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Women's political leadership and economic empowerment: Evidence from public works in India

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ABSTRACT

Despite recent advances, women trail men in political participation, especially in developing countries where the long-term economic benefits from empowering women politically have not been well-researched. We use data from 163 villages of 12 main Indian states to explore whether requiring that village leadership positions be held by women (political reservation) affected uptake of economic opportunities via the National Rural Employment Guarantee Scheme. Reservation triggered increases in women's demand for work, program participation, and access to financial services that were sustained beyond the period of female political leadership. Enhanced female participation in program oversight, civic engagement, and electoral participation are plausible channels for such effects and political and economic empowerment seem to be complementary.

1. Introduction

India's National Rural Employment Guarantee Scheme (NREGS) is the largest workfare program globally and one of the government's main efforts to reduce the vulnerability of poor people to shocks, directly by paying them wages and indirectly by creating productive local assets. An impressive array of studies has found that NREGS has had positive and far-reaching impacts: The program improved wages especially for women (Azam, 2012) and in the dry season (Imbert and Papp, 2015); it also increased consumption by the poor (Bose, 2017) and reduced short-term migration (Imbert and Papp, 2015). Access to predictable income allowed beneficiaries to choose income generation strategies that had higher risk-return profiles (Gehrke, 2017). Beyond direct program impacts, spillover effects were significant (Muralidharan et al., 2017).

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Yet the literature also suggests that the generally positive performance hides considerable heterogeneity of subnational outcomes (Sukhtankar, 2016). Much of the evidence of positive NREGS impact on access to economic opportunities and female empowerment is from states with good governance and strong institutions that often put in place additional controls, e.g., use of electronic payments (Muralidharan et al., 2016) or social audits (Afridi and Iversen, 2014). In underperforming states, leakage and rationing often undermined the program's effectiveness, as in Orissa where corrupt schemes ensured that 75–80% of total program cost went to 'ghost workers' (Paul Niehaus and Sukhtankar, 2013). Rationing at both national (Desai et al., 2015) and state (Ravallion et al., 2015) levels has been pervasive. Part of the variation in performance can be attributed to political factors that affect NREGS funding patterns in states (Chopra, 2015) and districts (Gupta and Mukhopadhyay, 2016). At the same time, villages have enormous discretion about such aspects of their NRGES programs as publicity, identification of potential beneficiaries, worker selection, work scheduling, monitoring, ensuring payment, and budgeting. This implies that the quality type of local leadership could affect the success of the program, its economic impact, and, if effects are sustained over time, long-term welfare.

This paper explores whether empowering women politically by random reservation of leadership positions in the *gram panchayat* (village council) increases women's workfare participation and thus improves their economic outcomes and well-being. We also explore whether the monetary and nonmonetary benefits from NREGS employment enhance women's political participation through involvement in NREGS governance structures like social audits and voicing concerns in local decision-making bodies. Our analysis is inspired by studies on the longer-term impact of reserving village leadership positions for women in terms of providing goods they value (Chattopadhyay and Duflo, 2004), improving the quality of service delivery (Bhalotra and Clots-Figueras, 2014); breaking gendered stereotypes (Beaman et al., 2012); contributing to lasting changes in deep-rooted social norms regarding domestic violence (Iyer et al., 2012); and the status of girls compared to boys (Kalsi, 2017).

We trace individual level outcomes beyond the period in which reservation was effective by matching individual data from the Rural Economics and Demographic Survey (REDS) to village reservation history. Doing so allows us to add to the literature in several ways: We show that the effects of reservation differ by gender and are sustained over time. In fact, higher female participation in NREGS due to leadership reservation for women may be at the expense of male involvement in the program. Such effects would be missed if only household data were available. Also, most effects persist beyond the immediate reserved period as point estimates for effects are often higher than in the reserved period. For example, past reservation increased women's participation in NREGS by more than 50% and doubled the share of women opening bank accounts.

Moreover, detailed data on program implementation allow us to show that, beyond expanding women's participation in NREGS, their leadership, possibly reinforced by the resources and social skills women acquired through economic empowerment, triggered more involvement of women not only in NREGS governance through social audits but also their civic and political engagement generally. Having been exposed to reservation in the past almost eliminated the need to bribe officials to participate in the program. Well beyond what could be explained by incumbency effects, the share of women candidates for the leadership position in non-reserved villages was 55% if the villages had previously been reserved but just 14% if they had not.

The fact that most studies focus on individual states -often high performers that invested in technology to increase NREGS transparency- could raise concerns about results being biased upward or having limited external validity. Our sample covers 12 states spread over the entire nation, thus allaying such concerns. As tests also reject the notion of female leadership having affected local council budget envelopes, the share of resources spent on capital vs. labor, wage rates, or the share of households with job cards, we can be more confident that it is indeed female leadership rather than changes in the program that drive our results.

In what follows, Sections 2 and 3 provides institutional background and reviews the literature on impacts of political reservation, the genesis and impact of India's NREGS, and our empirical strategy. Section 4 describes data and descriptive evidence as well as results from household regressions on NREGS participation. Section 5 presents the results from gender-differentiated regressions to assess the effects of current or past reservation on program access, use, and participation in governance and discusses potential mechanisms as well as robustness checks. Section 6 concludes by highlighting the substantive implications of this research for the study of political transformation.

2. Context and background

While a large and growing literature discusses short- and longer-term impacts of randomly reservation of specific village leadership positions for women on political empowerment and female participation in decision-making, the way in which such political empowerment may interact with economic variables has not been extensively studied. We review the evidence on effects from reservation as well as NREGS and argue that, as a flagship program to reduce poor Indians' vulnerability to shocks -directly by paying wages to those requesting work under the program and indirectly by creating local assets- implementation of NREGS provides an ideal opportunity to do so, discuss ways in which economic and political empowerment could interact, and explain our empirical approach to doing so.

2.1. Political reservations: a means for economic empowerment?

To overcome longstanding discrimination against females, reservation of leadership positions in village councils for women and scheduled castes (SCs) or tribes (STs) is a key component of decentralization efforts in India as originally introduced in India's 73rd Constitutional Amendment in 1993. The share of seats reserved for women is fixed at state level and, unlike seats that are reserved for

SCs or STs,[1] the seats to be reserved for women are selected randomly in every election. The literature shows how, by affecting the nature and quality of public services, allowing women to gain experience, and changing social norms and political participation more broadly, this measure had far-reaching impacts.

Female leadership can change the nature and quality of public goods supplied locally: In West Bengal and Rajasthan, female policy makers came to power via quotas related to public goods such as water and roads that women value (Chattopadhyay and Duflo, 2004). Female leadership reservation was associated with higher child survival and more breastfeeding and immunization (Bhalotra and Clots-Figueras, 2014). There is also evidence of improved learning outcomes in primary school when children were exposed to reservation in utero and very early in life (Pathak and Macours, 2017).

One factor affecting the quality of public service delivery is the capacity of women leaders.[2] In Andhra Pradesh, villages where reservation brought in women leaders from upper castes—but not those where they were from scheduled- or backward-caste women—saw a reduction in the prevalence of waterborne diseases (Dongre, 2010). The need for women leaders to gain experience in administration (Afridi et al., 2017), the maturity of the political system (Ban and Rao, 2008; Besley et al., 2004; Raabe et al., 2009), and the degree of electoral competition (Bardhan et al., 2010) have been identified as explanatory factors.

In the long term, exposure to female leadership can also alter social norms. Reserved female leaders acting as role models triggered higher school enrollment of adolescent girl, especially those from poorer and less- educated households (O'Connell, 2018) It narrowed gender gaps (Beaman et al., 2012), improved female labor force participation (Duflo, 2005; Iyer et al., 2012), and raised girls' educational attainment and aspirations. Changes in beliefs regarding gender roles and women's voices due to exposure to female leaders are argued to be central reasons for increased survival of higher-birth = order girls if local seats were reserved for women (Kalsi, 2017).

To the extent that it changes attitudes, female leadership reservation should affect political participation, as was indeed observed in West Bengal (Beaman et al., 2012), South India (Besley et al., 2004), and urban Mumbai (Bhavnani 2009).[3] Competitive election of women to state legislatures, on the other hand, was not associated with entry of new female candidates, higher female voter turnout, or effects in neighboring areas. Instead, female incumbents running for re-election blocked new entries (Bhalotra et al., 2018).[4]

2.2. Genesis and economic impact of NREGS

With cumulative spending of US$ 44.4 billion in 2005–15, India's Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS) is the largest workfare program in the world. Building on the country's long tradition of food-for-work schemes (Dutta et al., 2012; Subbarao, 1997), it guarantees up to 100 days of employment per year to households that have registered locally and established eligibility by obtaining a job card.[5] Unskilled labor supplied by locals is expected to build productive assets (access roads, water harvesting structures, etc.) in ways that explicitly encourage female participation by paying equal wages to men and women and requiring that a minimum share of work be performed by women.[6]

The fact that the program was phased in across districts has allowed researchers to assess aggregate NREGS impacts,[7] and major program-induced effects have been confirmed in three areas.[8] First, NREGS increased wages, especially for women (Azam, 2012), in the dry season (Imbert and Papp, 2015), and for the unskilled (Berg et al., 2014).[9] Second, by providing a predictable source of income, it helped reduce seasonal short-term migration (Imbert and Papp, 2015), encouraged diversification of cropping patterns (Gehrke, 2017), and improved agricultural productivity (Deininger et al. 2016). Finally, because the program is self-targeting, the distributional effects have been largely positive: For the poor NREGS enhanced consumption (Bose, 2017) and asset accumulation (Deininger and Liu, 2013). It also affected health positively (Ravi and Engler, 2015), female empowerment (Afridi et al., 2013), primary school participation (Islam and Sivasankaran, 2015), primary learning outcomes (Mani et al., 2014), though not secondary l (Shah and Steinberg, 2015), and reduced gender-based violence (Amaral et al., 2015).

The vast variation by state in program quality gives rise to concerns, however. First, the general positive impacts may be driven by a few states with good institutions that put in place measures to minimize abuse and ensure transparency Sukhtankar, 2016), such as

[1] As seats are not allocated randomly and evidence suggests that politicians' incentives to allocate benefits along party lines may blunt such quotas' distributive effects (Dunning, Thad, Nilekani, Janhavi, 2013), we do not deal with this in detail and instead referred readers to (Kaletski, Elizabeth, Prakash, Nishith, 2016) and (Chin, Aimee, Prakash, Nishith, 2011) for further discussion.

[2] Note that for a large effort of affirmative action to improve access to academic programs no evidence of such adverse impacts is found (Bagde et al. 2016), presumably due to a larger pool of potential applicants and the possibility to prepare in advance.

[3] Length of exposure to women politicians is also linked to more formal sector entrepreneurship (Ghani et al. 2013).

[4] Mixed evidence of the persistence of the effects of female leadership in India is mirrored by results in U.S. states (Broockman, David E., 2014).

[5] Applicants are eligible to receive a job card containing photos of all adult household members free within 15 days of application. The indicative work demands by job-card holders lead to elaboration of an annual plan that, once ratified by the village assembly, is transmitted for consolidation at the district level, although in practice a more top-down process is often followed, based on central budget allocations.

[6] UNDP's gender inequality index, based on 2008 data, ranks India 122 out of 138 countries, below Rwanda (83), Lao PDR (88), Egypt (108), Moldova (38), and China (40).

[7] The program was launched with 200,districts in February 2006, adding 130 in April 2007 and 285 in April 2008, with roll-out in three phases from the poorest to the least poor districts, based on a poverty index developed by the National Planning Commission at the time.

[8] Analytically, identification was achieved by relying either on time variation in program roll-out or use of panel data for individual states, often those where implementation quality was higher (Sukhtankar, Sandip, 2016).

[9] This finding is not universal; net creation of new employment may have been more limited or nonexistent (Zimmermann, Laura, 2015).

electronic payments (Muralidharan et al., 2016) and social audits (Afridi and Iversen, 2014). This is consistent with findings that political factors affected program quality at state (Chopra, 2015),[10] district (Gupta and Mukhopadhyay, 2016),[11] and village level (Das, 2015) and that leakage and rationing were widespread. In Orissa, payments to ghost workers accounted for 75–80 percent of total program cost, although expectation of an ability to extract rents in the future may also have limited theft—the 'golden goose effect' (Paul Niehaus and Sukhtankar, 2013) —and the marginal rate of corruption was close to 100% (Paul Niehaus and Sukhtankar, 2013). It was found that more than 70% of India's poor, though willing to work, did not get NREGS employment due to supply-side rationing (Desai et al., 2015). A second concern was that work generated through NREGS may have displaced existing activities directly, as in Bihar (Dutta et al., 2012), or indirectly by affecting labor supply, profits, and productivity in private factories, which then may have shifted to more capital-intensive production techniques and reduced employment (Agarwal et al., 2017).[12]

2.3. Interactions between economic and political empowerment

While political reservations may allow women to benefit from NREGS more effectively, the economic empowerment resulting from the associated resource transfers may contribute to persistence of reservation-induced effects. To see why, we note that, within the parameters set by states and districts, village councils have wide discretion in how they discharge their far-reaching NREGS responsibilities. They decide on such parameters as (i) the type of works to be performed, by prioritizing and approving lists of potential projects and assigning their budgets; (ii) scheduling of projects and provision of services (e.g., child care) mandated by law; (iii) worker selection and assignment to specific work sites; and (iv) supervision of projects, including financial management and wage payment. Exogenous reservation of council leadership positions for women together with the councils' far-reaching discretion in carrying out NREGS projects thus provides a strategy for testing whether reservation affects outcomes immediately or in the longer term.

Three channels for such impacts to coma bout are of particular interest: First, if women who came to occupy their seats through reservation cater to voters with distinct preferences (e.g., for water supply and sanitation, as suggested elsewhere in the literature) and council leaders can allocate funds to specific types of work, female leadership could shift the NREGS project portfolio toward types of activities that may be more aligned with women's preferences. Second, if women leaders act as role models and interact with different social networks than the male leaders they replace, female reservation may induce shifts in awareness of and participation in NREGS activities. As a result, participation in program oversight and transparency of program implementation may change. Third, if NREGS participation provides women with income and expands local economic opportunities, it may permanently change attitudes or behavior by triggering positive feedback loops, e.g., via women's ability to take risks or bargain within the household or community and participate in civic processes and political decision-making, which could help program-induced changes to persist even after the original reservation has lapsed.

Although intuitive, available studies do not fully support this reasoning.[13] In Andhra Pradesh, NREGS implementation was less efficient and leakage higher if village council leadership was reserved for women, a finding explained by their limited formal education and experience (Afridi et al., 2017). Similarly, aggregate administrative data from 5850 village councils in Uttar Pradesh suggest that female reservation changed the type of NREGS work undertaken and the demand for employment, but not outcomes (Bose and Das, 2018).[14]

3. Econometric approach

To identify the impact of political preference on women's economic empowerment, we use the fact that, in each period, a predetermined share of villages is randomly chosen to have the leadership (*Pradhan*) position reserved for a woman.[15] In most states, the local government had been recently elected when NREGS was launched. New elections were held in 2010 or 2011 and village council leaders elected then had just completed their terms when our data were collected.[16] We thus estimate the impact of current or past leadership reservation, allowing for the possibility that its full effect may materialize only with a time lag (Beaman et al., 2012).

[10] Key factors of commitment are ((1) the locus of the initiative; (2) the degree of analytical rigor applied to understanding the context and causes of failure; (3) mobilization of constituencies of stakeholders in support of implementing the policy; (4) application of credible sanctions in support of program objectives; and (5) continuity of policy effort.

[11] In districts where the relevant member of Parliament (as one of the members of the body approving fund allocations), is from the Congress party, more funds were allocated to blocks in which this party had fewer seats with a 1 pp lower seat share estimated to be associated with an increase in the amount of funds by 1.5 pp.

[12] The argument of Alik-Lagrange et al. (2015) that failure to adjust for the unpleasantness of NREGS work would overestimate the program's impact goes in the same direction.

[13] Ghan et al. (2013) suggest that longer exposure to female political representation at state or district level increases women's participation in he labor force n and the share of NREGS work allotted to women.

[14] Electoral information from Dunning and Nilekani (2013) is combined with data on spending and works from the NREGS MIS website.

[15] In 2009/10 all states in our sample except Bihar and Madhya Pradesh (where the share was 50%) required a third of villages to reserve the *pradhan* position for a woman. By 2015 all except Haryana and Uttar Pradesh had increased the share of *panchayats* required to reserve seats for women to 50%. Whatever the overall share, because a village's reservation status is exogenously given it does not affect our analysis. For a detailed discussion of how randomization is implemented see Dunning and Nilekani (2013) and Chattopadhyay and Duflo (2004).

[16] See Table A1 for a graphical summary of survey timing relative to state local government elections and the NREGS rollout in 2006 and 2007.

Letting v denote villages, i individuals, and t time, we assess the impacts of female reservation on outcome variables relating to individual i's NREGS participation and the other outcome variables by estimating the following equation.

$$Y_{iv} = \beta_0 + \beta_1 R_v^1 + \beta_2 R_v^2 + \beta_3 \boldsymbol{X}_{iv} + \beta_4 \boldsymbol{V}_v + u_d + \varepsilon_{iv} \quad (1)$$

where Y_{iv} is the outcome variable of interest, R_v^1 is an indicator variable that equals one if council leadership in village v was reserved for women in the most recent election (i.e., the *pradhan* at the time of the survey was a woman who assumed her position as a result of reservation) and zero otherwise; R_v^2 is an indicator variable that equals one if council leadership in village v had been reserved for a woman in the previous election and zero otherwise ;[17] $\boldsymbol{X}$ is a vector of household and individual controls ;[18] $\boldsymbol{V}$ is a vector of village and *pradhan* characteristics; u_d a district fixed effect; and ε_{iv} an error term. Our main interest is in β_1 and β_2, the parameter estimates of current or past reservation on household outcomes relative to the base category of a village never having been reserved.

To explore the gender dimension of reservation using individual data, we let f_{iv} be an indicator variable taking a value of one if the respondent is female and zero otherwise. With interactions between respondent's gender and current or past reservation, our estimating equation becomes:

$$Y_{iv} = \beta_0 + \beta_1 R_v^1 + \beta_3 \{R_v^1 \times f_{iv}\} + \beta_2 R_v^2 + \beta_4 \{R_v^2 \times f_{iv}\} + \beta_5 \boldsymbol{X}_{iv} + \beta_6 V_v + u_d + \varepsilon_{iv} \quad (2)$$

where parameters are as above and the main difference from other studies is that the parameters estimated are gender-specific, i.e., β_1 and β_2 are the estimated impact of current or past reservation on men and $\beta_1 + \beta_3$, $\beta_2 + \beta_4$ are estimated impacts of current and past reservation on women; the impact on households of current reservation is thus $\beta_2 + \beta_4$. The significance of linear combinations of estimated parameters can be tested via F-tests. This, for example, allows us to test if insignificant results at household level could be due to impacts on males and females being of similar magnitude but opposite sign so they would cancel out.

4. Data, descriptive evidence and household-level regressions

Balance between reserved and non-reserved villages in covariates not affected by the program supports the notion of random assignment having been followed. Descriptive statistics suggest reservation empowered leaders with less formal education, resulting in significantly increased demand for and access to NREGS together with an application process that was easier to navigate and more transparent. Household-level regressions point towards reservation triggering increased female NREGS participation, justifying detailed econometric investigation of gender-differentiated impacts and their persistence.

4.1. Data and characteristics at village, household, and individual level

To explore possible links between political and economic empowerment, we use individual data from a complete enumeration in 2014/15 of all adult residents in 163 villages in 12 states part of the National Council for Applied Economic Research's (NCAER) long-running ARIS-REDS panel.[19] Information was collected on about 213,000 individuals in 69,206 households, of which 17,948 had a job card that allowed household members listed on it to apply for NREGS work. For households with a job card, member data was collected on such details as (i) whether an individual's name was on the job card; (ii) his or her desired and actual participation in NREGS; (iii) issues encountered at different stages in the process; and (iv) participation in program governance and local political decision-making.[20] Individual and household schedules were complemented by a detailed village questionnaire that, in addition to standard demographic, social, and economic characteristics, elicited information on timing, candidates, participation, outcomes, and reservation status of all village council elections from 2005 through 2015 and the characteristics of elected officials. Finally, as data from the official management information system (MIS) differed markedly from local records, NREGS employment and spending data consisted of digitized e-level records for all years since the program began.[21]

To the extent that, as stipulated, villages to be reserved were chosen randomly, covariates not affected by the program should be balanced between reserved and non-reserved villages so that differences in program-related outcomes can be interpreted as causal. To check this empirically, Table 1 documents household, individual, and village characteristics for all sample villages (col. 1) as well as those ever- (col. 3) or never- (col. 5) reserved and the p-value of a t-test for equality of means between them (col. 7). Despite the power implied by the size of the sample, p-values do not allow us to reject the hypothesis that relevant variables were balanced

[17] To illustrate: R_v^1 for villages in Andhra Pradesh equals one if, in this village, the 2011 election was reserved for a woman and R_v^2 equals one if in this village the 2006 election had been reserved. Similarly, for villages in Orissa R_v^1 and R_v^2 equal 1 if the 2012 or 2007 elections were reserved.

[18] Controls include the individual's gender, marital status, age, and education (and squared terms); households' size, composition, land ownership, and the head's marital status, gender, age, and education; access to road, distance to town and district HQ, population, share of SCs, STs, and key religions; years since the last village election; characteristics of the *pradhan,*(education, caste, religion, previous tenure and candidacy for office. To the extent that reservation is not only random but also implies systematic changes *pradhan* attributes such as lower levels of educational qualifications, one may not want to control for such covariates. Results from doing so do not substantively differ from those reported here and are reported in tables in the online appendix.

[19] Following the original survey of 4500 households in 252 villages of 16 states in 1971, subsequent rounds took place in 1982, 1999, and 2006. While resource limitations precluded expansion of this exercise to all states, villages in the states of Andhra Pradesh, Bihar, Chhattisgarh, Haryana, Jharkhand, Madhya Pradesh, Rajasthan, Tamil Nadu, Uttar Pradesh, Maharashtra, Orissa and West Bengal were revisited in 2014/15.

[20] See the online appendix for questions about program participation.

[21] For detailed information see https://nrega.nic.in/netnrega/mgnrega_new/Nrega_home.aspx.

Table 1
Summary statistics of household, individual and village characteristics by reservation status.
Source: Own computation from 2014/15 REDS village census data.

	Total		Ever Reserved		Never Reserved		p-value
	Mean	SD.	Mean	SD	Mean	SD	
Panel A: Household characteristics							
Female household head	0.117	0.322	0.114	0.318	0.120	0.325	0.172
Head's age	49.433	12.785	49.382	12.881	49.484	12.689	0.581
Head's education (years)	3.84	4.14	3.798	4.114	3.881	4.165	0.165
Married head (percent)	0.844	0.363	0.840	0.367	0.848	0.359	0.136
Household size	4.62	2.309	4.653	2.378	4.587	2.239	0.049
Hindu	0.915	0.279	0.915	0.278	0.914	0.280	0.739
SC/ST	0.407	0.491	0.405	0.491	0.408	0.491	0.702
Number of observations. (households)	17,948		9534		8414		
Panel B: Individual characteristics							
Share female	0.491	0.500	0.492	0.500	0.491	0.500	0.705
Age	39.913	15.729	39.896	15.743	39.929	15.715	0.799
No education but literate	0.396	0.489	0.394	0.489	0.398	0.489	0.321
Up to primary	0.198	0.398	0.197	0.398	0.199	0.399	0.579
Up to HS	0.280	0.449	0.280	0.449	0.280	0.499	0.853
Up to university/college level	0.110	0.313	0.112	0.316	0.108	0.310	0.081
Others	0.017	0.128	0.017	0.129	0.016	0.126	0.545
Number of observations. (households)	53,612		28,336		28,276		
Panel C: Village characteristics							
Panel C1: Village structure							
Village population	2787	2786	2386	1729	3202	3532	0.061
Number of households	499	550	424	327	576	705	0.079
Share of SCs	0.216	0.168	0.204	0.152	0.231	0.183	0.338
Share of STs	0.096	0.216	0.101	0.235	0.09	0.194	0.745
Share in agriculture	0.588	0.271	0.618	0.27	0.555	0.27	0.16
Share Hindu	0.902	0.208	0.900	0.218	0.904	0.197	0.916
Distance. to nearest town (km)	15.24	15.279	15.077	16.91	15.41	13.486	0.89
Distance to district HQ (km)	46.404	27.907	47.564	27.628	45.2	28.318	0.59
Share with good road access	0.577	0.496	0.566	0.499	0.587	0.495	0.785
Previously contested position	0.147	0.355	0.133	0.341	0.162	0.371	0.592
Panel C2: Pradhan characteristics							
Held gov't position before	0.429	0.497	0.422	0.497	0.438	0.499	0.840
Up to primary school	0.27	0.445	0.361	0.483	0.175	0.382	0.007
Up to high school	0.264	0.442	0.253	0.437	0.275	0.449	0.752
Higher secondary and above	0.344	0.476	0.265	0.444	0.425	0.497	0.032
Higher education and professional	0.123	0.329	0.12	0.328	0.125	0.333	0.930
SC	0.601	0.491	0.639	0.483	0.563	0.499	0.325
ST	0.074	0.262	0.084	0.28	0.063	0.244	0.596
OBC	0.110	0.314	0.108	0.313	0.112	0.318	0.935
OC	0.209	0.408	0.169	0.377	0.25	0.436	0.204
Hindu	0.509	0.501	0.41	0.495	0.613	0.490	0.009
Muslim	0.092	0.290	0.096	0.297	0.087	0.284	0.846
Other	0.398	0.491	0.493	0.503	0.300	0.461	0.011
Panel C3: Village NREGS implementation							
Employment days per person	38.337	54.483	41.964	50.302	34.575	53.707	0.388
Share of employment in							
…water conservation	0.445	0.537	0.466	0.583	0.421	0.464	0.282
…irrigation	0.039	0.154	0.032	0.086	0.046	0.216	0.791
…land development	0.114	0.149	0.108	0.148	0.12	0.15	0.829
…rural connectivity	0.295	0.349	0.269	0.317	0.325	0.39	0.884
…others	0.108	0.291	0.125	0.325	0.088	0.235	0.35
Number of observations (villages)	163		83		80		

Note: p-values are for tests of the equality of means between ever- and never-reserved villages. SD = standard deviation. See appendix Table A3 for descriptive statistics on all household characteristics included in regressions.

between ever- and never-reserved villages, in line with the random selection of villages for reservation. With an average household size of 4.6 individuals, 12% of households are headed by a woman, 84% of heads are married, and 91.5% are Hindus. Individuals are on average 40 years old; about 40% are literate but uneducated, 20% have educational levels less than primary; 28% up to high school, and 11% up to college.

The village variables in Table 1 (panel C1) point to a mean village population of about 500 households or 2787 individuals, mostly Hindu (90%), of whom about one third belong to either an SC (22%) and an ST (10%). Almost 59% of the population relies on agriculture as the main source of income; the average distance to the next town is 15 km and to district headquarters 46 km; and about 58% of villages have access to a good road. We thus cannot reject the hypothesis of no significant differences in socioeconomic characteristics between ever- and never-reserved villages on conventional terms.

Table 2
NREGS participation by reservation status.
Source: Own computation from 2014/15 REDS village census data for households holding a job card and individuals in such households only.

	Total		Ever reserved		Never reserved		p-value
	Mean	SD	Mean	SD	Mean	SD	
Panel A: Households							
Households has job card[29]	0.252	0.434	0.302	0.459	0.216	0.411	0.000
For those with job card:							
Share worked in NREGS	0.469	0.499	0.547	0.498	0.392	0.488	0.000
Days under MNREGS	57.125	37.533	61.314	39.184	51.306	34.278	0.000
Days worked by men	17.589	24.491	17.041	25.327	18.351	23.262	0.013
Days worked by women	39.536	37.610	44.273	40.470	32.955	32.096	0.000
Ratio of days worked by women to total	0.637	0.384	0.660	0.383	0.606	0.384	0.000
Completed 100 days	0.154	0.361	0.195	0.396	0.098	0.297	0.000
NREGS income (Rs.)	8144	11,345	7749	54,325	8692	16,325	0.698
Number of observations.	17,948		9534		8414		
Panel B: Individuals							
B.1 Demand for work and procedural compliance							
Ever wanted NREGS work	0.280	0.449	0.306	0.461	0.253	0.435	0.000
If no:							
Wage too low	0.512	0.500	0.526	0.499	0.497	0.500	0.000
Site too far/no work	0.062	0.241	0.062	0.241	0.062	0.241	0.935
Domestic duties	0.113	0.317	0.116	0.320	0.111	0.314	0.038
Too young or old, or student	0.073	0.260	0.078	0.268	0.068	0.252	0.000
Household did do not have job card	0.147	0.354	0.120	0.325	0.175	0.380	0.000
Have other job	0.093	0.291	0.099	0.299	0.087	0.282	0.000
B.2 Job applications and actual work							
Getting a job was tedious	0.028	0.164	0.024	0.154	0.031	0.172	0.000
Made formal job application	0.296	0.456	0.319	0.466	0.273	0.445	0.000
If yes,							
Application documented	0.694	0.461	0.699	0.459	0.688	0.463	0.102
Mot unemployment allowance	0.021	0.145	0.020	0.141	0.023	0.149	0.066
Worked for NREGS	0.232	0.422	0.265	0.441	0.199	0.399	0.000
If yes, bribe demanded	0.018	0.132	0.014	0.116	0.022	0.146	0.000
No NREGS days worked per year	38.914	31.417	42.420	34.394	34.219	26.198	0.000
More days desired	0.289	0.454	0.268	0.443	0.313	0.464	0.000
if yes, how many	20.922	35.188	18.077	28.407	24.008	41.081	0.000
NREGS wage	131.335	529.112	129.578	386.54	133.683	674.20	0.660
B.3 Social and economic connectedness							
Have bank savings account	0.595	0.491	0.566	0.496	0.624	0.484	0.000
if yes, opened via NREGS	0.296	0.456	0.319	0.466	0.272	0.445	0.000
Post office savings account	0.102	0.302	0.113	0.316	0.091	0.287	0.000
Ever participated in social audit	0.040	0.197	0.050	0.218	0.030	0.171	0.000
Number of observations.	53,612		28,336		28,276		

Note: P-values are from testing equality of means between ever and never reserved villages. SD = standard deviation.
[29] The total number of households in the sample is 69,206, of which 17,948 had a job card/.

While data on village characteristics supports the notion that reservation was allocated randomly, data on *pradhan* characteristics suggest reservation resulted in election of non-Hindu village leaders with less formal education: in never- reserved villages the shares of *pradhans* with primary education or less were 18%, 43% had higher secondary or more education, and 61% were Hindu; in ever-reserved villages, 36% had primary or less, 28% had higher secondary and above, and 41% were Hindu. Reservation was not associated with more individuals contesting leadership positions or who had held government positions before being elected (Table 1 panel C2).

Data on how NREGS was implemented (Table 1 panel C3) also point towards some significant differences: program-related household employment per year was slightly higher in ever-reserved villages (at 42 days vs. 35 days in never-reserved villages), and villages reserved in the past spent more NREGS time on water- than on connectivity-related activities, though this is not significantly different from the 45% spent on water-related activities and the 30% spent on connectivity-related activities generally.

4.2. Data on NREGS participation

Household- and individual data on job card holders, demand for work, participation, and social as well as economic connections in Table 2 allow going beyond the suggestive evidence of reservation-induced effects on program implementation discussed above. They suggest that reservation affected household program demand and participation. The same is true for the ability of individuals to access NREGA work, the extent to which prescribed procedures were adhered to, and the participation of individuals in financial markets and social audits. With some 30% of households in ever-reserved villages and 22%in never-reserved villages possessing job cards, reservation broadened the range of those able to register and thus become eligible for program participation (Table 2, panel A).

Among individuals with job cards, participation was much higher in reserved villages at 55% than the 39% in never reserved villages.

The number of days worked (61 in ever- vs. 51 in never-reserved villages), the share of female participation (66% in ever- vs. 60% in never-reserved villages), and the share of households that exhausted the maximum allowance of 100 days (20% in ever- and 10% in never-reserved villages) all suggest that reservation significantly enhanced the scope for female participation. While daily wages do not differ between the two types of villages, rationing and unsatisfied demand are higher in never-reserved villages, with 31% of individuals stating they would like to have worked more; and 27% of ever-reserved villages had unsatisfied demand. As 32% of job card holders in ever-reserved villages made a formal application and 27% of these worked in an NREGS project (in never-reserved villages 27% made a formal application and 20% worked on an NREGS project), reservation appears to have made it easier to convert demand into work and income; the number of days worked per job card holder was 42 in ever- and 34 in never- reserved villages.

The data for individuals (Table 2, panel B) suggest that in reserved villages awareness of the program was higher than in never-reserved ones (31% to 25%) having ever wanted to work under NREGS and the share of these who did not want to work because of low wages (53% vs. 50%) was higher. In reserved villages, more respondents (32 to 27%) applied formally for NREGS work, the amount of days worked was higher (42 to 34), and fewer had unsatisfied demand for more work (27% vs. 31%). Also, in the never-reserved villages, getting a job was perceived to be less tedious (2.4 to 3.1%), and fewer applicants (1.4 to 2.2%) were asked for a bribe. Broader impacts of the program were more positive in reserved villages where 32%, vs. 27%, used the program to open a savings account and 5%, vs. 3%, participated in a social audit.

Although the shares are small, descriptive data also support the notion that reservation made access to the program and participation in its governance easier: in ever-reserved villages, 2.4% (vs. 3,1%) stated that getting a job was tedious and 1,4% (vs. 2,2%) said that they had to pay a bribe to get work. Also, with 5% in ever- but only 3% in never-reserved villages having participated in social audits to review program accounts and assess the quality of works under the program, participation in program governance was higher in reserved villages. Some 30% of job card holders (32 vs. 27%) filed a formal job application, and about 70% of these were documented. Case studies have found that social audits, the main tool to curb corruption and ensure accountability from below, are often ineffective (Afridi and Iversen, 2014) as participation is limited to men and local elites (Lakha et al., 2015), possibly due to greater awareness. The fact that 32% of individuals in reserved vs. 27% in never-reserved villages reported having opened a savings account to receive NREGS payments suggests that the program facilitated catch-up by groups that had been disadvantaged.[22]

4.3. Household level regressions

To provide household level evidence, we aggregate data on the amount of days worked in NREGS by the entire household or females and combine these with information on whether the household had a job card or attained the 100-day limit to run regressions at household level. One way for female leaders to affect NREGS implementation would be to provide certain households with a job card, thus altering eligibility for NREGS. Results from regressions with possession of a job card as the dependent variable (Table 3, col. 1) suggest that neither current nor past reservation of village council leadership for a woman had significant impact on household access to a job card, thus allaying such fears.

For households with a job card, there is clear evidence of a reservation-induced and persistent shift in the number of NREGS days women worked and the share of work they performed (Table 3, cols. 2 and 3, for job card holders only). Having the village leadership position reserved for a woman is estimated to have increased the number of days worked by households holding job cards by 9.7% age points (pp) and the share of days worked by women by 2.2 pp. Coefficients on past reservation (β_2) of 0.29 for days worked and 0.030 for share of female days suggest that impacts of female reservation persist and may even be larger after the female leadership requirement lapses. Descriptive statistics and village or household regressions imply, rather, that reservation did not affect how NREGS was conducted but did raise women's awareness of the program, made it easier for them to participate, and reduced rent-seeking without affecting the share of households that completed the statutory maximum of 100 days of work. This motivates us to use individual data to further explore the program's impacts.

5. Estimates of reservation-induced effects at individual level

Individual-level regressions point towards reservation-induced increases in women's awareness of and participation in NREGS as well as their ability to use formal channels to articulate demand and exercise oversight. Gender-differentiated impacts persist even after the leadership reservation lapsed, suggesting that this one-time intervention led to shifts in awareness and attitudes. The finding that past reservation improved the frequency and quality of female participation in village governance and decision-making as well as women's ability to hold officials to account provides a plausible channel for such impacts to materialize and is suggestive of important feedback loops between political and economic empowerment.

5.1. Impacts on program demand, access, and participation

If female reservation increases women's awareness of and participation in community affairs, one would expect it to result in changes in demand for NREGS work and program participation. Regression results for demand for NREGS work and the ability to

[22] With 57% in never- vs. 62% in eve- reserved having access to a savings account, access to public services in ever-reserved villages is significantly below that observed in never-reserved ones.

Table 3
Impact of political reservation on nregs involvement at household level.

	Have a Job Card	Days Worked (log)	Days worked by women	100 Days Completed
Reserved now (β_1)	0.012	0.097**	0.022**	−0.007
	(0.038)	(0.042)	(0.009)	(0.010)
Reserved before (β_2)	0.026	0.292***	0.030**	0.012
	(0.045)	(0.041)	(0.008)	(0.011)
Observations	69,206	17,948	17,948	17,948
R-squared	0.315	0.518	0.516	0.278
F-statistics ($\beta_1+\beta_2=0$)	0.300	52.13	17.10	0.0119
(p- value)	(0.586)	(0.000)	(0.000)	(0.732)
Dep. var. mean	0.252	1.873	0.637	0.1549
Standard deviation	0.434	1.976	0.384	0.362

Note: Each column reports results from a separate regression with dependent variables listed at the top of each column. Regressions are at the household level. Column 1 uses the entire sample while column 2 to 4 are conditional on the household having a job card. Each regression includes controls at household (head's gender, education, marital status, age and age squared; household size, dependency ratio, share of working-age members, and mean education level) and village (share of agricultural households, SCs, STs, and OBCs, distance to nearest town, good road availability, log of population, arable area, share of Hindu households, length of any reservation in the current or village council term; *pradhan's* age, caste, education, and prior political experience, and district fixed effects. The excluded category is 'never reserved,' standard errors are clustered at village council level, and robust standard errors reported in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 4
Impact of female political reservation on NREGS work.

	Formally Applied for Work	Worked in NREGS	Log of NREGS Days Worked	Rationing Overall	Extensive Margin	Intensive Margin
	Panel A					
Reserved now (β_1)	−0.009	−0.005	−0.030	−0.027***	0.011	−0.038***
	(0.008)	(0.006)	(0.022)	(0.008)	(0.007)	(0.005)
Reserved before (β_2)	0.025***	0.020***	0.113***	−0.039***	0.020**	−0.059***
	(0.009)	(0.007)	(0.026)	(0.010)	(0.008)	(0.007)
Observations	53,612	53,612	53,612	53,612	53,612	53,612
R-squared	0.196	0.253	0.270	0.131	0.053	0.160
F statistics ($\beta_1+\beta_2=0$)	2.028	3.207	8.267	33.100	12.080	162.700
(p value)	(0.154)	(0.073)	(0.004)	(0.000)	(0.001)	(0.000)
	Panel B					
Reserved now (β_1)	−0.044***	−0.045***	−0.164***	−0.038***	0.007	−0.045***
	(0.008)	(0.007)	(0.024)	(0.009)	(0.007)	(0.005)
Reserved before (β_2)	−0.038***	−0.031***	−0.196***	−0.066***	0.019**	−0.085***
	(0.010)	(0.009)	(0.029)	(0.011)	(0.008)	(0.007)
Reserved now × female (β_3)	0.071***	0.062***	0.277***	0.024***	0.008**	0.015***
	(0.006)	(0.006)	(0.021)	(0.005)	(0.004)	(0.004)
Reserved before × female (β4)	0.130***	0.151***	0.636***	0.055***	0.001	0.054***
	(0.007)	(0.007)	(0.028)	(0.006)	(0.004)	(0.004)
Observations	53,612	53,612	53,612	53,612	53,612	53,612
R-squared	0.202	0.268	0.284	0.132	0.053	0.162
F statistics (β_1+ β3 = 0)	11.720	4.818	21.180	2.703	4.694	27.610
(p value)	(0.001)	(0.028)	0.000	(0.100)	(0.030)	0.000
F statistics ($\beta_2+\beta_4=0$)	83.120	178.100	223.200	1.037	6.533	18.090
(p value)	(0.000)	(0.000)	(0.000)	(0.308)	(0.011)	(0.000)
F statistics (β_1+ β2 + $\beta_3+\beta_4=0$)	96.14	176.30	254.20	4.36	15.38	51.95
(p value)	(0.000)	(0.000)	(0.000)	(0.037)	(0.000)	(0.000)
Dep. var. mean	0.300	0.233	1.600	0.196	0.094	0.102
Standard deviation	0.456	0.422	1.490	0.397	0.292	0.303

Note: Each column reports results from a separate regression at individual level with dependent variables listed at the top of each column. Every regression includes household controls (head's gender, education, marital status, age and age squared; household size, dependency ratio, share of working age members, and mean education level) and village controls (share of agricultural households, SCs, STs and OBCs, distance to nearest town, good road availability, log of population, arable area, share of Hindu households, length of any reservation in the current or past village council term, and *pradhan's* age, caste, education level and prior political experience) as well as district fixed effects. Bonferroni-adjusted p-values are reported for multiple tests in bottom rows of each panel of the table. The excluded category is 'never reserved,' standard errors are clustered at village council level, and robust standard errors reported in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

access it in Tables 4A (for households) and 4B (for individuals) indeed point to such effects. They suggest that in village councils that were or had previously been reserved, demand for NREGS work was 2.3 pp (9.1%) higher and the share of people formally applying was 3.4 pp (12.5%) higher than in those never reserved (Table 4, cols. 1 and 2). Disaggregating by council term suggests that concurrent reservation had no effect on the share of individuals formally applying for work, but past reservation increased the share of those who formally applied to MREGS by 2.5 pp.

Gender disaggregation suggests that reservation increased women's demand for NREGS participation but decreased men's (Table 4B). Current reservation of leadership for a woman is estimated to have raised women's formal demand for NREGS work by 3.7 pp and past reservation raised it by 9.2 pp. The larger point estimate for past reservation is consistent with the theory that reservation helped empower women in the longer term.[23] Beyond allowing them to more effectively articulate demand for NREGS, reservation increased women's ability to access NRGES employment and reduced the extent of rationing in ways that lasted beyond the immediately reserved period.

In ever-reserved villages with randomly reserved council leadership, the share of individuals who got NRGES work was an estimated 1.5 pp higher (up 7.5%) and the estimated number of days worked was 8.3 pp higher (up 9.3%) than in never-reserved ones (Table 4, cols. 2 & 3). Interestingly, while it is impossible to reject the notion of concurrent reservation not having had any significant effect, past reservation is estimated to have increased participation by 2 pp and days worked by 11.3 pp. Persistent effects along these lines are plausible if, beyond the village leader's ability to influence NREGS-related decisions, reservation changed how NREGS was implemented at village level. Gender-disaggregated estimates suggest reservation affected men and women differently: concurrent and past reservation are estimated to have a significant negative effect on men's participation (-4.5 pp for current and -3.1 pp for past) and the number of days worked by men (-16.4 pp for current and -19.6 for past reservation) but a positive one for women, for whom we find an increase in participation of 1.7 pp increase for current and 12.2 pp for past reservation and an increase in the number of days worked by women of 11.3 pp for current and 44 pp for past reservation.

Although employment should be available to anybody interested, studies often show rationing at the extensive margin (individuals applying for NREGS work who did not get it) and intensive margin (program participants wanting to work more). Gender disaggregation shows differences in impact for men and women; rationing for men was reduced by 3.8 pp for current and 6.6 pp for past reservation, and for women, a rise in rationing at the extensive margin balanced a decrease at the intensive margin.

Results for the extensive margin (whether an individual worked) and the intensive margin (days worked) are similar: regressions suggest that having village leadership reserved for a woman reduced total rationing, at both margins, by 2.7 pp. Again, the estimated coefficients for past reservation are almost double those of concurrent reservation. This effect persisted over time; at 3.9 pp, the point estimate for past reservation is slightly higher than for current reservation. The lower incidence of rationing seems to largely result from a major drop in rationing at the intensive margin with a point estimate of 3.8 pp' with the impacts on rationing at the extensive margin being insignificant. The estimated impacts of past reservation suggest that the decrease in overall rationing by an estimated 3.9 pp consists of a 5.9 pp decrease in rationing at the intensive margin, but a 2 pp increase at the extensive margin.

5.2. Impacts on program governance

The persistence of effects along the lines observed earlier could be explained by reservation affecting how NREGS is implemented at local level. Estimates of reservation impacts on NREGS governance (Table 5) indeed suggest that reservation had significant impacts on empowering females that persist and, if anything, get stronger over time. Having village leadership reserved for a female reduced reported incidence of bribe demands by 0.6 pp for current and 0.9 pp for past reservation,[24] in line with evidence from elsewhere on the effectiveness of reporting systems on reducing corruption.[25] Interestingly, gender disaggregation is consistent with the interpretation of this effect being entirely attributable to reduction in the need to pay bribes by women -which was lower by 1.1 pp for current and 1.8 pp for past reservation. In fact, with the mean of those who reported having had to bribe officials of 1.8%, past reservation close to eliminating reported bribe taking. This is consistent with a reservation-induced reduction, by 0.4 pp for current and 0.9 pp for past reservation, in the share of women who reported that getting a NREGS job was tedious.

The literature on social audits, the government's main tool to ensure bottom-up accountability in the program, suggests that they have often been ineffective due to low beneficiary participation. Although they reject the notion of current reservation having a significant impact, aggregate estimates suggest an increase of 1 pp, equivalent to a 25% increase, in social audit participation due to past reservation. Male participation is estimated to have decreased by 0.7 pp due to current and 2.0 pp due to past reservation, and female participation to have increased by 1.3 pp for current and 4.1 pp for past reservation. While the survey lacks information on how much participation in such audits was perceived to have made an impact, the increased size of estimated impacts over time and their gender-differentiated pattern suggest that reservation reduced barriers to and enhanced the perceived benefits from women's participation in social audits.

[23] Coefficients of a similar order of magnitude for having applied formally and the application documented (Table 4A) suggest that reservation increased not only women's awareness and interest in MREGS but also their ability to use NRGES channels to articulate their demands.

[24] This is consistent with results from close mayoral elections in Brazil, where women were less likely than males to engage in corruption or hire temporary public employees during the electoral year although doing so reduced their likelihood or being re-elected (Brolloet al. 2016).

[25] Experimental evidence suggests that accountability systems where citizen reporting leads to formal punishment can be very effective in curbing corruption even if the probability of punishment is low (Serra and Danila, 2012), consistent with evidence that self-reporting platforms are most effective if they are specific and thus could serve to identify the least corrupt officials (Ryvkin et al. 2015).

Table 5
Impact of female political reservation on NREGS governance and accountability.

	Bribe Demanded	"Getting Job Is Tedious"	Participation in Social Audit	New NREGS Account
	Panel A			
Reserved now (β_1)	−0.006**	−0.002	0.003	0.042**
	(0.003)	(0.001)	(0.003)	(0.020)
Reserved before (β_2)	−0.009**	−0.003	0.010***	−0.007
	(0.004)	(0.002)	(0.003)	(0.029)
Observations	53,612	53,612	53,612	53,612
R-squared	0.107	0.040	0.146	0.240
F statistics ($\beta_1 + \beta_2 = 0$)	5.590	4.651	5.671	0.834
(p value)	(0.0181)	(0.0310)	(0.0173)	(0.364)
	Panel B			
Reserved now (β_1)	−0.002	0.001	−0.007***	0.016
	(0.003)	(0.002)	(0.003)	(0.028)
Reserved before (β_2)	−0.000	0.003	−0.020***	−0.082*
	(0.004)	(0.003)	(0.004)	(0.047)
Reserved now × female (β_3)	−0.009***	−0.005***	0.020***	0.052
	(0.002)	(0.002)	(0.003)	(0.047)
Reserved before × female (β_4)	−0.018***	−0.012***	0.061***	0.153**
	(0.002)	(0.002)	(0.004)	(0.075)
Observations	53,612	53,612	53,612	53,612
R-squared	0.109	0.041	0.152	0.247
F statistics ($\beta_1 + \beta_3 = 0$)	14.41	5.095	17.04	3.957
(p value)	(0.0001)	0.0240	(0.000)	(0.0507)
F statistics ($\beta_2 + \beta_4 = 0$)	17.54	7.017	102.8	2.161
(p value)	2.83e-05	0.00808	0.000	0.146
F statistics ($\beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$)	18.67	13.14	71.73	4.029
(p value)	1.56e-05	0.000290	0.000	0.0487
Dep. var. mean	0.018	0.030	0.040	0.300
Standard deviation	0.134	0.164	0.200	0.456

Note: Each column reports results from a separate regression at individual level with dependent variables listed at the top of each column. Every regression includes household controls (head's gender, education, marital status, age and age squared; household size, dependency ratio, share of working-age members, and mean education level) and village controls (share of agricultural households, SCs, STs and OBCs, distance to nearest town, good road availability, log of population, arable area, share of Hindu households, length of any reservation in the current or past village council term; *pradhan's* age, caste, education level and prior political experience) as well as district fixed effects. Bonferroni-adjusted p-values are reported for multiple tests in bottom rows of each panel of the table. The excluded category is 'never reserved,' standard errors are clustered at village council, and robust standard errors reported in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

By reducing the role of intermediaries and the scope for resources being made available to individuals other than the intended beneficiary, transfer of NREGS wages directly to the accounts of individual workers is often viewed as a precondition for greater transparency and accountability in program administration. This is relevant because gender discrimination in access to financial services is still widespread (Demirguc-Kunt et al., 2013). Current reservation is estimated to have increased the likelihood of opening a savings account to receive NREGS payments by 4.2 pp, with virtually all the impact caused by an estimated 6.8 pp increase in women opening accounts. Past reservation has no significant impact as this is a one-time event.

5.3. Channels for and robustness of observed impacts

The above results suggest that female leadership reservation affected NREGS outcomes and governance in ways that were sustained beyond the reservation period. In addition to women leaders serving as a role model, a possible channel for such effects could be that the tangible benefits from NREGS participation created opportunities for positive feedback loops between political and economic empowerment -reserved female leadership seem to have created opportunities for women to participate in NREGS and associated decision-making processes. The benefits from such participation, e.g., in terms of access to resources or new networks made available through the program, not only made such participation more attractive and effective but allowed it to expand to village-level decision-making processes beyond the program. While we cannot test directly for such interaction effects, changes in electoral dynamics and increased female political participation even after reservations lapsed provide strong support for this.

Results from regressions for broader political empowerment in Table 6 show that, overall, concurrent reservation improved village governance by increasing meeting attendance by 1.8 pp and the number of meetings attended by 4.6 pp. It reduced the share of individuals who did not have information on meeting agendas in advance by 6.2 pp, though it did not affect the effectiveness of meetings in solving the issues raised. Disaggregation by gender and time reveals an interesting picture; in line with the notion of aggregate effects concealing individual variations that are key to understanding overall reservation-induced impacts. For concurrent reservation, while estimated improvements in terms of meeting attendance at the intensive and extensive margin are largely limited to males, improved access to information, measured by the share of individuals who report to have access to meeting agendas before council meetings, accrues equally to men and women. One explanation may be that women leaders entering office via reservation

Table 6
Impact of female political reservation on participation in village governance.

	Attended GS Meetings (Yes/No)	Number of Meetings Attended	No information on meetings	Problems Raised Were Solved
	Panel A			
Reserved now (β_1)	0.018*	0.046**	−0.062*	−0.003
	(0.010)	(0.019)	(0.031)	(0.017)
Reserved before (β_2)	−0.008	−0.019	−0.002	−0.006
	(0.011)	(0.023)	(0.022)	(0.010)
Observations	53,612	53,612	53,612	53,612
R-squared	0.189	0.046	0.162	0.105
F statistics ($\beta_1 + \beta_2 = 0$)	0.715	1.269	2.255	0.655
(p value)	0.398	0.260	0.139	0.422
	Panel B			
Reserved now (β_1)	0.035***	0.087***	−0.060*	−0.001
	(0.013)	(0.026)	(0.033)	(0.021)
Reserved before (β_2)	−0.034***	−0.085**	−0.002	−0.003
	(0.013)	(0.035)	(0.022)	(0.014)
Reserved now × female (β_3)	−0.041***	−0.058**	0.017	−0.027*
	(0.014)	(0.029)	(0.027)	(0.015)
Reserved before × female (β_4)	0.067***	0.081**	−0.039**	0.043**
	(0.014)	(0.034)	(0.019)	(0.021)
Observations	53,612	53,612	53,612	53,612
R-squared	0.191	0.046	0.162	0.106
F statistics ($\beta_1 + \beta_3 = 0$)	0.00543	0.0107	3.775	0.0631
(p value)	0.941	0.918	0.0577	0.803
F statistics ($\beta_2 + \beta_4 = 0$)	5.110	0.735	0.927	1.464
(p value)	0.0240	0.392	0.340	0.232
F stat ($\beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$)	5.004	0.692	4.138	1.257
(p value)	0.0255	0.406	0.0473	0.268
Dep. var. mean	0.184	0.324	0.247	0.0917
Standard deviation	0.388	1.430	0.432	0.289

Note: Each column reports results from a separate regression with dependent variables listed at the top of each column. Every regression includes household controls at household (head's gender, education, marital status, age and age squared; household size, dependency ratio, share of working-age members, and mean education level) and village controls (share of agricultural households, SCs, STs and OBCs, distance to nearest town, good road availability, log of population, arable area, share of Hindu households, length of any reservation in the current or a past village council term; *pradhan's* age, caste, education level, and prior political experience) and district fixed effects. Bonferroni-adjusted p-values are reported for multiple tests in bottom rows of each panel of the table. The excluded category is 'never reserved,' standard errors are clustered at village council, and robust standard errors reported in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

expand the circle of people who participate in decision-making beyond those traditionally involved.[26]

Gender-differentiated effects also emerge regarding the impact of past reservation. While Table 6A suggests no overall impact, this seems to be a due to a reduction in intensity of male participation by about 3.4 pp (and the number of meetings attended by males by about 8.5 pp) this is compensated by an increase in attendance by women of 3.3 pp though there is no change in the number of meetings attended by women. Moreover, after the reservation period, the share of women with access to information on village meeting agendas increased by 3.9 pp and the share of women who perceived problems raised in village meetings to be solved went up by 4.3% age points. All of this is consistent with the notion that reservation, together with the implementation of NREGS, affected village governance more generally.[27]

A potential concern for our estimates is that women leaders may have altered the way in which the program was implemented, e.g. by devoting more program resources to wages and in doing so alter the nature or sustainability of work done under the program. To test whether that may have been the case, we use annual village data for 2007–13 digitized from local administrative records by enumerators who visited the village to administer the household survey in a subset of villages, to run village-level regressions of the form

$$Y_{vt} = \beta_0 + \beta_1 R^1_{vt} + \beta_2 R^2_{vt} + \beta_3 V_v + u_t + u_d + \varepsilon_{vt} \quad (3)$$

where Y_{vt} is the labor-capital ratio or the number of days of NREGS employment in village v at time t, R^1_{vt} is an indicator variable

[26] Females in currently reserved villages perceive a reduction of the effectiveness with which problems raised in such meetings were solved, possibly due to increased interest in such issues.

[27] One explanation for the observed increase in male participation during periods reserved for female leadership could be a desire to balance the leader's influence. While this tapers off once female reservation lapsed, estimated increases in female participation are consistent with long-term effects of reservation, e.g. through experience with participation in decision-making or female leaders acting as a role model.

Table 7
Estimates of capital labor ratio and days of specific types of NREGS work by reservation status.

	Total	Water cons.	Irrigation	Land development	Rural road connectivity	Others
Panel A: Capital labor ratio						
Reserved now (β_1))	177.176	50.304	−0.537	−0.544	839.239	0.438
	(187.626)	(46.926)	(5.423)	(4.224)	(970.895)	(1.479)
Reserved before (β_2)	−30.094	35.422	−2.101	4.247	−56.741	−0.261
	(50.162)	(40.551)	(3.116)	(5.139)	(231.568)	(0.663)
Observations	798	798	798	798	798	798
R-squared	0.880	0.985	0.096	0.477	0.573	0.297
F statistics ($\beta_1 + \beta_2$)	0.800	1.098	0.139	0.896	0.755	0.0101
(p value)	(0.374)	(0.298)	(0.710)	(0.346)	(0.387)	(0.920)
Dep. variable mean	1.666	1.179	1.793	3.203	3.384	0.695
Standard deviation	28,814	19,262	25.87	20.04	73,525	5.892
Panel B: Number of work days						
Reserved now (β_1)	0.148	0.164	−0.175	0.362	−0.338	−0.135
	(0.225)	(0.343)	(0.256)	(0.486)	(0.745)	(0.376)
Reserved before (β_2)	−0.348	−0.277	−0.395	−0.872*	0.174	−0.438
	(0.313)	(0.326)	(0.247)	(0.439)	(0.532)	(0.303)
Observations	798	798	798	798	798	798
R-squared	0.363	0.328	0.577	0.424	0.322	0.283
F statistics ($\beta_1+\beta_2=0$)	0.201	0.0572	2.571	0.512	0.0373	1.095
(p value)	(0.655)	(0.812)	(0.113)	(0.476)	(0.847)	(0.298)
Dep. var. mean	6.391	4.009	0.604	1.876	2.740	0.959
Standard deviation	2.558	3.482	1.837	2.967	3.490	2.430

Note: Each column reports results from a separate regression. The unit of observation is the village by year and as explained in the text, annual village-level data are used for 114 villages in 2007–13 inclusive. Village controls in every regression include share of agricultural households, share of scheduled castes, scheduled tribes, and other backward castes (OBCs), distance to nearest town, good road access, log of population, arable area, share of Hindu households, length of reservation (if reserved) in the current or past council period, and the *pradhan's* age, caste, education, and prior political experience. Year and district fixed effects are included but not reported. Standard clustered at village council level and reported in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 8
Election participation by reservation status.
Source: Author's own calculation from the survey data.

	No. of candidates	Share of women candidates	No of villages
Never reserved	8.05	0.14	80
Reserved in both periods	8.63	1.00	24
Switched from non-reserved to reserved	4.72	1.00	37
Switched from reserved to non-reserved	9.30	0.55	22

taking a value of one if in year t a village was reserved concurrently and zero if not, R^2_{vt} an indicator variable taking a value of one if in t the village was reserved in the previous village council term and zero otherwise; $\boldsymbol{V}_v$ is a vector of village and *pradhan* characteristics; u_d is a district fixed effect, u_t is a year fixed effects, ε_{vt} is a village-level error term, and β_1 and β_2 are coefficients to be estimated.[28]

Results in Table 7 suggest that neither current nor past reservation affected the capital-labor ratio or the number of days of NREGS-related work provided. A potential explanation could be the lead time required to prepare activities to enter the NREGS "project bank." Disaggregating by type of work does not allow us to reject the hypothesis the current reservation has no impact on NREGS labor demand for any of the categories used here. In villages where leadership had been reserved in the past, we note a possibly marginal shift of NREGS activity from irrigation to water conservation. This could be in line with assumptions that female leaders invest more on public goods that are aligned with female preferences (Chattopadhyay and Duflo, 2004). It supports the possibility that, beyond the reserved period, reservation may have enabled women to articulate demands more effectively and affected ways in which public resources, including those earmarked for NREGS, are spent.

Beyond regressions related to the level and effectiveness of individuals participating in village governance (council meetings, advance access to agendas, effectiveness in resolving agenda items), a simple measure of longer-term reservation-induced impacts on political participation is the share of women running for non-reserved elections. Although the number of villages is limited, descriptive statistics in Table 8 point towards significant and large effects: in villages that had previously been reserved, 55 percent of candidates were women, well above the level of 14% in villages that had never been reserved. This is not a result of elections having become less competitive; to the contrary the number of candidates (9.3) contesting *pradhan* elections in villages that shifted from

[28] Because this regression uses longitudinal data, estimated effects would be more short-term than those in Eqs. (1) and (2).

reserved to unreserved is slightly above that (8.05) in never-reserved elections and well above that (4.7) in villages that shifted from never-reserved to reserved status. Regression results (not reported) are consistent with this and incumbency effects as identified by Bhalotra et al. (2017) for the period before NREGS cannot explain such a result. This points towards existence of feedback loops between political and economic empowerment along the lines described above.

6. Conclusion and policy implications

Motivated by continued vast gender gaps in India and potential synergies between female empowerment in the political and the economic spheres, this study assessed the short- and longer-term impact of reserving political leadership positions for women on the ability of women to access the economic resources of NREGS, the world's largest workfare program, and the ways in which the social skills and material resources this program provided might in turn have affected political participation.

We find that having a female leader imposed by reservation enabled women to more effectively voice their demand for and to benefit from NREGS. It reduced the bribes and harassment they encountered in the process and enhanced their participation in program governance (e.g., by social audits). These impacts, many of which, being gender-specific, would not be visible in analysis of households, are not only sustained but often increased beyond the immediate reserved period and in most cases the estimated magnitude of reservation-induced effects increased after lapse of the reservation of village leadership position for females. This is consistent with the notion that political empowerment catalyzed broader changes that were reinforced by synergies and positive feedback loops between increased political awareness and voice as well as economic empowerment.

Declaration of Competing Interest

None.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jce.2019.12.003.

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