0.56	0.56	0.60	0.50	0.61	0.00
N	18488			14613			9841		
Joint p (LR)	0.00			0.00			0.00		
Joint p (RI)	0.00			0.00			0.00		

Notes: Balance tests for quota assignment across all Uttar Pradesh panels. "Quota" = GPs reserved for women in base year; "Open" = GPs not reserved. Covariates from 2001 Census Village Directory (SHRUG). p-values from t-tests. Joint p (LR) from likelihood ratio test of logistic regression. Joint p (RI) from randomization inference with 1,000 permutations.

C Power Analysis

To interpret our null results, we calculate minimum detectable effects (MDE) for each specification. The MDE represents the smallest true effect we could detect with 80% power at $\alpha = 0.05$. Table C1 shows that our MDEs range from 1.7 to 4.0 percentage points across specifications. For comparison, Bhavnani (2009) found a 15 percentage point effect in urban Mumbai – an effect size we would detect with >99% power. Our null results are therefore not driven by lack of statistical power; rather, they reflect genuinely small or absent effects in the rural, non-partisan context we study.

Table C1: Power Analysis: Minimum Detectable Effects for Quota Effects

State	Period	Type	N	Control Mean	Coef (pp)	SE	95% CI	MDE (pp)	MDE (% of ctrl)
Rajasthan	2005-2010	Short-term	4,062	9.2%	1.44	(1.00)	[-0.52, 3.39]	2.79	30%
Rajasthan	2010-2015	Short-term	3,867	8.8%	-2.81	(1.02)	[-4.82, -0.81]	2.86	32%
Rajasthan	2015-2020	Short-term	3,962	12.1%	0.49	(1.17)	[-1.80, 2.78]	3.27	27%
Rajasthan	2005-2020 (LT)	Long-term	2,630	11.2%	3.08	(1.41)	[0.31, 5.85]	3.96	35%
UP	2005-2010	Short-term	14,469	14.8%	1.08	(0.61)	[-0.12, 2.27]	1.71	12%
UP	2010-2015	Short-term	14,194	16.0%	1.67	(0.66)	[0.37, 2.96]	1.85	12%
UP	2015-2021	Short-term	14,132	17.4%	4.43	(0.67)	[3.12, 5.75]	1.88	11%
UP	2005-2021 (LT)	Long-term	14,132	19.2%	0.36	(0.68)	[-0.98, 1.70]	1.91	10%

MDE calculated for 80% power at $\alpha = 0.05$. Control Mean is the proportion of women winning in open seats among GPs not previously reserved. Coefficient and CI are in percentage points. Short-term panels test one-election lag effects; long-term (LT) panels test 2005 treatment effect on 2020/2021 outcomes. MDE (% of ctrl) expresses the minimum detectable effect as a percentage increase relative to the control mean. For comparison, Bhavnani (2009) found a 15 percentage point effect in urban Mumbai.

D Placebo and Falsification Tests

To verify random assignment, we conduct temporal falsification tests. If quota assignment is truly exogenous, future treatment should not predict past outcomes. Table D1 shows that 2010 treatment has no predictive power for 2005 outcomes in Rajasthan ($p > 0.5$), and 2015 treatment does not predict 2010 outcomes ($p > 0.5$). Similar patterns hold for UP.

Separately, we validate our main treatment effect using randomization inference with 1,000 permutations. Comparing the observed coefficient to the permuted null distribution, we find the effect is distinguishable from random chance in both Rajasthan ($\hat{\beta} = 0.02$, $p = 0.07$) and UP ($\hat{\beta} = 0.02$, $p < 0.01$).

Table D1: Placebo Tests: Future Treatment Predicting Past Outcomes

	Rajasthan						Uttar Pradesh					
	05→10		10→15		15→20		05→10		10→15		15→21	
	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE
	[i]	[ii]	[iii]	[iv]	[v]	[vi]	[vii]	[viii]	[ix]	[x]	[xi]	[xii]
Intercept	0.04***		0.11***		0.07***		0.12***		0.15***		0.17***	
	(0.00)		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)	
Quota_t	-0.00	-0.00	-0.01	-0.01	0.01	0.02*	-0.00	-0.00	-0.00	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
R^2	0.00	0.06	0.00	0.11	0.00	0.15	0.00	0.07	0.00	0.06	0.00	0.05
Observations	5,101	5,101	3,919	3,919	4,061	4,060	20,922	20,897	23,164	23,160	27,859	27,859
(District, Samiti) FE		✓		✓		✓		✓		✓		✓

Notes: ***p<0.01; **p<0.05; *p<0.1. Placebo test: FUTURE quota status predicting PAST outcomes. Outcome: woman elected in open seat at $t-1$. Treatment: quota status at t. Sample restricted to GPs where seat was open in the outcome election. Null effects support random assignment of quota. Heteroskedasticity-robust standard errors.

E Robustness Checks

E.1 Alternative Specifications

We assess robustness to alternative model specifications including: (1) no fixed effects with robust standard errors, (2) district-samiti fixed effects, (3) district fixed effects only, (4) GP-level clustering, and (5) district-level clustering. Coefficient estimates remain stable (approximately 2 percentage points) across all specifications in both states.

Table E1: Short-Term Effects with Census Covariates

	Rajasthan						Uttar Pradesh					
	05→10		10→15		15→20		05→10		10→15		15→21	
	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE
	[i]	[ii]	[iii]	[iv]	[v]	[vi]	[vii]	[viii]	[ix]	[x]	[xi]	[xii]
Intercept	0.75***		-0.00		1.2***		0.32***		0.37***		0.38***	
	(0.21)		(0.19)		(0.25)		(0.09)		(0.11)		(0.14)	
Quota_{t-1}	0.02	0.01	-0.02	-0.03**	-0.00	-0.00	0.00	0.00	0.01*	0.01*	0.04***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Log(Population)	-0.03*	-0.01	0.02	0.02	-0.03	-0.00	0.00	0.00	-0.00	0.00	0.00	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
SC Share	-0.05	-0.07	-0.12**	-0.03	-0.11	-0.13	-0.04*	-0.05*	-0.11***	-0.15***	-0.07*	-0.10**
	(0.06)	(0.08)	(0.05)	(0.06)	(0.07)	(0.10)	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Literacy Share	-0.12**	-0.15*	-0.04	-0.04	-0.15**	-0.25**	-0.15***	-0.03	-0.28***	-0.00	-0.17***	-0.08
	(0.05)	(0.08)	(0.05)	(0.08)	(0.07)	(0.11)	(0.03)	(0.04)	(0.04)	(0.06)	(0.04)	(0.07)
Female Share	-0.70**	0.39	-0.06	-0.08	-1.6***	0.36	-0.26*	-0.44*	-0.10	-0.70**	-0.23	0.02
	(0.31)	(0.52)	(0.28)	(0.34)	(0.37)	(0.65)	(0.14)	(0.23)	(0.17)	(0.28)	(0.22)	(0.33)
Infrastructure Index	-0.00	-0.00	-0.00**	-0.00	-0.00	0.00	-0.00	-0.00	0.00*	0.00*	-0.00**	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
R^2	0.00	0.13	0.00	0.20	0.01	0.16	0.00	0.06	0.00	0.06	0.00	0.07
Observations	2,788	2,788	2,454	2,454	2,324	2,324	12,405	12,405	9,636	9,636	6,484	6,484
(District, Samiti) FE		✓		✓		✓		✓		✓		✓

Notes: ***p<0.01; **p<0.05; *p<0.1. Outcome: woman elected in open seat. Census covariates from 2001 Census via SHRUG (LGD Block Panchayat matching). Population, SC share, literacy, and female share computed at GP level. Infrastructure index is the sum of education, medical, power, and banking facilities. Sample restricted to GPs with SHRUG match and non-missing covariates. Heteroskedasticity-robust standard errors.

Table E2: Short-Term Effects: Random Rotation Districts

	Rajasthan						Uttar Pradesh					
	05→10		10→15		15→20		05→10		10→15		15→21	
	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE
	[i]	[ii]	[iii]	[iv]	[v]	[vi]	[vii]	[viii]	[ix]	[x]	[xi]	[xii]
$Quota_{t-1}$	0.01	0.01	0.02	0.02	0.02	0.01	0.02***	0.02***	-0.00	-0.00	—	—
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.00)	(0.01)	(0.01)	(0.01)		
R^2	0.00	0.10	0.00	0.08	0.00	0.15	0.00	0.06	0.00	0.04	—	—
Observations	4,062	4,062	777	777	1,303	1,303	20,152	20,152	4,050	4,050	—	—
(District, Samiti) FE		✓		✓		✓		✓		✓		

Notes: ***p<0.01; **p<0.05; *p<0.1. Outcome: woman elected in open seat. Sample restricted to districts where chi-square test fails to reject independence of quota assignment across periods ($p > 0.05$). Districts retained: Raj 05→10: 32/32; 10→15: 8/33; 15→20: 12/33; UP 05→10: 48/63; 10→15: 14/54; 15→21: 0/61. Heteroskedasticity-robust standard errors.

Table E3: Long-Term Effects: Random Rotation Districts (2005–2015)

	Rajasthan		Uttar Pradesh	
	05→15		05→15	
	No FE	FE	No FE	FE
	[i]	[ii]	[iii]	[iv]
$Quota_{2005}$	-0.00	0.00	0.01	0.00
	(0.02)	(0.02)	(0.02)	(0.02)
$Quota_{2010}$	0.00	0.01	0.00	-0.00
	(0.02)	(0.02)	(0.03)	(0.03)
$Quota_{2005} \times Quota_{2010}$	-0.01	-0.02	-0.00	-0.00
	(0.03)	(0.03)	(0.04)	(0.04)
R^2	0.00	0.08	0.00	0.06
Observations	603	603	1,772	1,772
(District, Samiti) FE		✓		✓

Notes: ***p<0.01; **p<0.05; *p<0.1. Outcome: woman elected in open seat (2015). Sample restricted to districts where chi-square test fails to reject independence of quota assignment for BOTH 05→10 AND 10→15 transitions ($p > 0.05$). Districts retained: Raj: 9/32; UP: 10/44. Heteroskedasticity-robust standard errors.

F Heterogeneous Effects

We examine whether quota effects vary by village characteristics using data from the 2001 Census matched to our electoral panels via SHRUG. Specifically, we test for heterogeneity along three dimensions: proximity to towns, infrastructure availability, and female literacy. We include all three interactions simultaneously in a single model, allowing us to estimate the independent contribution of each moderator while controlling for the others.

Table F1 presents heterogeneous effects by village characteristics from Census 2001. The three moderators are: (1) Near Town, a binary indicator for villages within the median distance to the nearest town; (2) High Infrastructure, indicating above-median sum of education facility, medical facility, power supply, and bank facility indicators; and (3) High Female Literacy, indicating above-median female literacy rate (literate females aged 7+ divided by total females aged 7+).

Across all specifications, we find no consistent evidence that quota effects vary meaningfully by these village characteristics. Point estimates for interaction terms are generally small and statistically insignificant, suggesting that the null effects documented in our main analysis are not masking heterogeneous treatment effects across different types of villages.

Table F1: Heterogeneous Effects by Village Characteristics

	Rajasthan						Uttar Pradesh					
	05→10		10→15		15→20		05→10		10→15		15→21	
	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE	No FE	FE
	[i]	[ii]	[iii]	[iv]	[v]	[vi]	[vii]	[viii]	[ix]	[x]	[xi]	[xii]
Intercept	0.09***		0.08***		0.15***		0.16***		0.19***		0.16***	
	(0.01)		(0.02)		(0.02)		(0.01)		(0.01)		(0.02)	
Quota_{t-1}	0.04	0.02	-0.03	-0.03	0.00	-0.00	-0.00	-0.00	0.02	0.01	0.06**	0.07**
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Near Town	-0.02	-0.02	0.00	0.00	-0.05**	-0.07***	-0.02*	-0.01	0.01	0.00	0.00	0.00
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)
High Infrastructure	0.03*	0.02	0.01	0.02	0.02	0.01	0.00	0.00	-0.01	-0.02*	0.03	0.02
	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
High Female Literacy	-0.01	-0.01	-0.02	-0.01	-0.04	-0.05*	-0.04***	-0.02*	-0.05***	0.00	-0.03***	-0.00
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
$\text{Quota}_{t-1}\times$ Near Town	-0.02	-0.00	0.00	-0.00	0.05*	0.05	0.02	0.02	-0.02	-0.02	-0.01	-0.02
	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
$\text{Quota}_{t-1}\times$ High Infra	-0.03	-0.03	0.01	0.00	-0.06*	-0.06*	0.00	0.00	0.02	0.01	-0.01	-0.02
	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
$\text{Quota}_{t-1}\times$ High F. Literacy	0.03	0.04*	-0.00	0.00	0.02	0.04	0.01	0.00	-0.02	-0.01	-0.02	-0.02
	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
R^2	0.00	0.13	0.00	0.20	0.00	0.17	0.00	0.06	0.00	0.06	0.00	0.07
Observations	2,784	2,784	2,449	2,449	2,322	2,322	12,399	12,399	9,635	9,635	6,478	6,478
(District, Samiti) FE		✓		✓		✓		✓		✓		✓

Notes: ***p<0.01; **p<0.05; *p<0.1. Outcome: woman elected in open seat. Near Town = 1 if distance to nearest town ≤ median. High Infrastructure = 1 if sum of (education facility, medical facility, power supply, bank facility) > median. High Female Literacy = 1 if female literacy rate > median (females aged 7+ who are literate / total females aged 7+). All variables from Census 2001 Village Directory via SHRUG (LGD Block Panchayat matching). Heteroskedasticity-robust standard errors.

G Phone Survey Details

G.1 Survey Script

We used the following script for our survey, conducted by a native Hindi speaker in India. The interviewer documented the date, time, and duration of each call. To monitor data quality, interviewers provided screenshots of call history, which we randomly sampled and matched against spreadsheet entries with perfect accuracy.

> Hello, I am [RA name] talking on behalf of researchers based in the United States who want to study the panchayat system in Rajasthan. Am I talking to [representative name]?
>
> *Notes to RA: If the name does not match, ask them to transfer the call to the actual representative. If they refuse, thank them and end the conversation, recording their reason.*
>
> If the actual representative answers: We obtained your phone number from the Rajasthan Election Commission's website. We want to understand governance in panchayats in Rajasthan. We have a few questions about your experience as an elected representative; this will take 2-3 minutes. We will not share any personal information and are not recording this call, but will take notes. Would you be willing to talk to us?

If the respondent consents, proceed to questions:

1. Have you contested in elections before?
2. Has anybody from your family contested in elections before?
3. Are the Panchayat meetings held regularly?
4. Do you attend meetings regularly?

G.2 Survey Results

Table G1: Characteristics of Representatives Who Answered Phone Calls

Variable	Mean
Age	37.40
Total Children	1.74
Graduation Status	0.34
Unemployed	0.54
Assets (log)	12.51

Notes: Characteristics of representatives in quota seats who answered our phone calls. N = 35.

Table G2: Urban Phone Survey Response Distribution (Jaipur)

(a) Gender-Quota Seats

Response Category	N (%)
Initial Contact (N = 78)	
Answered	63 (80.8)
No Answer	15 (19.2)
Among Answered (N = 63)	
Elected Representative	20 (31.7)
Non-Member	41 (65.1)
Male	39 (95.1)
Female	2 (4.9)
Unknown	2 (3.2)

(b) Open Seats

Response Category	N (%)
Initial Contact (N = 170)	
Answered	146 (85.9)
No Answer	24 (14.1)
Among Answered (N = 146)	
Elected Representative	129 (88.4)
Spouse/Relative	7 (4.8)
Other Non-Member	7 (4.8)
Unknown	3 (2.1)

Notes: We contacted each representative up to three times between 10:00 am and 6:30 pm (average duration: one minute). *Panel A:* All gender-quota seats in urban Jaipur, June–August 2024. Of 78 representatives, 63 (81%) answered; only 20 (32%) were the elected representatives themselves. Among 41 non-member respondents, 39 (95%) were male. *Panel B:* All open seats in urban Jaipur, June–August 2024.

H Additional Electoral Outcomes

Table H1 examines whether past quota exposure affects women's candidacy in open seats. We restrict to GPs where the sarpanch seat was not reserved for women in 2020 and test whether prior quota assignments (2005, 2010, 2015) predict female candidacy. We find no evidence that past quotas increase the proportion of women candidates, the number of women running, or women's vote share. Most coefficients are statistically indistinguishable from zero.

Table H1: Candidacy Effects in Open Seats (2020, Rajasthan)

	Prop. Women		Num. Women		≥1 Woman		Vote Share	
	[i]	[ii]	[iii]	[iv]	[v]	[vi]	[vii]	[viii]
Constant	0.12***		0.90***		0.45***		0.07***	
	(0.02)		(0.10)		(0.04)		(0.02)	
$Quota_{2005}$	0.00	0.04	-0.02	0.20	-0.07	-0.02	0.02	0.04
	(0.03)	(0.03)	(0.18)	(0.21)	(0.07)	(0.07)	(0.03)	(0.03)
$Quota_{2010}$	-0.00	0.02	-0.12	0.04	0.00	0.06	0.01	0.03**
	(0.02)	(0.02)	(0.12)	(0.13)	(0.05)	(0.05)	(0.02)	(0.02)
$Quota_{2015}$	-0.01	0.02	-0.24**	-0.01	-0.04	0.03	0.00	0.04**
	(0.02)	(0.02)	(0.11)	(0.14)	(0.05)	(0.05)	(0.02)	(0.02)
$Quota_{2005} \times Quota_{2010}$	0.02	-0.01	0.01	-0.21	0.04	-0.02	0.00	-0.01
	(0.04)	(0.04)	(0.22)	(0.24)	(0.09)	(0.09)	(0.04)	(0.04)
$Quota_{2005} \times Quota_{2015}$	0.03	-0.00	0.22	-0.06	0.16**	0.07	0.01	-0.01
	(0.03)	(0.04)	(0.20)	(0.25)	(0.08)	(0.08)	(0.03)	(0.03)
$Quota_{2010} \times Quota_{2015}$	0.01	-0.03	0.14	-0.08	-0.00	-0.08	-0.00	-0.04
	(0.03)	(0.03)	(0.16)	(0.18)	(0.07)	(0.07)	(0.03)	(0.03)
$Quota_{2005} \times Quota_{2010} \times Quota_{2015}$	-0.06	-0.02	-0.20	0.07	-0.13	-0.05	-0.03	-0.00
	(0.05)	(0.04)	(0.28)	(0.29)	(0.12)	(0.10)	(0.05)	(0.04)
R^2	0.00	0.24	0.00	0.21	0.00	0.18	0.00	0.19
Observations	1,702	1,702	1,702	1,702	1,702	1,702	1,702	1,702
(District, Samiti) FE		✓		✓		✓		✓

Notes: ***p<0.01; **p<0.05; *p<0.1. The sample is restricted to GPs where the seat was open (not reserved for women) in 2020. Columns [i]-[ii]: proportion of women among all candidates; Columns [iii]-[iv]: number of women candidates; Columns [v]-[vi]: whether at least one woman ran; Columns [vii]-[viii]: women's vote share among winner and runner-up. Odd-numbered columns show estimates without fixed effects; even-numbered columns include (District, Samiti) FE. Heteroskedasticity-robust standard errors.

I Weaver Data Replication (Uttar Pradesh 2010–2020)

We replicate our main analyses using the Weaver dataset, covering 61,338 Gram Panchayats in Uttar Pradesh across the 2010, 2015, and 2020 elections. This dataset includes reservation status, winner characteristics (gender, age, education, assets), turnout, and candidate pool composition.

Table I1: Winner Characteristics in Uttar Pradesh

	2010			2015			2020		
Variable	Open	Quota	Diff.	Open	Quota	Diff.	Open	Quota	Diff.
Age	41.59	40.20	1.40***	41.46	40.31	1.15***	42.24	40.94	1.30***
Education	2.87	1.67	1.20***	2.67	1.71	0.96***	2.88	2.05	0.84***
Assets (asinh)	–	–	–	–	–	–	13.00	12.88	0.12***

Notes: ***p<0.01; **p<0.05; *p<0.1. Data from Weaver (2022). T-tests comparing winner characteristics in open vs quota seats. Education is ordinal (1=Primary to 9=Doctorate). Assets (asinh transformation) available only for 2020.

Table I2: Short-Term Effects: Weaver UP Replication

	Woman Elected			
	2015	2015 (FE)	2020	2020 (FE)
	[i]	[ii]	[iii]	[iv]
Constant	0.16***		0.18***	
	(0.00)		(0.00)	
Quota_{t-1}	0.02***	0.02***	0.04***	0.04***
	(0.00)	(0.00)	(0.00)	(0.00)
R^2	0.00	0.00	0.00	0.06
Observations	37,484	37,484	39,222	39,222
(District, Samiti) FE		✓		✓

Notes: ***p<0.01; **p<0.05; *p<0.1. The dependent variable is whether a woman was elected in an open seat in the subsequent election. Columns [i]-[ii] show effects of 2010 quota on 2015 outcomes; columns [iii]-[iv] show effects of 2015 quota on 2020 outcomes. Models [i] and [iii] use heteroskedasticity-robust SE. Models [ii] and [iv] add (District, Samiti) FE.

Table I3: Long-Term Effects: Weaver UP Replication

	Woman Elected	
	No FE	FE
	[i]	[ii]
Constant	0.18***	
	(0.00)	
Quota_{2010}	0.00	0.00
	(0.00)	(0.00)
Quota_{2015}	0.04***	0.04***
	(0.00)	(0.00)
$\text{Quota}_{2010} \times \text{Quota}_{2015}$	0.00	0.00
	(0.00)	(0.00)
R^2	0.00	0.06
Observations	36,925	36,925
(District, Samiti) FE		✓

Notes: ***p<0.01; **p<0.05; *p<0.1. The dependent variable is whether a woman was elected in an open seat in 2020. All models include 2010 quota, 2015 quota, and their interaction. Model [i] uses heteroskedasticity-robust SE. Model [ii] adds (District, Samiti) FE.