New Evidence on the Effects of Randomly Assigned Reservations for Women Leaders in Indian Local Government*

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PRELIMINARY VERSION – NOT FOR QUOTATION

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Abstract

Influential early studies of reservations for women in India's local government bodies, like the Gram Panchayat, showed them to have important policy consequences. Outcomes such as local budget allocations seemed to shift as women suddenly entered leadership roles. Specifically, Gram Panchayats, where the role of the council president was reserved for women, tended to allocate more resources to areas that women are thought to be especially concerned about, such as drinking water and maternal care. This paper revisits the ongoing naturally occurring experiment of reservations for women in India's Gram Panchayats, focusing on Rajasthan, where reservations have been assigned by lottery (in most districts) since the 1990s. Our dataset, which examines the effect of women's reservations from 2005 on, consists of 4,351 Gram Panchayats, which is more than 50 times larger than the original study of Rajasthan.

Using administrative data for outcomes, including some of the same public works spending measures used by the authors of the original study, we find no evidence of substantively meaningful effects on the scale or content of public works programs, even in domains such as drinking water, sanitation, and maternal care. Censuses of local health, infrastructure, and schooling also show no apparent effects.

We find no evidence of effects on budget allocations or Census outcomes even when comparing Gram Panchayats that were reserved for women for four consecutive 5-year terms starting in 2005 to their counterparts that were never reserved for women during this period. Nor do we find effects when examining public works allocations in a different state, Uttar Pradesh. We conclude by discussing possible explanations for this pattern of null findings.

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1 Introduction

The path-breaking work by Chattopadhyay and Duflo (2004) was the first major research effort to assess the vast policy experiment that occurred when India adopted a constitutional amendment that both restructured rural local government and instituted reservations for women. As Chattopadhyay and Duflo (2004) explain:

> In 1992, the 73rd amendment to the Constitution of India established throughout India the framework of a three-tiered Panchayat system with regular elections. It gave the GP [Gram Panchayat] primary responsibility in implementing development programs, as well as in identifying the needs of the villages under its jurisdiction... The major responsibilities of the GP are to administer local infrastructure (public buildings, water, roads) and identify targeted welfare recipients. The main source of financing is still the state, but most of the money which was previously earmarked for specific uses is now allocated through four broad schemes: The Jawhar Rozgar Yojana (JRY) for infrastructure (irrigation, drinking water, roads, repairs of community buildings, etc.); a small additional drinking water scheme; funds for welfare programs (widow's, old age, and maternity pensions, etc.); and a grant for GP functioning. The GP has, in principle, complete flexibility in allocating these funds...[The 73rd Amendment provided that one-third of the seats in all Panchayat councils, as well as one-third of the Pradhan [council president] positions, must be reserved for women. (p.1412)

By comparing local governing bodies where women were or were not randomly allocated leadership positions, Chattopadhyay and Duflo (2004) effectively launched four related research programs. The first is whether the policies of local councils differ depending on whether the leadership role is reserved for women. This line of research follows naturally from the fact that the 73rd Amendment introduced a decentralization program that assigned Gram Panchayats (GPs) two main responsibilities: managing public infrastructure (e.g., roads, clinics, irrigation systems) and deeming villagers eligible for welfare schemes (e.g., ration cards, public works programs) (Cassan and Vandewalle, 2021, p.2).

The second research program investigates whether and how policy priorities differ across male and female Sarpanchs (council presidents). This measurement effort uses a combination of leader surveys, voter surveys, and local complaint registries to assess gender differences in the priority that men and women accord to domains such as drinking water or roads. This research agenda complements the study of policy outputs by discovering which policy outcomes might be affected by reserving the position of Sarpanch for women. The third strand considers the social-psychological effects of elevating women to local leadership positions on women's political participation in local meetings (Priebe, 2017), attitudes of men and women concerning gender equality (Dasgupta, 2022), and the aspirations of girls

(Beaman et al., 2012). The fourth program examines whether the exogenous increase in the supply of experienced local elected officials led to more women running for office and winning seats in state and national elections, which do not reserve seats for women (O'Connell, 2020, Karekurve-Ramachandra, 2020).[5]

Our focus in this paper is the first of these research programs, which remains relatively small and unsettled. Based on their analysis of 322 villages (in 161 GPs) in West Bengal and 100 villages (in 100 GPs) in Rajasthan, Chattopadhyay and Duflo (2004) find that GPs randomly headed by women had significantly more drinking water facilities and significantly fewer road-related projects (p.1432). When contributors to this literature refer to the impact of women leaders on public goods or policy outcomes, they are referring to indications that local councils pursue different goals or exert different levels of administrative effort in certain domains, such as water, sanitation, or maternal health.

The question of whether such policy effects manifest in the wake of reservations is linked to the question of why they occur. Chattopadhyay and Duflo (2004) trace these policy outputs to the distinctive policy demands of women. Using a combination of survey data and records of constituents' complaints to local presidents, Chattopadhyay and Duflo (2004) argue that women and men have different priorities (Table 4, p.1430). In Rajasthan, for example, female villagers accord higher priority to welfare programs, childcare, and drinking water than men; men place a much higher priority on public works. The measurement of women's and men's opinions and complaints adds an important nuance to Chattopadhyay and Duflo (2004)'s argument:

> Controlling for the average preference of women in the district...there are not more investments in goods about which women have complained more often in a specific village in GPs reserved to women...There are also not more investments in goods about which both men and women have complained more often in GPs that are headed by women...Individual women are thus not particularly more responsive to the needs of women and men in their communities. Rather, it is because their own preferences are more aligned to the preferences of women that they end up serving them better. (p. 1434)

In essence, the authors argue that women representatives are not more responsive to their constituents' opinions in general, nor are they especially attentive to the complaints brought by women constituents; rather, the policy effects are driven by the very fact that women representatives themselves share the priorities typical of women.[6]

How has the conclusion about the policy consequences of women's reservations in rural India held up in the two decades since the publication of this landmark study? The

[5] See also Bhavnani (2009) who analyzes the effect of reserving seats for women in an urban area (Mumbai) on the probability of winning future open-seat elections.

[6] The authors also address competing explanations, such as the fact that women presidents are more likely to be lame ducks than their male counterparts.

literature, much of which is unpublished, can be divided into two categories. The first category includes studies that look at the allocative decisions by local governments. These outcomes include, for example, counts of "the number of drinking water facilities newly built or repaired." Some studies field surveys (or focus groups) of villagers to assess infrastructure (Ban and Rao, 2008, Bardhan et al., 2010, Deininger et al., 2020, Gajwani and Zhang, 2008, Priebe, 2017, Sathe et al., 2013, Raabe et al., 2009); others conduct their own assessment of infrastructure (Cassan and Vandewalle, 2021, Pandey et al., 2023); others survey GP government staff (Rajaraman and Gupta, 2008); and others use official statewide infrastructure inventories or surveys (Bose and Das, 2018, Nilekani, 2010) or a combination of the two (Deininger et al., 2020, Duflo et al., 2004).[7] A second line of research looks not at direct administrative actions, such as building or maintaining clinics, but at their consequences, such as lower rates of maternal mortality, increased female labor force participation, or improved outcomes for the poor.[8]

Both strands of literature are deeply divided on whether reservations for women affect local outcomes. Those skeptical of Chattopadhyay and Duflo (2004)'s conclusions include Ban and Rao (2008), who report the results of a 2002 study of 106 GPs in four southern states. Although they find that women villagers accord higher priority to drinking water and sanitation than men, they find no effects on infrastructure in GPs reserved for women, even drinking water infrastructure. Gajwani and Zhang (2008)'s study of 144 GPs in Tamil Nadu finds no effect on public goods provision, except for schools, which receive more attention in male-headed GPs, consistent with male villagers' preferences. Nilekani (2010)'s analysis of reservations in Karnataka finds "very weak effects of reservation on the available governance indicators. The difference of means between the two groups on any available indicator of government performance was statistically significant for just six t-tests out of 286." (p. 36) Priebe (2017) reports results from a 2008 survey of Maharashtra in 32 GPs. Although the data suggest that women villagers became significantly more active in local governance, objective and subjective measures of policy outcomes were no better in reserved GPs, except for more bus stops. Another study of Karnataka conducted by Raabe et al. (2009) examined 80 GPs but found "weak support to the existence of gender effects of reservation policies on local governance and rural service provision." (p.

[7] There seems to be a lively debate about the biases associated with surveys versus government assessments. The former is criticized on the grounds that respondents give socially desirable answers that may also be colored by their own views about having a woman in charge; the latter is criticized on the grounds that middlemen and fraud distort the number of beneficiaries. The electronic system of reporting Mahatma Gandhi National Rural Employment Guarantee Act (MNREGA) expenditures appears to have improved accuracy and reduced corruption (Banerjee et al., 2020).

[8] A third line of research focuses on the extent to which women office-holders are less prone to corruption, as measured by surveys of villagers that ask about paying bribes in return for services. Duflo et al. (2004) and Deininger et al. (2020) report significant declines in reported bribes. Another literature compares the background of women leaders in reserved GPs to leaders in unreserved GPs, pointing out that, after the adoption of reservations, the former had less education and office-holding experience (Ban and Rao, 2008, Chattopadhyay and Duflo, 2004).

vii). Rajaraman and Gupta (2008) analyze a 2005-2006 survey of 780 GPs in four states, focusing on "receipt and utilization of funding from the SGRY," a national program for rural employment and infrastructure. Reservations, however, had no apparent effect on water-related expenditures or total expenditure, although they may have increased allocations to building maintenance projects. Sathe et al. (2013) report a much smaller study focusing on 32 villages in Maharashtra in 2008 and found no overall effect on service delivery. Somewhat more encouraging are the results reported in Bose and Das (2018), showing an increase in MNREGA public works programs concerning water (p.210), although many MNREGA projects are only weakly affected by reservations for women (p.214). More encouraging still is the fact that MNREGA job cards appear to go up in the wake of reservations for women, judging from Deininger et al. (2020)'s analysis of a 2014-2015 survey of 163 villages in 12 states and accompanying administrative data. Cassan and Vandewalle (2021), studying a nationally representative survey of rural adults from 2005, find that the relative preferences of men and women interact strongly with reservations for women, with women presidents being much more likely to act on women's preferences. Perhaps the strongest endorsement of Chattopadhyay and Duflo (2004)'s findings comes from Deininger et al. (2020), which analyzes a 2006 survey of 242 villages spread across 16 states. The authors find that "with reservations, the time spent in fetching water declines significantly over time" (p. 346), and female labor force participation increases.[9]

In this paper, we revisit the question of whether reservations for women affect the provision of public goods or social outcomes. Like Chattopadhyay and Duflo (2004), we analyze data from Rajasthan, a state where random assignment of reservations has been conducted in a well-documented manner and for which administrative outcomes are available in digital format since 2011. We focus on the policy consequences of reservations in 2005, when one-third of GPs were reserved for women, and in 2010, when the share of reservations was raised to one-half. This analysis exploits data from a much larger number of GPs than any previous study (our collection of GPs is larger than all previous studies combined), and we corroborate these results by analyzing similar data from Uttar Pradesh for the same period. To further demonstrate the robustness of our findings, we restrict attention to a subset of GPs in Rajasthan, where reservations were implemented in four consecutive elections from 2005 to 2020 or never implemented at all during this period.

[9]In addition to the literature on the allocation of public goods by GPs, there is also a large literature on the social, economic, and distributive effects of women leaders, using reservations as an identification strategy. The causal effects reflect the combination of role-model effects, allocation effects, and other dynamics arising from the installation of women leaders. A short list of such studies includes findings suggesting that local reservations for women contributed to early childhood development (Pathak and Macours, 2017), improved the fairness of land inheritance for women (Brulé, 2020), increased female labor force participation (Deininger et al., 2020), but mustered an inferior public response to COVID (Pandey et al., 2023). An even larger literature leverages inter-state variation or over-time variation rather than random assignment per se. For example, Kumar and Prakash (2017) find reductions in child mortality, O'Connell (2020) find increases in adolescent girls' educational attainment, and Iyer et al. (2012) find increased reporting of crimes against women.

This paper is structured as follows. We begin by briefly summarizing the competing theories for why reservations for women may or may not be consequential for local government output in the Indian context. Next, we describe the reservation system in Rajasthan as implemented in 2005 and 2010. In particular, we describe the statistical patterns associated with the sequence of assignments, as well as covariate balance across reserved and unreserved GPs. After establishing the reservations for women largely operate in a manner consistent with random assignment in these two elections (whereas subsequent reservations were in some places selected by lottery and in other places determined by rotation), we describe the outcome measures drawn from public works programs, censuses, and large-scale official surveys. After presenting a statistical model to estimate the average effect of reservations on outcomes, we assess the effects of reservations in 2005 and 2010 on outcomes measured from 2011 through 2019. Although our large collection of GPs is well-powered to detect even substantively small effects, our results suggest that reservations for women had little effect on any of these outcomes. The same holds true when we focus specifically on GPs that have been continuously reserved for women versus never reserved for women over four consecutive elections since 2005. Nor do we find effects in the state of Uttar Pradesh when we revisit and expand the scope of Bose and Das (2018)'s investigation there. We conclude by discussing potential explanations for why women's reservations may have weak effects in this context.

2 Competing Hypotheses

Perhaps due to the litany of mixed results on the effects of reservations for women on public goods provision, scholars have offered a variety of reasons to think that effects should be strong, weak, or in flux over time. We briefly review these arguments about causal mechanisms here because, in some cases, they suggest lines of empirical investigation that we will take up later.

Another theoretical perspective stresses the ethnic and interest group divisions that shape local politics. In this vein, women are sometimes said to upset the dominance of high-caste men (Cassan and Vandewalle, 2021), although others have charged that women are, on average, less likely to distribute public resources to lower castes (Bardhan et al., 2010). Another version of the thesis that reservations disturb interest group arrangements is the claim that women leaders are less prone to corruption, whether due to temperament or merely because, as newcomers, they have had less opportunity to form quid pro quo arrangements (Hessami and da Fonseca, 2020).

Pessimistic accounts of women's reservations tend to deploy the following arguments. Some suggest that women are stymied by "patriarchal" local rivals who prevent them from governing in a manner that reflects their distinctive priorities (Lawrence and Hensly, 2023). Another argument is that women are political novices and therefore either ignorant

about how local government works (Gajwani and Zhang, 2008) or easily outmaneuvered by their opponents (Prodip, 2023). A further argument is that many women elected under the reservation system are likely to hail from politically active families (Chattopadhyay and Duflo, 2004) and were encouraged by their spouses to run and, once in office, operate as figureheads (Tripathi, 2022).

Somewhere between the poles of optimistic and pessimistic accounts are arguments that the average treatment effect changes as social and political conditions evolve over time. Lawrence and Hensly (2023) argue that women presidents perform well when the headwinds of patriarchy are not too severe, implying that there may be a gradual trend of improving performance as rural Indians gradually become more accepting of women's leadership (Evans et al., 2022, Bhatnagar, 2023). Similarly, arguments suggesting that women are ineffective because they are inexperienced (Gajwani and Zhang, 2008) imply that performance will improve as the stock of experienced women politicians increases, though it could also be that women become less effective as they gain experience because they become more enmeshed in quid pro quo arrangements with local interest groups (Hessami and da Fonseca, 2020). In all, strong arguments on all sides underscore the value of revisiting the question with fresh data.

3 Reservations for Women in Rajasthan

The official government policy in Rajasthan is that a lottery is used to decide which seats are reserved for women in each election cycle. The empirical implication of this policy is that a GP's reservation status at a given time is statistically independent of variables measured before the assignment of reservations, potential outcomes at the time of random assignment, and subsequent reservation lotteries.

Statistical evidence at the GP level for 2005 and 2010 confirms the first prediction regarding covariate balance.[10] Reservations in 2005 and 2010 are no more correlated with variables measured in the 1991 or 2001 census than would be expected by chance. F-tests of the joint significance of variables such as (male and female populations, male and female children under six, male and female schedule caste members, male and female scheduled tribe members, male and female literate members, male and female workers, and male and female agricultural cultivators) as predictors of reservation status in each election cycle show the expected pattern of non-significant p-values (see Table 1).

Another implication of lottery-based assignment is the statistical independence of reservations in 2005 and 2010. Consistent with the hypothesis of random assignment, we find that reservations in 2005 are weakly correlated with reservations in 2010. Regressing a binary indicator for women reservations in 2010 on a binary indicator of women reservations

[10]The patterns of reservations become more complicated from 2015 on, with some regions within Rajasthan allocating reservations by lotteries and others by rotation (reserved become unreserved and vice versa).

in 2005 for the 5,246 GPs in our sample (which we describe below) renders an estimated slope of 0.01 with a standard error of 0.01. Repeating the exercise with fixed effects for the 33 districts within Rajasthan leaves these coefficients and standard errors unchanged except for rounding ($\hat{b} = 0.0012, \hat{SE} = 0.0116$). The same may be said for regressing reservations in 2010 on reservations in 2005 within each of the 33 districts–just 1 of the 33 slope estimates is significant at the $p < .05$ level, which is about what one would expect by chance.

The one feature of Rajasthan's reservation lotteries that does not follow the expected pattern is the marginal distribution of reservations in 2010. In 2005, one-third of all GPs should have been reserved for women, and indeed, the actual proportion was 34 percent.[11] In 2010, one-half of all GPs should have been reserved for women; in fact, the proportion was 47.6 percent, which is slightly but significantly different from the expected value of 50 percent. Close inspection of the 245 Samitis' allocations reveals that not a single Samiti rounded an odd number of GPs in favor of women's reservations. This apparent rounding policy had the cumulative effect of reducing the number of women's reservations statewide. In addition, in one district, Baran, we find a clear instance of misapplication of the reservation rules insofar as none of the 37 Sarpanch positions allocated to Scheduled Castes was reserved for women, and just 1 of the 50 Sarpanch positions allocated to Scheduled Tribes was reserved for women. The rest of the districts' patterns look consistent with random assignment in accordance with the official rules. In the analysis below, we show that inclusion or exclusion of this district has little effect on our results.[12] The identification strategy proposed by Chattopadhyay and Duflo (2004) seems to be on firm ground for the 2005 and 2010 Rajasthan elections.

4 Identification and Estimation of Causal Effects

Define the intention-to-treat effect (ITT) in a given election cycle as the average effect of assigning GPs $\{i \in 1, \cdots, N\}$ to either a reservation for women ($z_i = 1$) or not ($z_i = 0$), ignoring whether some of the unreserved GPs elect a woman.[13]

[11] This proportion is based on the full set of GPs in 2005 before merging them with GPs for other years, which results in some loss of observations when we exclude cases with less-than-perfect match quality.

[12] We also assessed the bivariate distribution of 2005 and 2010 reservations at the district and Samiti levels. We used a chi-square test to compare the actual and expected number of reservations; we found significant imbalances at the $p < .10$ level in just 2 of the 33 districts or, at a finer geographic level, in 8 of 245 Samitis. These rejection rates are about what would be expected by chance.

[13] It is rare for women to be elected without reservations. In 2005, 95.5 percent of unreserved seats were won by men; in 2010, this figure was 90.3 percent. If we sought to estimate the complier average causal effect, i.e., the average effect of having a woman president in those GPs where women would be elected if and only if the GP were reserved, we would define the (endogenous) election of a woman as d_i and regress outcomes on d_i using z_i as an instrumental variable. In this case, doing so will make the resulting estimates and their standard errors slightly larger. To estimate the complier average causal effects of reservations in 2010, for example, the instrumental variables estimate would be the reduced form estimate divided by 0.903.

Under random assignment of reservations for women and under the stable unit treatment value assumption (Angrist et al., 1996), unbiased estimation of the average intent-to-treat effect is straightforward.[14] One may simply compare mean outcomes for reserved and unreserved GPs or, equivalently, regress a given outcome Y_i on z_i:

$$Y_i = \beta_0 + \beta_1 z_i + u_i$$

where u_i is an unobserved disturbance term.[15]

This type of analysis could be conducted election cycle by election cycle, each time regressing post-election outcomes (e.g., budget allocations) on reservation status in the most recent election. But notice that this characterization of potential outcomes imposes a strong and perhaps fallible substantive assumption: outcomes respond only to current reservation status. What happens if potential outcomes are shaped by two inputs: the current assigned reservation status and the reservation status in the preceding election cycle(s)? Can the average causal effect of each of these inputs still be identified? Fortunately, the identification problem is innocuous when reservations are assigned by lottery in each election cycle. In that case, reservations in this cycle are statistically independent of reservations in all previous cycles. Even if past reservation status were influential, it is balanced for the GPs that currently do or do not have reservations for women, which implies that a simple difference-in-means estimator will identify the average effect of reservations in the current period. And if one wanted to identify lagged effects (or the interaction effects between reservations in different periods), one could do that by comparing average outcomes within each of these strata.

The implication is that when assessing the effects of lottery-based reservations, we should include a series of reservations as right-hand-side regressors back to the election cycle in which a full lottery was implemented (in our case, 2005). With lottery-based reservations in hand, suppose we expand our regression model to account for past reservation status. Consider the case in which Y_i refers to an outcome observed after the 2010 election, and we seek to assess the average contributions of reservations in the prior two elections. Using the notation z_{ji} to refer to the reservation status of GP i in year j, the regression equation becomes

$$Y_i = \beta_0 + \beta_1 z_{2005i} + \beta_2 z_{2010i} + \beta_3 z_{2005i} z_{2010i} + u_i.$$

[14] SUTVA in this context presupposes that outcomes in a given GP reflect the reservation status of that GP and not the reservation status of other GPs. It is not impossible that budget allocations in nearby GPs could influence each other's outcomes, but it seems implausible that such spillover effects would materially bias our estimates.

[15] To improve the precision with which β_1 is estimated, one could adjust for pre-treatment covariates, such as outcomes from years prior to the elections, census measures, and the like. We draw covariates from the 1991 and 2001 censuses, but we find that including them has little bearing on the estimates or their standard errors.

This agnostic modeling approach offers a parsimonious account of how outcomes respond to election status in each cycle. The model essentially fits four parameters to four mean outcomes: (1) when no reservations are assigned in either election, (2) when reservations occur only in 2005, (3) when reservations occur only in 2010, and (4) when reservations occur in both 2005 and 2010. The interaction term (β_3) allows for the possibility that the effects of reservations in a given election amplify (or diminish) when a GP is reserved for women in consecutive elections. We seldom find any convincing evidence of such an interaction, however; for ease of interpretation, the main text reports only the main effects and assumes $\beta_3 = 0$. The full results, including the interaction, are presented in the appendix. When outcome data are measured at the village level, we cluster the standard errors at the GP level, as GP is the level of random assignment.[16]

5 Data

Our final dataset builds on four key datasets: election data, SHRUG (Asher et al., 2019), administrative data on MNREGA outcomes, and the Local Government directory,[17] the "[a]uthoritative directory" on rural government by the Indian government that provides information about the relationship between GPs (electoral data) and census areas (administrative units) like villages.

5.1 Outcomes

The major responsibilities of government at the GP level are to administer local infrastructure (e.g., public buildings, water, and roads) and to identify eligible recipients of public assistance and job programs. Central funding comes from higher levels of government, but GPs play a role in requesting or directing this funding. Thus, leaders may affect outcomes due to the effort they exert when proposing projects or lobbying for funding, or they may shape the GP's policy priorities, affecting what kinds of projects are funded or which types of villagers receive assistance. Beyond this list are other causal pathways that involve inspiring others to contribute to public goods or, conversely, provoking resistance and deadlock.

[16]Because we have one district that appears to have assigned reservations with a distinctly different probability than the others, we can estimate treatment effects via weighted least squares, weighting each treated GP by the inverse of its probability of receiving treatment and weighting each untreated GP by the inverse of its probability of being assigned to control (Hirano et al., 2003). Because this alternative estimation approach produces similar results, we report it in the appendix. For simplicity, when generating the regression results presented in the main text, we simply exclude the problematic district.

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

5.2 MNREGA

The largest and best-measured flows of public funding come from a vast public works program consolidated under the Mahatma Gandhi National Rural Employment Guarantee Act of 2005 (MNREGA). This act, which was rolled out to all regions of India by 2008, guarantees 100 days of wage employment per year to anyone willing to perform unskilled labor. This entitlement program, in theory, sets aside at least one-third of the jobs for women applicants. To access MNREGA work, applicants must request registration from the local village council (GP). Once registered, a household seeking work must submit a written application to the GP. Employment should be provided within 15 days of the application, and if this timeline is not met, the household is entitled to a daily unemployment allowance. MNREGA projects are intended to contribute to local infrastructure, such as irrigation, drinking water, roads, and repairs of community buildings. However, the material component cost of NREGA projects should not exceed 40% of total project costs, with the majority of expenses going towards labor wages. GP leaders propose projects, hire workers, and generally manage implementation. Annual reports make it possible to study these transactions in detail.[18]

One key advantage of using MNREGA activities as outcomes is that this program puts the village Sarpanch into a central distributive and administrative role. The field assistant, who assists the village council in MNREGA implementation, is appointed based on the Sarpanch's recommendation. The Sarpanch is responsible for selecting suppliers of material inputs for projects, and Afridi et al. (2017a) have highlighted the oversight the Sarpanch can exercise over the distribution of funds. The village Sarpanch also serves as the link between citizens and government officials (Misra, 2022). Most importantly, MNREGA payments depend on ongoing projects and their labor requirements, which are again predominantly controlled by the Sarpanch. Afridi et al. (2017b), Shon and Swatman (1998)

The precursor to MNREGA, Jawhar Rozgar Yojana (JRY), was used by the early studies of reservations to gauge the allocative influence of women when they ascended to the position of panchayat president (Chattopadhyay and Duflo, 2004). The particular outcomes of interest concern the number of projects and expenditures in various domains, which the public database divides according to whether the projects are ongoing or completed. Because our outcomes are measured from 2011 on, they give a sense of the cumulative effects of reservations in 2005 and the short-term effects of reservations in 2010.

To give readers a sense of scale, MNREGA projects employed workers from 3,686,818 households in 2014 and 6,385,201 households in 2023, spending approximately 259,688 lakhs ($412,200,000) in 2014 and 763,669 lakhs ($954,500,000) in 2023 in Rajasthan. A majority

[18]The government publishes detailed records of what projects were funded via the scheme, who was paid, what days were worked, and such. We create our MNREGA dataset by scraping the Government of India website (see here). Our data include R1 and R6 reports, which provide spending summaries and the number of projects in a few broad areas from 2014 to 2022.

of MNREGA spending is focused on rural infrastructure (28%) and water conservation, irrigation, and agriculture support (water conservation 26%, renovation of traditional water bodies 8%, micro-irrigation projects 6%, drought-proofing 3% and land development 3%). The pattern of MNREGA allocations varies markedly across GPs with similar census characteristics, suggesting that Sarpanches have considerable latitude in how they allocate funding.

5.3 Quality of Life Measures

Although the MNREGA records provide useful indicators of GP leaders' policy outputs, they leave open the question of whether the quality of life changes materially under female leadership. For quality of life measures, we turn to censuses, which furnish information about employment, poverty, schooling, and health. They also provide information about the infrastructure that contributes to the quality of life, such as access to water, maternal care facilities, and schools. We use the SHRUG 2 (Asher et al., 2019) dataset to obtain village-level data from 1991, 2001, and 2011 Census Abstracts and the 2011 Census Directory (of the Registrar General and Census Commissioner, 2011, Chandramouli and General, 2011), the Sixth Economic Census (2013) (Central Statistics Organisation and Programme Implementation, 2013), and the 2020 Mission Antyodya survey (Ministry of Rural Development, 2020).

5.4 Balance Tests

To confirm that merging datasets and excluding certain unmatched GPs does not upset the equivalence among randomly assigned groups, Table 1 compares the means for pre-treatment covariates, subdivided by reservation assignments in both 2005 and 2010. As expected, the means differ across the randomly assigned groups no more than would be expected by chance. A likelihood-ratio test confirms that the covariates are jointly insignificant predictors of assignment to the four treatment combinations ($p = .9$).

6 Results

We begin by focusing on the effects of reservations in 2005 and 2010 on MNREGA programs and Economic Census outcomes in 2013 and 2014, respectively. Our model for each of the outcome measures is:

$$Y_i = \beta_0 + \beta_1 z_{2005i} + \beta_2 z_{2010i} + u_i$$

where β_1 and β_2 represent the ITT effects of reservations in 2005 and 2010, respectively. The underlying hypothesis is that β_1 and β_2 will be positive for outcomes that reflect women's interests and/or priorities: drinking water, sanitation, women's employment, and schooling for girls. Priorities favored by men, such as roads, are expected to show negative effects.

Table 1: Summary statistics and p-value of the F-statistics for variables.

	Means by Group				
Variable	**T-T**	**T-C**	**C-T**	**C-C**	**p (F-stat.)**
Total population	4109.5	4138.1	4053.94	4183.93	0.4
Total population (Female)	1979.37	1999.23	1958.8	2020.76	0.42
Total SC (Female)	374.26	367.4	354.12	369.36	0.34
Total ST (Female)	313.74	324.26	333.08	318.11	0.78
Number of medical facilities	1.19	1.14	1.16	1.14	0.7
Number of maternity homes	0.02	0.04	0.04	0.04	0.15
Number Of Family Welfare Centres	0.04	0.04	0.06	0.06	0.06
Number of Primary Schools	4.31	4.32	4.24	4.35	0.61
Number of Middle Schools	1.49	1.61	1.48	1.5	0.05
Handpumps	4.52	4.33	4.28	4.45	0.21
Tap water	6.51	6.13	6.25	6.3	0.46
Wells	4.33	4.07	4.1	4.2	0.34
Banking facility	0.21	0.22	0.21	0.22	0.94
Power supply	3.19	3.09	3.1	3.11	0.81
Approach - mud road	2.53	2.4	2.43	2.44	0.59
n	728	820	1480	1626	

Note:
T denotes GPs that were reserved for women and C denotes other GPs. All the variables were taken from the 2001 Census Village Directory. N indicates the number of Gram Panchayats. The p-value of the F-statistic is derived from regressions using randomized inference. We also fit a null model and a multinomial model that used all the above variables to predict the type of transition, e.g., T-T, T-C, etc. The p-value of the Likelihood Ratio test is 0.48 which suggests that the complete multinomial model does not fit any better than a null model with no predictors.

We begin by looking at the effects of reservations in 2005 and 2010 on MNREGA outcomes in 2011, 2012, 2013, and 2014. Of the 4,654 GPs in this analysis, 820 were assigned to reservations only in 2005, 1480 were assigned to reservations only in 2010, and 728 were assigned to reservations in both cycles. In the interest of brevity, we present a condensed set of results in Table 2, focusing on the total number of completed projects between 2011 to 2014; these results convey the basic pattern across all outcomes. Regression results for ongoing from completed projects may be found in the SI 2.

The results presented in Table 2 suggest the absence of appreciable reservation effects for either election cycle. The first column reports the effects on the total number of projects

Table 2: Effects of Reservations on the Number of Completed MNREGA Projects, 2011-2014

	All	Rural Roads	Sanitation	Water Conservation	Traditional Water
	(1)	(2)	(3)	(4)	(5)
2005	−0.03	−0.23	0.29	−0.05	0.11
	(1.88)	(0.27)	(1.43)	(0.27)	(0.14)
2010	−0.86	0.41	−0.87	0.05	0.002
	(1.78)	(0.25)	(1.36)	(0.25)	(0.13)
Constant	51.56	7.93	17.77	5.76	2.69
	(1.38)	(0.20)	(1.05)	(0.20)	(0.10)
Covariates	No	No	No	No	No
Observations	4,351	4,351	4,351	4,351	4,351
Adjusted R^2	−0.0004	0.0003	−0.0004	−0.0004	−0.0003

Note: *p<0.1; **p<0.05; ***p<0.01
All - Total completed or ongoing MNREGA projects (2011–2014);
Rural Roads: Number of projects to improve connectivity and roads (2011–2014);
Sanitation: Number of projects to improve sanitation facilities (2011–2014);
Water Conservation: Number of projects to improve water conservation (2011–2014);
Trad. Water: Number of projects to maintain traditional water bodies (2011–2014).

pooled over all years. The intercept (51.56) represents the expected number of projects in the absence of reservations in either 2005 or 2010. The estimated effects of reservations in 2005 and 2010 are weakly negative but statistically indistinguishable from zero. The estimated effect of 2005 reservations is -0.83, which represents a 0.2 percent decrease from the control group mean. It does not appear that reservations appreciably affect the overall output in terms of MNREGA projects.

The same conclusion holds when we look at specific project categories that ostensibly reflect the distinctive preferences of men and women. Infrastructure is said to be a greater policy priority for men. Rural roads account for an average of 7.93 projects in unreserved GPs. Reservations in 2005 lower this mean by -0.23 (s.e. = 0.27), while reservations in 2010 raise the mean by 0.41 (s.e. = 0.25). The apparent effects seem to be negligible. Water projects are said to be favored by women. But neither the "Water Conservation" category nor the "Traditional Water" category show a coherent pattern of effects. In the former case, 2005 reservations have an estimated effect of -.05, and 2010 reservations have an estimated effect of 0.05. In the latter case, 2005 reservations have an estimated effect of 0.11, and 2010 reservations have an estimated effect of .002. Each of these estimates lies within one standard error of zero. The same inscrutable pattern applies to sanitation, where the estimated effect is 0.29 in 2005 and -.87 in 2010, again both within a standard error of zero. Table 2 seems to suggest weak effects across the board, and this conclusion persists when we disaggregate the MNREGA outcomes by year, focus on expenditure amounts rather than the number of projects, or consider test statistics such as medians rather than means.

Table 3: Effects of Reservations on Long-term Outcomes Concerning Children

	Immunized	Underweight	Truant	Anganwadi	SC/ST
	(1)	(2)	(3)	(4)	(5)
2005	−4.05	3.82	−7.10	−4.40	24.58
	(8.03)	(5.03)	(11.52)	(10.11)	(22.79)
2010	−4.87	0.21	−15.38	−3.08	−48.22
	(7.56)	(4.74)	(10.85)	(9.52)	(21.47)
Constant	162.70	30.37	116.37	215.82	209.62
	(5.88)	(3.68)	(8.43)	(7.40)	(16.68)
Covariates	No	No	No	No	No
Observations	3,365	3,365	3,365	3,365	3,365
Adjusted R^2	−0.0004	−0.0004	0.0001	−0.001	0.001

Note: *p<0.1; **p<0.05; ***p<0.01
Immunized: Percent of 0–3 year old children who are immunized.
Underweight: Percent underweight children under 6.
Truant: Percent children not attending school.
Anganwadi: Percent of children under 6 registered in Aanganwadis.
SC/ST: Number of SC/ST/OBC/minority children getting scholarship.

In order to examine the long-term consequences of reservations in 2005 and 2010, we next consider village-level outcomes measured in 2019. Table 3 presents regression results for outcomes related to the well-being of children. The first column shows a glimmer of an effect in the expected direction insofar as reservations in 2005 and 2010 reduced the number of underweight children under age six. Neither estimate is significantly less than zero, but both are more than one standard error away from zero. The other outcomes, however, show a less propitious pattern. The number of truant children increases, and the number of children in preschool declines, although none of these apparent changes is statistically distinguishable from zero. The number of SC/ST/OBC school enrollments shows no consistent pattern. The number of immunized children also moves up or down with reservations, depending on the election cycle.

Table 4: Effects of Reservations on Long-term Outcomes Concerning Schooling

	Primary School	Middle School	Tot. Pri. School Students	Mid-day Meal
	(1)	(2)	(3)	(4)
2005	−0.10	0.06	−22.69	−0.05
	(0.14)	(0.10)	(34.33)	(0.14)
2010	0.01	0.0002	−13.42	−0.02
	(0.13)	(0.10)	(32.33)	(0.13)
Constant	4.03	2.41	491.90	3.60
	(0.10)	(0.08)	(25.12)	(0.10)
Covariates	No	No	No	No
Observations	3,365	3,365	3,364	3,365
Adjusted R^2	−0.0004	−0.0005	−0.0004	−0.001

Note: *p<0.1; **p<0.05; ***p<0.01
Is there a primary school or not;
Is there is a middle school available;
The number of students enrolled in primary school per 1000 people;
Is there is a mid-day meal facility for students in school.

Table 4 continues the progression of null results for school-related outcomes in 2019. The regression results suggest that women's reservations in 2005 and 2010 had little effect on the availability of a primary school or a middle school. Primary school enrollments seem to move in opposite directions depending on whether reservations occurred in 2005 or 2010. The provision of midday school meals for students is also unaffected by reservations in earlier election cycles.

Table 5, which focuses on health-related outcomes in 2019, reports the sole significant point estimate. The first column suggests reservations in both 2005 and 2010 increased the number of primary health centers. The number of community health centers and maternal health facilities also rose slightly, though not significantly. Overall, the estimated effects on health facilities are in the expected direction but statistically equivocal, and there do not appear to be detectable gains in terms of health. The proportion of anemic pregnant women in 2019 was essentially unaffected, and the number of non-stunted children was slightly lower in previously reserved locations.

Table 5: Effects of Reservations on Long-term Outcomes Concerning Health

	Primary Health Ctr.	Community Health Ctr.	Maternal Health Ctr.	Anemic Preg. Women	Non-Stunted Children
	(1)	(2)	(3)	(4)	(5)
2005	0.06	0.04	−0.02	0.45	7.16
	(0.04)	(0.04)	(0.09)	(1.87)	(4.76)
2010	0.03	0.05	0.11	−0.19	−5.84
	(0.03)	(0.04)	(0.09)	(1.76)	(4.48)
Constant	0.26	0.49	1.71	22.43	34.61
	(0.03)	(0.03)	(0.07)	(1.37)	(3.48)
Covariates	No	No	No	No	No
Observations	3,365	3,365	3,365	3,365	3,365
Adjusted R^2	0.0004	0.0003	−0.0002	−0.001	0.001

Note: *p<0.1; **p<0.05; ***p<0.01
Is there a primary school available?
Is there a middle school available?
The number of students enrolled in primary school per 1000 people;
Binary variable to indicate if there is a mid-day meal facility for students in school.

Table 6 reports an assortment of 2012 and 2019 outcomes. The outcomes for 2012 concern outcomes such as the proportion of households living in structures with solid walls or solid roofing. The point estimates are all close to zero. The amount of irrigated land seems to be unaffected by reservations, with a positive point estimate for 2005 and a negative point estimate for 2010. The 2019 outcomes include the number of female children under 6, which addresses the question of whether women leaders reduce female infanticide rates. The point estimates for both 2005 and 2010 are weakly negative, whereas our expectation is that reservations should produce more girls. BPL ration cards are, if anything, diminished in GPs that had women's reservations in 2005 or 2010. The number of recent mothers eligible for cash transfers shows no apparent relationship to past reservations.

Table 6: Effects of Reservations on Other Short- and Long-term Outcomes

	Female Children	BPL Cards	Maternity Benefit	Solid Wall	Solid Roof	Irrigation
	(1)	(2)	(3)	(4)	(5)	(6)
2005	−1.05	−12.38	−2.53	0.06	0.03	342.37
	(9.93)	(15.77)	(1.63)	(0.12)	(0.11)	(2,223.29)
2010	−9.86	−13.33	0.87	0.02	0.04	38.79
	(9.35)	(14.85)	(1.53)	(0.12)	(0.10)	(2,092.88)
Constant	210.75	337.90	27.75	3.09	2.53	3,412.15
	(7.26)	(11.54)	(1.19)	(0.09)	(0.08)	(1,626.82)
Covariates	No	No	No	No	No	No
Observations	3,339	3,339	3,339	3,339	3,339	3,339
Adjusted R^2	−0.0003	−0.0002	0.0002	−0.001	−0.001	−0.001

Note: *p<0.1; **p<0.05; ***p<0.01
Number of female children (0-6 years);
Number of Households having BPL ration cards;
Number of eligible beneficiaries under Pradhan Mantri Matru Vandana Yojana
Share of households with solid wall - wood/stone/sheets/burnt brick/concrete;
Share of households with solid roof - stone/slate sheets/concrete burned brick;
Total land with assured irrigation for two crops

Table 8 summarizes the litany of null results. It lists all the outcomes we considered, including some that we did not present in the main regression tables. Point estimates that are statistically insignificant at the .05 level using a two-tailed test are shown in black; significant estimates are shown in red. The table makes clear that we observe very few significant estimates. Any kind of multiple-comparisons correction would render these marginally significant estimates insignificant.

6.1 What about the Cumulative Effects of Sustained Reservations?

Would the effects of women's reservations be more noticeable if we focused our attention on instances in which GPs were reserved for women in four successive elections (2005, 2010, 2015, and 2020), as compared to GPs that were never selected for women's reservations in any of these years? By focusing on settings where women leaders have been in power for almost two decades, we address arguments suggesting that women leaders are ineffective due to their lack of experience.

The challenge when studying elections after 2010 in Rajasthan is that the randomization procedure becomes more opaque. Unlike reservations in 2005 and 2010, whose statistical patterns conform to what one would expect given independent random assignment, reservations in 2015 and 2015 seem to be a combination of random assignment and rotation. By rotation, we mean that GPs reserved for women in a given election are not reserved in the next election, while GPs not reserved in a given election are reserved in the next election. Detailed inspection of the transition patterns at the district and Samiti level reveals no consistent patterns, so we cannot simply partition Rajasthan into geographic areas that use lotteries and those that do not. By focusing on "extreme" GPs – reserved for four consecutive elections or not reserved for four consecutive elections – we exclude cases in which rotation was used after 2010. That alone does not guarantee that extreme reservations are ignorable because we do not know why rotations were applied in some GPs but take some solace from the fact that the GPs that were reserved throughout and those that were never reserved seem to have statistically indistinguishable background characteristics.

Looking at the aggregate number of completed MNREGA projects over the period 2011-2023, we see no evidence of a reservation effect (see Table 9). Rural roads, sanitation, water conservation, and traditional water each have insignificant point estimates.

6.2 Out-of-Sample Replication in Uttar Pradesh

Are our results specific to Rajasthan? The study most similar to ours in terms of research design is Bose and Das (2018), which assesses the effects of reservations in 2005 and 2010 on

MNREGA allocations in Uttar Pradesh using data from 9 districts.[19] They find reservations to have some apparent effects on employment in MNREGA, but mainly in regions in Phase 1 of MNREGA. These apparent effects do not appear in Phase 2, nor do they seem to apply to reserved GPs' allocations to many specific MNREGA domains, such as drought-proofing or irrigation canals (p.214). Although we obtained their replication data, we set about to build the dataset up from scratch, linking electoral data to MNREGA data not only for the districts they studied but also for the districts that they excluded.

As shown in the SI (see Table SI 3.3), our results do not reveal any substantial or significant effects of women's reservations except for an unexpectedly negative coefficient for 2005 for total job cards. When we examine 2,137 GPs in the same districts that Bose and Das (2018) studied, we find little indication that reservations affected MNREGA projects, and we find relatively weak effects on women's employment. Expanding the analysis to 21,627 GPs in all of Uttar Pradesh confirms these null results. We conclude that evidence from Uttar Pradesh is in line with what we find in Rajasthan.

7 Conclusion

Unlike the classic Chattopadhyay and Duflo (2004) study and several studies since, we find no evidence suggesting that reservations for women changed budgetary outlays. In Rajasthan, reservations for women at the GP level in 2005 and 2010 had minimal effects on both discretionary allocations and census outcomes. We find no apparent effects on the number or content of MNREGA-funded projects. Although early results reported by Chattopadhyay and Duflo (2004) seemed to suggest that women leaders would allocate more funding and effort to policy domains that were priorities for women, we find meager effects of gender reservations on completed or ongoing MNREGA-funded projects in every policy domain, including maternal health, water, or sanitation. Census outcomes measured in 2019 show no apparent effects on long-term outcomes in domains such as schooling, health, or agriculture. This conclusion holds even when we compare high-dosage GPs that were reserved for women in four consecutive elections to GPs that were never reserved. And it holds when we replicate our analysis in Uttar Pradesh, contrary to previously reported results based on a subset of districts in that state (Bose and Das, 2018). In addition, as shown in the supplementary materials, the weak effects we find for Rajasthan are little changed when we focus attention specifically on GPs reserved for leaders from Scheduled Tribes, on the grounds that such leaders have expanded powers (Gulzar et al., 2024). Reservations for

[19] They write on pp.207-8: "To analyze the role-model effect of women leaders, we focus on the number of women employed under NREGA ... [and] on the number of works undertaken under NREGA for the various categories of public works, which include works that are both gender neutral and gender-specific in terms of benefits."

women in such GPs again seem to have weak effects on MNREGA allocations and other outcomes.

We hasten to add that our data do not address many of the original findings presented by Chattopadhyay and Duflo (2004). For example, we have no data that speaks to the question of whether women presidents are less likely to take bribes. What we can infer, however, is that if women's leadership reduced the extent of petty corruption in local government, that change was insufficient to affect the outcomes examined here, such as the initiation or completion of public works. The same argument holds for the causal claim that women leaders inspire women villagers to participate more in local GP meetings. That may be true, judging from early surveys, but evidently, women's heightened participation does not have downstream consequences for policy outcomes such as dispensaries, maternal health units, or the allocation of MNREGA public works.

What explains the fact that women's reservations do not have appreciable effects on program outputs or tangible outcomes?[20] Are women leaders stymied by their male opponents? Or are male leaders adjusting their allocative decisions to be more in line with their female counterparts, who may come into power in the future? Or are male and female leaders more similar than previously thought in terms of their policy preferences and capacity to put them into effect?

These questions suggest a new theoretical and empirical research agenda. To date, this literature has been guided by the theoretical model advanced by Chattopadhyay and Duflo (2004), which downplayed electoral incentives and instead emphasized the personal tastes of the local leaders. With the prospect of reservations for women in state legislatures mandated by the 128th Amendment to India's Constitution in 2023 (Joshi, 2023), the time has come to reopen the question of what role electoral incentives play in this context. Do women who run for office appeal to voters with policy- or program-related promises? If so, do women candidates make efforts to appeal to male voters' preferences, given that typically more than half of all voters in GP elections are male? Once in office, do female incumbents typically seek re-election if the district is reserved again? If women candidates are indeed electorally motivated, one could imagine ways in which a "median voter" logic would push them to appeal to men's policy preferences. Did the behavior of women council presidents change, on average, when the share of reserved GPs was raised from one-third to one-half, perhaps because electoral incentives changed when the odds that a woman would be re-elected improved?

On the other hand, if the women who come to hold office are indifferent about winning reelection, they might be at liberty to pursue their own personal policy goals. We lack

[20] Or, to be more precise, do not appear to have appreciable effects during the period we examine. The more contemporary period has the advantage of allowing us to assess how reservations for women operate in a "steady state" given that the policy had been in place for multiple election cycles by 2005. Raabe et al. (2009) speculates that Karnataka, a place that had reservations for women before the nationwide mandate, might show weaker effects than other states in India during the period when the policy was novel.

systematic empirical evidence about what those personal policy goals are. One conjecture is that those goals are inherited from male family members who are part of the ruling elite in a GP; the men hold office until a reservation is declared and wait in the wings while their female family member presides. Once the reservation is no longer operative, the male family members return to power. Although anecdotes abound, systematic evidence about the extent of the prevalence of such family dynasties remains elusive. One possible explanation for the contrast between our results and results based on earlier election cycles is that, over time, men in power became more skilled at putting their female relatives into leadership positions. By that account, women initially brought about policy changes under the reservation system, but gradually, men learned to game the system in ways that maintained the status quo.

As the research agenda pivots in a new direction, we would offer one further experimental finding that seems theoretically telling. If random reservations indeed disrupt male dominance of local politics in ways that are consequential for policy or governance, one might expect voters to reward women in reserved GPs when those reservations expire. This prediction would also follow from the notion that women office-holders dispel negative stereotypes about women (Beaman et al., 2009, Dasgupta, 2022), a claim that has received empirical support from studies of reservations in urban settings (Bhavnani, 2009). However, as shown in Table SI 1.1, this hypothesis is not borne out in Rajasthan. Restricting attention to unreserved GPs in 2010, we find that GPs that experienced reservations in 2005 were no more likely than their randomly selected counterparts to elect a woman in 2010. Similarly, when we restrict attention to GPs who had no reservations in 2015, we find that reservations in 2010 had no effect on the election of a woman in 2015. It does not appear that the experience of having a woman GP leader changed voters' subsequent demand for women leaders.

Table 7: Summary of Reservations' Effects on Outcomes

Category	Outcome	Dataset	2011	2012	2013	2014	2015	2016	2017	2018	2019
Sanitation	Expenditure	MNREGA	•	•	•	•	•	•	•	•	•
	Open Drain	Antyodaya									•
	Closed Drain	Antyodaya									•
	Disposal	Antyodaya									•
Children	Expenditure	MNREGA	•	•	•	•	•	•	•	•	•
	Underweight Child	Antyodaya									•
	Not attending School	Antyodaya									•
	Immunized	Antyodaya									•
	Registered Antyodaya	Antyodaya									•
	SC/ST children enrolled	Antyodaya									•
Education	Primary School	Census		•							
	Secondary School	Census		•							
	Mid day meal	Antyodaya									•
	Vocational Center	Antyodaya									•
Health	Primary Health Center	Antyodaya									•
	Community Health Center	Antyodaya									•
	Anemic Pregnant	Antyodaya									•
	Maternal Health Facility	Antyodaya									•
	Non-stunted child	Antyodaya									•
Water	Traditional Water Renewal	MNREGA	•	•	•	•	•	•	•	•	•
	Rural drinking water	MNREGA	•	•	•	•	•	•	•	•	•
	Water Storage	MNREGA	•	•	•	•	•	•	•	•	•
	Bharat Nirman	MNREGA	•	•	•	•	•	•	•	•	•
	Micro-Irrigation	MNREGA	•	•	•	•	•	•	•	•	•
	Drought Proofing	MNREGA		•	•	•	•	•	•	•	•
	Share Irrigated Land	SECC		•							
	Piped Water Connection	Antyodaya									•
Additional	Pension (NSAP/PMKPY)	Antyodaya									•
Benefits	Food Security (Fair Price Shop)	Antyodaya									•
	Affordable Housing (PMAY)	Antyodaya									•
	Below Poverty Cards	Antyodaya									•
	Health Insurance (PMJAY)	Antyodaya									•
	Clean Cooking Gas(PMUY)	Antyodaya									•
	Financial Support Pregnant (PMMVY)	Antyodaya									•
	Farmer's Credit Card	SECC		•							
	Food Grain Expenditure	MNREGA	•	•	•	•	•	•	•	•	•
Economic	Female Employee Count	Econ Census			•						
Wellbeing	Firms with Female Owner	Econ Census			•						
	Two/Three wheeled Vehicle	SECC		•							
	Monthly income >5k	SECC		•							
	Share of HH with solid wall	SECC		•							
	Share of HH with solid roof	SECC		•							
Rural	All weather road	Antyodaya									•
Connectivity	Connectivity Expenditure	MNREGA	•	•	•	•	•	•	•	•	•

Table 8: Summary of all the outcomes.

Table 9: Effects of Reservations on the Number of Completed MNREGA Projects, 2011-2023.

	All	Rural Roads	Sanitation	Water Conservation	Traditional Water
	(1)	(2)	(3)	(4)	(5)
T-T-T-T	27.77	1.88	−1.64	−1.76	−0.39
	(44.40)	(3.26)	(9.39)	(4.24)	(1.24)
Constant	231.99	20.02	28.95	15.57	5.05
	(22.89)	(1.68)	(4.84)	(2.18)	(0.64)
Covariates	No	No	No	No	No
Observations	143	143	143	143	143
Adjusted R^2	−0.004	−0.005	−0.01	−0.01	−0.01

Note: *p<0.1; **p<0.05; ***p<0.01
All - Total completed or ongoing MNREGA projects (2011–2023);
Rural Roads: Number of projects to improve connectivity and roads (2011–2023);
Sanitation: Number of projects to improve sanitation facilities (2011–2023);
Water Conservation: Number of projects to improve water conservation (2011–2023);
Trad. Water: Number of projects to maintain traditional water bodies (2011–2023).

Table 10: Effect of Reserving GP For Women on Probability of Women Being Elected After GP is Unreserved.

	2010	2015
	(1)	(2)
2005	0.002	0.01
	(0.01)	(0.01)
2010		0.001
		(0.01)
Constant	0.09	0.01
	(0.01)	(0.01)
Observations	2,273	1,311
Adjusted R^2	-0.0004	-0.001
Note:	*p<0.1; **p<0.05; ***p<0.01	

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Supporting Information

SI 1 Are GP Reservations Random?

Table SI 1.1: Predicting 2010 GP Reservation Status Using the 2005 Reservation Status in Rajastan.

	All	With District FE	Phase 1 and 2
	(1)	(2)	(3)
2005	0.002	0.002	−0.02
	(0.02)	(0.02)	(0.03)
Constant	0.48	0.42	0.48
	(0.01)	(0.04)	(0.02)
District FE	No	Yes	No
Observations	4,351	4,351	1,413
Adjusted R^2	−0.0002	−0.003	−0.0005
Note:	*p<0.1; **p<0.05; ***p<0.01		
	Within district regressions yield 1 significant coefficient which is what we expect by chance.		

Table SI 1.2: Predicting 2010 GP Reservation Status Using the 2005 Reservation Status in UP.

	All	Phase 1 or 2
	(1)	(2)
2005	−0.03	−0.02
	(0.01)	(0.01)
Constant	0.34	0.34
	(0.004)	(0.01)
Observations	21,627	14,677
Adjusted R^2	0.001	0.0003
Note:	*p<0.1; **p<0.05; ***p<0.01	

SI 2 Robustness Checks

Table SI 2.1: Effects of Reservations on the Number of Ongoing MNREGA Projects, 2011-2014

	All	Rural Roads	Sanitation	Water Conservation	Traditional Water
	(1)	(2)	(3)	(4)	(5)
2005	1.24	−0.07	1.51	−0.12	0.10
	(5.88)	(1.38)	(2.20)	(1.40)	(0.82)
2010	−4.09	0.11	−1.31	−0.31	0.002
	(5.57)	(1.31)	(2.09)	(1.33)	(0.77)
Constant	211.30	49.00	23.20	32.62	15.47
	(4.33)	(1.02)	(1.62)	(1.03)	(0.60)
Covariates	No	No	No	No	No
Observations	4,351	4,351	4,351	4,351	4,351
Adjusted R^2	−0.0003	−0.0005	−0.0003	−0.0004	−0.0005

Note: *p<0.1; **p<0.05; ***p<0.01
All - Total ongoing MNREGA projects (2011–2014);
Rural Roads: Number of projects to improve connectivity and roads (2011–2014);
Sanitation: Number of projects to improve sanitation facilities (2011–2014);
Water Conservation: Number of projects to improve water conservation (2011–2014);
Trad. Water: Number of projects to maintain traditional water bodies (2011–2014).

Table SI 2.2: Effects of Reservations on the Number of Completed MNREGA Projects, 2011-2014. (With Interaction Term).

	All	Rural Roads	Sanitation	Water Conservation	Traditional Water
	(1)	(2)	(3)	(4)	(5)
2005	−2.35	−0.18	−2.23	−0.05	0.18
	(2.61)	(0.37)	(1.98)	(0.37)	(0.19)
2010	−2.50	0.44	−2.65	0.05	0.05
	(2.19)	(0.31)	(1.67)	(0.31)	(0.16)
2005 * 2010	4.84	−0.11	5.28	−0.01	−0.15
	(3.77)	(0.53)	(2.87)	(0.53)	(0.27)
Constant	52.34	7.91	18.62	5.76	2.67
	(1.51)	(0.21)	(1.15)	(0.21)	(0.11)
Covariates	No	No	No	No	No
Observations	4,351	4,351	4,351	4,351	4,351
Adjusted R^2	−0.0003	0.0001	0.0002	−0.001	−0.0005

Note: *p<0.1; **p<0.05; ***p<0.01
All - Total completed or ongoing MNREGA projects (2011–2014);
Rural Roads: Number of projects to improve connectivity and roads (2011–2014);
Sanitation: Number of projects to improve sanitation facilities (2011–2014);
Water Conservation: Number of projects to improve water conservation (2011–2014);
Trad. Water: Number of projects to maintain traditional water bodies (2011–2014).

Table SI 2.3: Effects of Reservations on the Number of Completed MNREGA Projects, 2011-2014. (Phase 1 and 2 Only.)

	All	Rural Roads	Sanitation	Water Conservation	Traditional Water
	(1)	(2)	(3)	(4)	(5)
2005	1.37	−0.36	1.29	−0.15	−0.02
	(3.44)	(0.38)	(1.98)	(0.58)	(0.28)
2010	1.64	0.44	0.87	0.004	−0.02
	(3.25)	(0.36)	(1.88)	(0.55)	(0.26)
Constant	49.73	6.67	11.17	7.56	2.73
	(2.54)	(0.28)	(1.47)	(0.43)	(0.21)
Covariates	No	No	No	No	No
Observations	1,413	1,413	1,413	1,413	1,413
Adjusted R^2	−0.001	0.0003	−0.001	−0.001	−0.001

Note: *p<0.1; **p<0.05; ***p<0.01
All - Total completed or ongoing MNREGA projects (2011–2014);
Rural Roads: Number of projects to improve connectivity and roads (2011–2014);
Sanitation: Number of projects to improve sanitation facilities (2011–2014);
Water Conservation: Number of projects to improve water conservation (2011–2014);
Trad. Water: Number of projects to maintain traditional water bodies (2011–2014).

Table SI 2.4: Effects of Reservations on the Number of Completed MNREGA Projects, 2011-2014. (ST Only.)

	All	Rural Roads	Sanitation	Water Conservation	Traditional Water
	(1)	(2)	(3)	(4)	(5)
2005	1.88	−0.57	0.06	1.13	0.09
	(3.13)	(0.47)	(2.16)	(0.72)	(0.29)
2010	1.26	0.79	0.01	0.41	−0.32
	(2.71)	(0.41)	(1.87)	(0.62)	(0.25)
Constant	42.80	6.89	11.43	6.57	2.49
	(2.04)	(0.31)	(1.41)	(0.47)	(0.19)
Covariates	No	No	No	No	No
Observations	1,005	1,005	1,005	1,005	1,005
Adjusted R^2	−0.001	0.003	−0.002	0.001	−0.0003

Note: *p<0.1; **p<0.05; ***p<0.01
All - Total completed or ongoing MNREGA projects (2011–2014);
Rural Roads: Number of projects to improve connectivity and roads (2011–2014);
Sanitation: Number of projects to improve sanitation facilities (2011–2014);
Water Conservation: Number of projects to improve water conservation (2011–2014);
Trad. Water: Number of projects to maintain traditional water bodies (2011–2014).

Table SI 2.5: Effects of Reservations on the Number of Completed MNREGA Projects, 2011-2023.

	All	Rural Roads	Sanitation	Water Conservation	Traditional Water
	(1)	(2)	(3)	(4)	(5)
2005	3.62	−0.11	0.77	−0.49	0.34
	(8.25)	(0.81)	(1.80)	(0.79)	(0.33)
2010	9.60	0.78	−0.15	0.21	−0.08
	(8.75)	(0.86)	(1.91)	(0.84)	(0.35)
2015	−8.19	0.90	−2.64	−0.13	0.45
	(9.03)	(0.89)	(1.98)	(0.86)	(0.36)
2020	−8.72	−0.02	0.23	−0.07	−0.14
	(8.15)	(0.80)	(1.78)	(0.78)	(0.33)
Constant	239.58	21.16	19.58	15.36	5.48
	(9.97)	(0.98)	(2.18)	(0.95)	(0.40)
Covariates	No	No	No	No	No
Observations	2,553	2,553	2,553	2,553	2,553
Adjusted R^2	0.0001	−0.001	−0.001	−0.001	0.0001

Note: *p<0.1; **p<0.05; ***p<0.01
All - Total completed or ongoing MNREGA projects (2011–2023);
Rural Roads: Number of projects to improve connectivity and roads (2011–2023);
Sanitation: Number of projects to improve sanitation facilities (2011–2023);
Water Conservation: Number of projects to improve water conservation (2011–2023);
Trad. Water: Number of projects to maintain traditional water bodies (2011–2023).

Table SI 2.6: Effects of Reservations on the Number of Completed MNREGA Projects, 2011-2023.

	All	Rural Roads	Sanitation	Water Conservation	Traditional Water
	(1)	(2)	(3)	(4)	(5)
Reserved Once	6.31	1.98	−11.08	−0.17	0.57
	(20.45)	(2.01)	(4.46)	(1.95)	(0.82)
Reserved Twice	5.25	1.92	−11.88	−0.61	0.79
	(20.15)	(1.98)	(4.40)	(1.93)	(0.81)
Reserved Thrice	1.65	2.15	−8.19	−0.13	0.74
	(21.52)	(2.12)	(4.70)	(2.06)	(0.86)
Reserved Four Times	27.77	1.88	−1.64	−1.76	−0.39
	(37.41)	(3.68)	(8.17)	(3.57)	(1.50)
Constant	231.99	20.02	28.95	15.57	5.05
	(19.28)	(1.90)	(4.21)	(1.84)	(0.77)
Covariates	No	No	No	No	No
Observations	2,553	2,553	2,553	2,553	2,553
Adjusted R^2	−0.001	−0.001	0.002	−0.001	−0.001

Note: *p<0.1; **p<0.05; ***p<0.01
All - Total completed or ongoing MNREGA projects (2011–2023);
Rural Roads: Number of projects to improve connectivity and roads (2011–2023);
Sanitation: Number of projects to improve sanitation facilities (2011–2023);
Water Conservation: Number of projects to improve water conservation (2011–2023);
Trad. Water: Number of projects to maintain traditional water bodies (2011–2023).

SI 3 Uttar Pradesh

Table SI 3.1: Effects of Reservations on the Number of Completed MNREGA Projects, 2011-2014

	All	Rural Roads	Sanitation	Water Conservation	Traditional Water
	(1)	(2)	(3)	(4)	(5)
2005	−0.45	0.34	−0.45	−0.03	0.01
	(0.68)	(0.15)	(0.56)	(0.07)	(0.04)
2010	−0.05	0.33	−0.50	0.06	0.001
	(0.73)	(0.16)	(0.59)	(0.07)	(0.04)
Constant	47.06	15.82	10.71	2.65	0.75
	(0.53)	(0.12)	(0.43)	(0.05)	(0.03)
Covariates	No	No	No	No	No
Observations	21,627	21,627	21,627	21,627	21,627
Adjusted R^2	−0.0001	0.0003	−0.0000	−0.0000	−0.0001

Note: *p<0.1; **p<0.05; ***p<0.01
All - Total completed or ongoing MNREGA projects (2011–2014);
Rural Roads: Number of projects to improve connectivity and roads (2011–2014);
Sanitation: Number of projects to improve sanitation facilities (2011–2014);
Water Conservation: Number of projects to improve water conservation (2011–2014);
Trad. Water: Number of projects to maintain traditional water bodies (2011–2014).

Table SI 3.2: Effects of Reservations on Demand for Work and Women Employment via MNREGA, 2011-2014 (Phase 1 and 2, Bose and Das Sample)

	Ph. 1 Jobcards	Ph. 1 Workers	Ph. 1 Women	Ph. 2 Jobcards	Ph. 2 Workers	Ph. 2 Women
	(1)	(2)	(3)	(4)	(5)	(6)
2005	−0.04	−0.03	−0.02	−0.09	−0.08	−0.08
	(0.02)	(0.03)	(0.04)	(0.03)	(0.06)	(0.05)
2010	0.05	0.04	0.04	−0.03	−0.07	−0.06
	(0.02)	(0.03)	(0.04)	(0.03)	(0.06)	(0.06)
Constant	7.25	7.39	6.25	6.90	7.34	6.49
	(0.01)	(0.02)	(0.03)	(0.02)	(0.05)	(0.04)
Covariates	No	No	No	No	No	No
Observations	2,137	2,137	2,137	1,047	1,047	1,047
Adjusted R^2	0.004	0.0001	−0.0004	0.01	0.001	0.001

Note: *p<0.1; **p<0.05; ***p<0.01
Total Jobcards (2011–2014);
Registered Workers (2011–2014);
Registered Workers Women (2011–2014);

Table SI 3.3: Effects of Reservations on Demand for Work and Women Employment via MNREGA, 2011-2014

	Tot. Jobcards	Reg. Workers	Reg. Women	Log Tot. Jobcards	Log Reg. Workers	Log Reg. Women
	(1)	(2)	(3)	(4)	(5)	(6)
2005	−48.50	−0.74	−2.42	−0.03	0.01	0.02
	(10.71)	(15.12)	(7.14)	(0.01)	(0.01)	(0.02)
2010	7.23	17.75	5.97	0.01	0.02	0.02
	(11.39)	(16.06)	(7.59)	(0.01)	(0.02)	(0.02)
Constant	1,218.18	1,712.50	663.15	6.92	7.16	6.11
	(8.28)	(11.68)	(5.52)	(0.01)	(0.01)	(0.01)
Covariates	No	No	No	No	No	No
Observations	21,627	21,627	21,627	21,627	21,627	21,627
Adjusted R^2	0.001	−0.0000	−0.0001	0.0004	0.0000	0.0001

Note: *p<0.1; **p<0.05; ***p<0.01
Total Jobcards (2011–2014);
Registered Workers (2011–2014);
Registered Workers Women (2011–2014);