ESSAYS ON IDENTITY, WORKFARE PROGRAMS, AND LABOR MARKETS

By

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A DISSERTATION

Submitted to
Michigan State University
in partial fulfillment of the requirements
for the degree of

Agricultural, Food and Resource Economics—Doctor of Philosophy

2023

ABSTRACT

This dissertation explores three distinct aspects of labor markets in India: the impacts of the public works program on agricultural productivity, the impact of gender reservation in local leadership on labor market dynamics, and the role of identity-based political connections in public employment.

The first essay of the dissertation studies the impacts of India's National Rural Employment Guarantee Scheme (NREGS) on agricultural productivity. While previous research has primarily focused on wage effects, this study investigates the effects on agricultural productivity, considering wages, income, and infrastructure development. By employing the difference-in-differences method and utilizing detailed agricultural data, the analysis reveals that NREGS has led to a significant increase in agricultural productivity for marginal farmers and no effects for large farmers. The increase in agricultural productivity for marginal farmers can be attributed to the more intensive use of family labor by marginal farmers, coupled with mechanization and increased irrigation.

The second essay of the dissertation focuses on the decline in female labor force participation in rural India. The study explores the effects of gender reservation in village council leadership on women's access to job opportunities, labor force participation, income, and intra-household bargaining in the short-and medium term. The econometric results suggest that gender reservation in local leadership primarily affects female participation in public works and regular labor markets and their income and influence on household decisions. The effects of reservation are observed with a lag, indicating the influence of social norms and stereotypes.

The third essay investigates the role of identity-based political connections in public employment. A measure of political connection is constructed by matching surnames representing the caste of individuals and local leaders. The caste-based reservations are used to instrument the identities of council leaders to address the potential endogeneity problem.

The findings indicate that political connections significantly increased participation in public employment, both on intensive and extensive margins. The results are robust to the inclusion of controls and various fixed effects. Moreover, the study finds that connections with council leaders have a stronger effect compared to connections with village council members. This research emphasizes the dynamics of identity-based political networks and their implications for the distribution of public sector opportunities in India.

This dissertation is dedicated to
Elaine, Ben, and Lucy!

ACKNOWLEDGEMENTS

I am deeply grateful to my advisors and committee members, Songqing Jin, Chris Ahlin, Nicole Mason-Wardell, Kyoo Il Kim, and Eduardo Nakasone, for their invaluable and generous guidance and encouragement throughout my doctoral studies. A specific thank you goes to Songqing Jin and Chris Ahlin for their rigorous examination of each draft of the essays.

I extend my gratitude to Jeffrey Wooldridge, Robert Myers, Scott Swinton, Titus Awokuse, Richard Horan, Soren Anderson, Mywish Maredia, and Thomas Reardon for their unwavering support throughout my graduate journey. I also benefited from discussions with Hari K. Nagarajan, Nese Yildiz, Klaus Deininger, Yanyan Liu, and Caitlin Brown. Additionally, I wish to convey my sincere appreciation to my professors, colleagues, and friends from both the Economics and Agricultural, Food, and Resource Economics departments for their invaluable support.

At the University of Rochester, I am thankful to my advisor, Travis Baseler, my colleagues and friends, and the Department of Economics for providing an intellectually stimulating environment that has greatly benefited me during my graduate studies. Gratitude is owed to countless friends—far too numerous to acknowledge individually—spanning across Michigan State University and the University of Rochester.

Above all, I want to express my heartfelt gratitude to my family. Without them, it would not have been possible for me to be where I am today. I am particularly grateful to my partner, Elaine, for taking care of our son, Ben, and for her understanding and support—especially throughout the pandemic when I needed to stay apart. I truly love you both. I'm sincerely grateful to my mother-in-law, Sandra Hill, for her constant support, particularly throughout the pandemic. Lastly, I am immensely thankful to my parents and extended family members, who have always been there for me. Thanks to my pup, Lucy, for staying up with me during late working hours.

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CHAPTER 1

HOW PRODUCTIVE ARE WORKFARE PROGRAMS? EVIDENCE FROM INDIAN AGRICULTURE

1.1 Introduction

Workfare programs are increasingly popular in developing countries as a means to provide income support to the poor conditional on work while also promoting infrastructure development. One of the most notable and largest workfare programs in the world is India's National Rural Employment Guarantee Scheme (NREGS)[1]. Given its size and ambition, not surprisingly, NREGS has attracted considerable scholarly interest. NREGS has led to an increase in wages (Azam, 2011; Berg et al., 2014; Merfeld, 2019), income (Cook and Shah, 2022; Klonner and Oldiges, 2014), food security (Ravi and Engler, 2015), female empowerment (Afridi, Mukhopadhyay, and Sahoo, 2016), and reduced migration (Imbert and Papp, 2020), school enrollment, and scores (Shah and Steinberg, 2021), as well as conflict (Fetzer, 2020). There is also evidence that the program provides a safety net in response to shocks (Imbert and Papp, 2015), with positive impacts on reductions in infant mortality (Banerjee and Maharaj, 2020), stunting (Dasgupta, 2017), and higher human capital accumulation (Garg, Jagnani, and Taraz, 2020).

Yet, although most of India's rural poor are in the farming sector, and some of the above effects may be due to NREGS impacts on agricultural yields, program effects on agriculture productivity received little attention in the literature. Nationally, it was shown that program-induced wage increase triggered the adoption of small-scale labor-saving technologies, mainly by marginal farmers (Bhargava, 2014). In Andhra Pradesh, the safety net provided by NREGS helped smooth consumption in response to shocks and allowed farmers to adopt more risky crops with higher returns (Gehrke, 2019). While this suggests that NREGS

[1]The National Rural Employment Guarantee Scheme was introduced through the National Rural Employment Guarantee Act of 2005. Subsequently, on 2 October 2009, it was renamed the Mahatma Gandhi National Rural Employment Guarantee Act.

design features are well suited to provide targeted support to poor and smallholder farmers and act as a productive enhancing safety net, little is known about its impact.

In this paper, we aim to fill this gap by assessing NREGS impacts on agricultural productivity at the farmer level[2]. We draw on rich data from 1998/99 and 2007/08 rounds of the nationwide Rural Economic and Demographic Survey (REDS) panel household survey that covers all aspects of the production process (cultivated area, outputs produced in each season, input use, technology choice, etc.). To estimate the effects of NREGS on farmers and agricultural productivity, we exploit the variation from the phase-wise rollout of the program and compare farmers in early phase districts (treatment) to farmers in later phase districts (control)[3]. This allows us to assess the short-term productivity effects of NREGS, explore pathways for these to materialize, and draw out and test implications in terms of heterogeneity of effects across the farm size distribution. Additionally, we also use the data collected in 2014-16 on the number of employment days generated and capital use in NREGS projects for a subset of states to explore if NREGS affected agricultural productivity via project-related works generating public goods, an issue that to the best of our knowledge has thus far not been addressed in the literature.

Conceptually, NREGS' impact on agricultural production can materialize via several channels. By putting a floor under wages, the program is likely to raise the cost of agricultural production and reduce hired labor use (Santangelo, 2016). This may give rise to a substitution of the family for hired labor or efforts to replace labor with capital via mechanization. In the short term, higher wages, together with NREGS-induced expansion of labor demand, will result in an income effect that will transfer resources to benefit net sellers (i.e., small and marginal farmers) at the cost of net buyers (i.e., medium and large farmers) of labor.

[2]We use real output per acre as a measure of agricultural productivity throughout the paper.

[3]NREGS was implemented in a phased manner. In 2006, the program was initially introduced in the first 200 poorest districts. Subsequently, in 2007, the program extended to an additional 136 poorest districts. Finally, by April 2008, the scheme was expanded to cover the remaining districts.

As NREGS generates predictable employment in situations when, for example, exogenous shocks such as droughts or floods and other opportunities have dried up, it provides an implicit safety net (Godfrey-Wood and Flower, 2018). Possibly reinforced by the income effects noted above, this may encourage the adoption of income-generating strategies with higher risk-return profiles than those pursued traditionally by producers.

Finally, the program supports works to enhance agricultural productivity directly via land improvement and minor irrigation or indirectly via rural roads to improve market access. The size and incidence of productivity benefits from such works will depend on program management, including selecting improvements with high potential impact, implementing them transparently, and ensuring proper maintenance. NREGS works are thus expected to have the most impact in states with high implementation quality. Size and incidence of effects will depend on the type of work: land improvement or irrigation are often targeted towards small producers (Ranaware et al., 2015) whereas general-purpose investments such as roads are likely to benefit producers in proportion to their size of production.

Taken together, the program's self-targeting, the positive income effect, and the fact that an implicit safety net mainly benefits the poor lead us to expect NREGS effects to be particularly pronounced for small and marginal farmers, an effect possibly reinforced by the portfolio of work. Our DID regression suggests that NREGS indeed led to a significant increase of 18% in agricultural productivity for marginal farmers with an operating area of less than 2.5 acres ($\sim$ 1 ha).

We find that the program affected the input used in agricultural production for marginal and large farmers differently. It appears to have no effect on hired labor use for marginal farms, but it decreased hired labor use for large farms by six days per acre. This decrease seems to be offset by increased investment in irrigation and intensive use of fertilizer. Simultaneously, the program increased family labor use by 13 days per acre for marginal farms, coupled with mechanization and intensive irrigation use. However, it had no effects on family labor use for large or medium farms.

We also find that NREGS-related investment in infrastructure increased agricultural productivity. The estimated effects of total investment and employment generation on agricultural output per acre are 4% and 3%, respectively. For marginal farmers, these effects of investment and employment are 6% and 3%, respectively.

This paper contributes to two strands of literature. We add to the small literature exploring NREGS impacts on agricultural productivity by documenting different channels for such impact, including program-created works. While our findings regarding mechanization and risk-coping are in line with what was found by studies such as Bhargava (2014) and Gehrke (2019) and recent contributions that emphasized the role of NREGS as a safety net to cope with shocks, our figures illustrate that most benefits accrue to marginal and small farmers. We also find that NREGS-backed projects aimed at supporting agricultural production have a positive productivity impact and that the size of such productivity benefits is potentially large enough to allow recouping some of the program costs.

A second strand of the literature we contribute relates to the debate on the merits and demerits of workfare compared to cash transfers (Dreze and Sen, 1990; Ravallion, 2022). While this issue is of interest in many countries (Gehrke and Hartwig, 2018), it became salient in India following recent proposals to replace a plethora of subsidies with a basic income (Coady and Prady, 2018; Khosla et al., 2018). We show that, if effectively implemented, a workfare program can generate productivity benefits by building public infrastructure and providing a safety net while work requirements act as a screening device (Besley and Coate, 1995) that can sharpen targeting and enhance the program's cost-effectiveness. Implementation capacity thus seems a key factor: although workfare is preferable in principle, capacity gaps that would lead to rationing (Dutta et al., 2012) or works with high social payoffs difficult to implement (Alik-Lagrange and Ravallion, 2018) could tilt the balance in favor of cash transfers. Quantifying the associated trade-offs as well as ways to address capacity constraints via IT solutions, would thus be of great interest.

The rest of the paper is structured as follows: In section 1.2, we describe the program characteristics and potential impact pathways. Section 3.3 discusses the data used in the paper. In section 1.4, we discuss the empirical approach, the method used to evaluate it, the identification strategy, and the extent to which it is supported by pre-program parallel trends between the treatment and control groups. In section 1.5, we present the estimated impacts of NREGS on agricultural productivity and mechanism: labor use, mechanization, and input use. In section 1.6, we discuss the impact of different NREGS-supported works on agricultural productivity. And conclude in section 3.7.

1.2 Program Characteristics

To motivate our analysis, we describe National Rural Employment Guarantee Scheme (NREGS) characteristics and implementation arrangements and briefly summarize the literature assessing the program's impact on wages, households' ability to cope with shocks, and the provision of local public goods. We use this to set out a conceptual framework and hypotheses to be tested in our empirical investigation.

1.2.1 National Rural Employment Guarantee Scheme

India's National Rural Employment Guarantee Scheme (NREGS) is the largest workfare program of this nature globally. NREGS was implemented in a phase-wise manner. In 2006, the program was initially introduced in the first 200 poorest districts. Subsequently, in 2007, the program extended to an additional 136 poorest districts. Finally, by April 2008, the scheme was expanded to cover the remaining districts[4]. Building on the country's long tradition of food-for-work schemes (Dutta et al., 2012; Subbarao, 1997)[5], NREGS

[4]The selection of districts was based on a poverty index developed by the Planning Commission using data from the 1993/94 National Sample Survey, a routine survey used to produce among others estimates of poverty.

[5]The work requirement differentiates NREGS from conditional cash transfer programs such as PROGRESA (Attanasio et al. 2012) or Bolsa Escola (Glewwe and Kassouf 2012) that focus on education or from unconditional cash transfers.

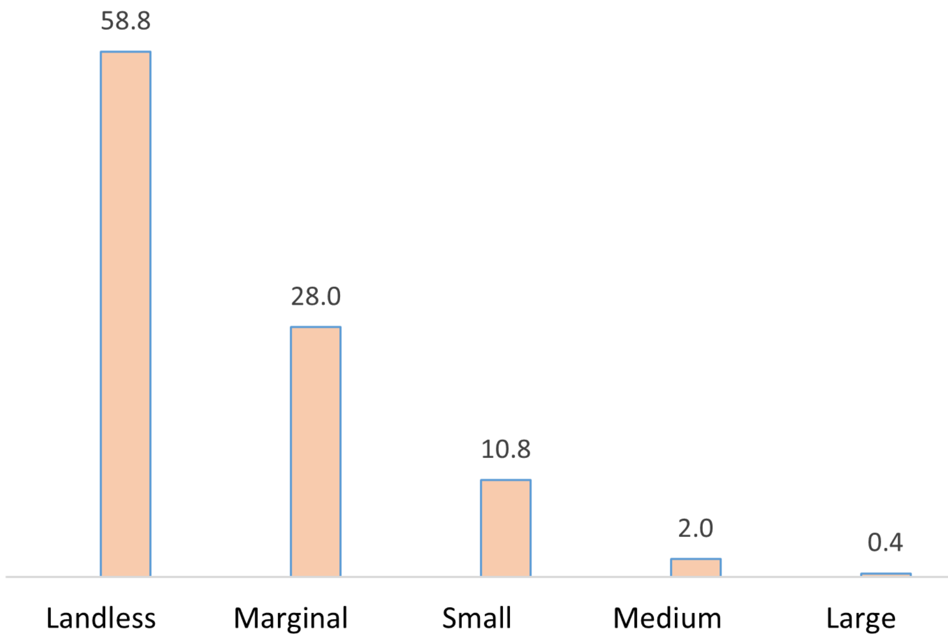

Figure 1.1 Distribution of Workers in NREGS

guarantees employment of up to 100 days per year to households who registered locally to obtain a job card and have applied for jobs [6]. In line with the program's self-targeting, anybody can apply for a job card and then a job. Once a job application is lodged, authorities are obliged to provide work opportunities within 15 days of application. However, if applicants do not receive employment within this timeframe, they become eligible to receive an unemployment allowance retroactively from the date of their application.

Physical labor is sourced to build productive assets such as access to roads and water harvesting structures. 60% of the total NREGS workers are landless and are likely to be net sellers of labor. The share of workers who are marginal farmers is 28 percent, while it is only 12 percent for small and medium farmers combined.[7] (See Figure 3.4)

[6] Applicants are eligible to receive a job card containing photos of all adult household members free within 15 days of application. Job-card holders' indicative work demand leads to the elaboration of an annual plan which, 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.

[7] Marginal, small, medium, and large farmers are defined as those owning less than 2.5 acres, 2.5-5 acres,

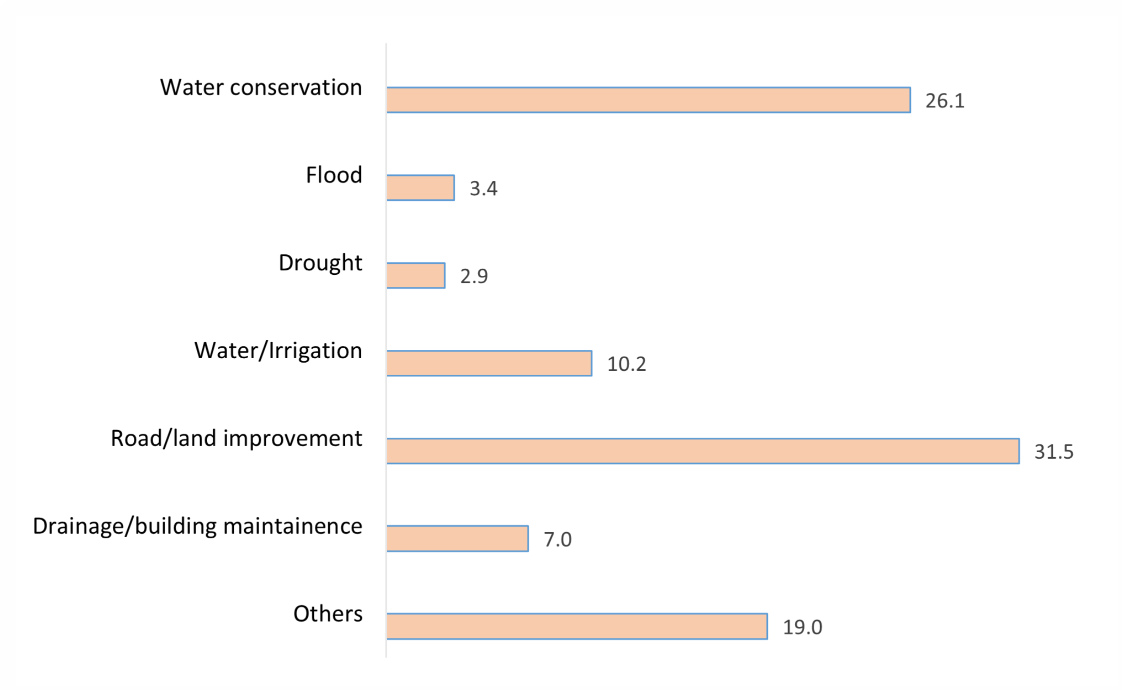

Figure 1.2 Distribution of Projects Carried Out Under NREGS

NREGS also explicitly encourages female participation by paying equal wages to men and women by requiring work to be provided in ways attentive to women's needs and a gender quota of one-third. Among landless workers, 68% are women while only 32% are men. Women are more likely to work among the marginal farmers, too, but the difference between men and women starts to disappear for small, large, and medium landholders (Figure 1.4). The largest share of work is carried out in the road and land-related works, and water conservation is the second largest work in the NREGS. Rural drinking water-related work, sanitation work, and construction of aganwadis and play fields together generate about 19 percent of total work in NREGS. Some of these projects, such as water and irrigation-related work, can directly affect agricultural productivity, while others, i.e., roads, can affect agricultural productivity indirectly by reducing the market access frictions that are likely to be faced by small and marginal farmers (See Figure 1.2).

5-10 acres, and more than 10 acres, respectively.

The overall responsibility for implementation is with states, and many functions are delegated to villages or panchayats[8]. States put in place controls, including the use of electronic payments and social audits, to minimize abuse, adjust to variations in capacity, and ensure the program is implemented transparently.

1.2.2 Potential Impact Pathways

NREGS is likely to impact agricultural production through three channels, namely, (i) a wage and income effect, the size and incidence of which will differ between net sellers and buyers of labor; (ii) a risk reduction effect that, in line with the program's self-targeting character, will mainly accrue to small producers; and (iii) a public good effect through works constructed by the program.

Wage and income effects arise as, by offering a minimum daily wage equally to men and women, NREGS puts a floor under wages. In the short run, the exogenous introduction of alternative employment opportunities will reduce labor supply to agriculture and tradable sectors and increase production cost (Santangelo, 2016). The direction of associated income effects depends on individuals' position in the labor market: NREGS will increase income for net sellers of labor but have an ambiguous or negative impact on net buyers. Producers can respond to wage increases by (i) substituting the family for hired labor; (ii) investing in labor-saving mechanical technology; or (iii) changing the output mix including the possibility of reducing output

Many studies indeed find NREGS increased wages. Monthly wage data for 2000-2011 suggest that NREGS increased the growth of real agricultural wages by 4.3% during the peak season, with gender wage gaps and skilled wages remaining unaffected (Berg et al., 2014)[9].

[8]Village governments (gram panchayats) and their staff are responsible to match jobseekers to jobs throughout the year. These include the preparing a list of projects at a reasonable distance from the village, measures to allow female participation (e.g. child care), supervision of ongoing projects, worker identification and assignment to specific sites, and financial management including wage payment

[9]A 2005-10 district panel that combines NSS data with information on rainfall and NREGS-induced asset generation suggests that, partly due to the assets generated by NREGS, the sensitivity of wages to rainfall shocks decreased by some 10 points Shah (2012).

National Sample Survey (NSS) data point towards program-induced wage increases of about 5 points overall and 9 points in 'star' states characterized by good program implementation (Azam, 2011; Imbert and Papp, 2015). Such increases have been shown to be confined to the dry season, reinforcing the notion of NREGS acting as a safety net (Imbert and Papp, 2020). This is consistent with reports of the program having reduced short-term 'distress' migration by unskilled workers in the dry season, in addition to, directly and indirectly, enhancing the returns to small and medium farmers' assets through water-conservation structures and improving liquidity without affecting long-term migration by the higher-skilled (Reddy, Reddy, and Bantilan, 2014). Econometric evidence from data specifically collected for this purpose supports the notion of a program-induced reduction in short-term dry-season migration but no effect on long-term migration (Coffey, 2013; Imbert and Papp, 2020).

In line with the notion that program-induced wage increase will provide the biggest benefit to net sellers of labor, NSS data point towards large season-specific effects for scheduled castes and tribes (SCs and STs) but not the general population[10], as NREGS enabled these groups to smooth consumption across seasons (Klonner and Oldiges, 2014)[11]. In Andhra Pradesh, one of the better performing states, panel data suggest that NREGS was well targeted, improving food consumption and asset accumulation by the poor (Deininger and Liu, 2019). This is likely to be the mechanism driving positive impacts on food security, savings, and health outcomes by the poor (Ravi and Engler, 2015), female empowerment (Afridi, Mukhopadhyay, and Sahoo, 2016), and less gender-based violence (Amaral, Bandyopadhyay, and Sensarma, 2015), especially for the poor. Positive investment effects are suggested by increased school participation by primary-aged children (Islam and Sivasankaran, 2015)

[10] Data from the India Human Development Survey (IHDS) have been used to argue that NREGS increased income and wages for the unskilled, reduced poverty by up to 32% and prevented 14 million from falling into poverty (Desai et al. 2015)

[11] A back-of-the-envelope cost-benefit analysis suggests that consumption smoothing benefits for the SC/ST population would have been sufficient to justify the wage outlays incurred by NREGS (Klonner and Oldiges, 2014).

and better primary achievement scores and grade progression (Mani et al., 2014). Effects on enrollment and test scores for those in secondary school are, however, negative, most likely because at this age children can substitute for adult labor (Shah and Steinberg, 2021)[12].

Potential risk reduction benefits from NREGS are well recognized (Zimmermann 2015b) and a series of recent studies link the NREGS roll-out with climatic data to document the program's impact on mitigating the impacts of drought on children's development (Dasgupta, 2017), infant mortality (Banerjee and Maharaj, 2020), cognitive development (Garg, Jagnani, and Taraz, 2020), and agricultural yields (Taraz, 2019). Access to implicit insurance via the program, particularly if combined with positive income effects, can allow investment in high-risk high return activities that, although available earlier, would have entailed too much of downside risk, especially in agriculture where NREGS access allowed mechanization (Bhargava, 2014) and a shift towards riskier but more remunerative crops (Gehrke, 2019) by small farmers.

If the program is implemented as directed, the public infrastructure created by NREGS could further enhance agricultural productivity. Variation in NREGS implementation is likely to affect the quality and longevity of any structures built under the program. Moreover, the size and incidence of benefits from such structures will depend on their type with investments facilitating market access likely to benefit all producers in proportion to their level of market participation. NREGS guidelines allow land improvement and minor irrigation activities to be conducted on marginal farmers' private land (Babu et al., 2014), an option that was widely utilized and rather well-targeted even in states not known for high-quality implementation.

Few studies tried to assess the impact of infrastructure constructed via NREGS. Desai et al. (2015) report that about 51% of program-related works are 'completed' but fail to provide information on quality. While effects of such infrastructure were indeed limited in

[12]The program is also shown to have affected election outcomes (Zimmermann 2015a) and local violence (Khanna and Zimmermann 2014).

Andhra Pradesh (Deininger and Liu, 2019), high quality of NREGS works was found to increase the share of area devoted to cash crops (Shah 2013) although it is not clear whether this is due to the works or the risk-reduction afforded by having employment through the program available in times of need.

1.3 Data

For our main empirical analysis, we use the data from three rounds of the Rural Economic and Demographic Survey (REDS) conducted by India's National Council of Applied Economic Research in 1981-82, 1998-99, and 2007-08[13]. The REDS builds on the Additional Rural Income Survey (ARIS), first conducted in 1969 and 1971 that provides comprehensive information on labor supply and wage receipts by each household member and inputs and outputs from agricultural production for 4,257 households in 259 villages located in 17 major Indian states that were representative of India's rural population, though with an emphasis on higher-potential areas, at the time the sample was drawn[14].

In 1981-82 surviving households and descendants from the original sample who resided in the same village and a random sample of new households were surveyed (Vashishta 1989). A similar procedure was applied to subsequent survey rounds resulting in a sample of 7,474 and 8,659 households across 242 villages in 17 major states in 1998/99 and 2007/08, respectively[15]. The survey is particularly suited for the analysis as it contains detailed information on labor supply and wage receipts by each household member and inputs and outputs from agricultural production. To assess NREGS impacts, we draw primarily on the survey's labor and agricultural modules to assess the impacts of the program being available

[13]Until 1981-82, the survey was conducted as Additional Rural Income Survey. The REDS 2006 survey started in 2006 with a listing of all households in sample villages. As agricultural data refer to the 2007/08 season, we refer to this survey as 2007/08 throughout.

[14]Unfortunately, data from the first round, which was based on a frame including one district per state in the Intensive Agricultural District Program (IADP) and a random sample of other districts

[15]Political unrest precluded data collection in Jammu & Kashmir and Assam in 2006 and is thus dropped from our analysis. Also, delayed survey implementation in Kerala implied that, when the 2006 REDS questionnaire was administered, all the state's districts had already been covered by NREGS, forcing us to drop observations from this state.

on labor markets and agricultural productivity[16].

Socio-Economic Profile of India (SEPRI) We also use data from the Socio-Economic Profile of India (SEPRI), conducted between 2014 and 2016, to enrich our analysis. The SEPRI is a follow-up survey of REDS and is administered to all households residing in the REDS villages across the 13 states. The SEPRI survey employed the complete set of questions of village modules from REDS, augmented by additional information on village-level activities related to the National Rural Employment Guarantee Scheme (NREGS). However, only a selected subset of questions from the household module of REDS was used. The SEPRI contains detailed information on inputs and outputs from agricultural production, consumption, assets, as well as NREGS activities such as project types, employment generated, and investment in each project types[17]

Summary Statistics Key household and agricultural production statistics for 1998/99 and 2007/08 and in treatment and control districts are in Table A.1. Household size significantly declines from 6.7 to 5.8 members on average. Partly due to subdivision with succession, the mean land endowment declined from 5.9 to 5.1, with a modest increase in the incidence of irrigation from 55% of land to 60% of land. Control areas are characterized by higher mean land endowment and a higher share of irrigated land. Although NREGS-related work is not identified separately in our data for the main analysis, we have detailed information on the number of days devoted to crop cultivation by male and female family members and hired workers. As a main outcome in the paper, we use harvest value per acre. Inputs such as seed, machinery, and fertilizer are measured as the value per acre[18].

Information on agricultural production outcomes in Panel B, Table A.1 points toward

[16]The sample includes all of a household's descendants who still reside in the village in case of a split.

[17]The SEPRI was conducted in 13 Indian states, namely Andhra Pradesh, Bihar, Chhattisgarh, Gujarat, Haryana, Jharkhand, Madhya Pradesh, Maharashtra, Orissa, Rajasthan, Tamil Nadu, Uttar Pradesh, and West Bengal. Unlike the extensive questionnaire used in the REDS survey, SEPRI uses a subset of questions from REDS with some additional questions on NREGS while sampling the entire population of the REDS survey villages.

[18]All values of output and inputs are in real term.

modest increases in the real value of output (from Rs. 7784 per acre to Rs. 8717 per acre). While the total number of hired laborers used increased from 16 to 20 days as well as family labor, there was no increase in female family labor use per acre between the two rounds of the survey. The summary table by farm size group is provided in Table A.2.

1.4 Empirical Strategy

The National Employment Guarantee program was implemented in three phases. Phase I was implemented in February 2006, Phase II was implemented in April-May 2007, and Phase III was implemented in April 2008. We combine the phase-wise implementation of the NREGS and REDS 1999 and 2007-08 to assess the program's impact on agricultural productivity. When the 2007/8 round of REDS was administered, NREGS had been implemented in phase I and II districts (treatment group) but not yet in those to be covered under phase III of the program, allowing us to use the latter as a control group in the difference-in-differences (DID) regression[19]. Letting i denote households, v village, d district, and t time. We estimate the following equation.

$$Y_{ivdt} = \beta_0 + \beta_1 NREGS_{dt} + \lambda X_v \times Round_t + Z_{vdt} + \mu_i + \phi_t + \epsilon_{ivdt} \tag{1.1}$$

where Y_{idt} is the outcome variable of interest such as output and inputs per acre for farmer i, in village v and district d in year t, $NREGS_{dt}$ is an indicator variable that equals one if district d was the part of phases I or II of the program roll-out in year t and zero otherwise; X is a vector pre-treatment time constant village level control interacted with survey round year. *Round* is a dummy variable for the survey round and takes on the value one if t=2007/08 and zero otherwise. The time-invariant controls are constructed using data

[19]As discussed above, for Andhra Pradesh, Bihar, Chattisgarh, Haryana, Madhya Pradesh, Maharashtra, Rajasthan, and Tamil Nadu, we were also able to obtain administrative data on employment generated by NREGS overall and for irrigation-related activities, mainly cleaning and rehabilitating canals. If implemented effectively, the latter would quickly improve agricultural productivity even for non-participants while program-induced wage payments would affect risk coping mainly by direct beneficiaries. Due to delays in implementing the survey in Kerala, all of the state's districts had been covered by NREGS at the time the survey was administered, forcing us to drop observations from this state. Our final analysis sample thus includes 16 of the original states.

from REDS 1999 data and Census 2001. Z is a vector of time-varying controls such as village-level rainfall. μ_i is either district or household fixed effects and ϵ_{idt} is an error term. ϕ_t includes year-fixed effects, season-fixed effects, as well as stat-by-year fixed effects. We run the variation of the equation 1.1 to account for other sources of differential changes when necessary. Our main DID coefficient of interest is β_1, which measures the impact of NREGS exposure. Standard errors are clustered at the district level throughout.

Identification The identification of DID requires that households in both treated and control districts follow a similar agricultural productivity growth path in the absence of treatment. Even though a test of parallel trend is not possible, we can approximate the test using pre-treatment data. We use the data from 1982 and 1999 of REDS to test for the parallel pre-trend (see Table A.3). Test of parallel pre-trend is conditioned on household fixed effects, allowing us to control time-invariant unobservable that might affect selection into treatment. A notable caveat of using 1982 and 1999 REDS data is that there is a large gap between the survey periods. Although several challenges, including household splits and attrition, must be considered, results from tests for parallel trends in pre-program trajectories can, in principle, be conducted using the 1982 to 1999 rounds of the REDS panel. Results in Appendix Table A.3 suggest that the hypothesis of parallel pre-trends cannot be rejected. We also test for parallel pre-trend of the key variables by farm size group. Results are presented in Table A.4, suggestive of the fact that parallel pre-trends hold for all farm size groups.

As trends over the 1982-99 period may not be relevant for what occurred closer to NREGS introduction, we also use production data collected through the cost of cultivation survey (CCS) by the Ministry of Agriculture in 2000-2005, i.e., directly before NREGS became available. Contrary to Merfeld (2019) who uses NSS data to do so, we rely on the cost of cultivation survey because it provides information on key agricultural outcomes, including total and per acre agricultural output, days of family and hired labor, casual wage rates and value of main inputs (fertilizer, manure, machinery, and irrigation) in Rs/ha [20] We restrict our sample to the REDS districts and construct a pseudo-panel at the block level. Results in Appendix Table A.5 suggest that there is no evidence of pre-trends between treated and non-treated districts just before the program became effective.

1.5 Results

1.5.1 Effects on Agricultural Productivity

The NREGS can affect agriculture directly via land and soil improvement and indirectly through wages and reallocation of inputs. However, before discussing specific channels through which NREGS might affect productivity, we start with the evidence of aggregate productivity. Using the difference-in-differences (DID) framework discussed in equation 1.1, we estimate the program effects on agricultural productivity measured as real output per acre.

Results of the aggregate agricultural productivity impact are reported in Table 1.1. In the first column, we provide the baseline specification and the effect of NREGS estimated using the difference-in-differences (DID) method after accounting for household, year, season, and state-by-year fixed effects. By including household fixed effects, we are able to account for unobservable factors at the household level that could affect productivity. The inclusion of year-fixed effects helps capture any policy changes at the national level that may affect productivity. Additionally, we include season and state-by-year dummy variables to account for seasonal variations and state-specific trends in prices and agricultural policies. In the subsequent columns, we further enhance our baseline specification from column 1 of Table 1.1 by progressively incorporating additional fixed effects. This allows us to

[20] These data have been collected on an annual basis by the Directorate of Economics and Statistics in India's Ministry of Agriculture since 1971/72 mainly to determine the minimum support price at which the Government will procure main crops. For sampling purposes, states are divided into homogenous zones based on cropping patterns, soil quality, rainfall, and irrigation. A multi-stage stratified random sampling design was adopted, selecting districts and subdistricts (tehsil) in the first stage, villages in the second, and operational holding in the third stage. Operational holdings are divided into five categories, such as up to 1 ha, 1-2 ha, 2-4 ha, 4-6 ha, and above 6 ha, and at least two holdings are selected from each category.

control for differential changes at the year-season level, changes specific to different crops, and crop-by-season level variations such as the selection of crops influenced by seasonal factors.

Table 1.1 Effects of NREGS on Agricultural Productivty

	(1)	(2)	(3)	(4)	(5)	(6)
NREGS	0.115	0.108	0.121	0.101	0.127	0.107
	(0.095)	(0.093)	(0.084)	(0.078)	(0.085)	(0.079)
Observations	14,620	14,620	14,620	14,620	14,620	14,620
R-squared	0.462	0.463	0.578	0.581	0.585	0.587
Household, year FEs	Y	Y	Y	Y	Y	Y
Season FEs	Y	Y	Y	Y	Y	Y
State-by-year FEs	Y	Y	Y	Y	Y	Y
Season-by-year FEs		Y	Y	Y	Y	Y
Crop FEs			Y	Y		
Crop-by-season FEs					Y	Y
Controls				Y		Y

Note: Dependent variable is the log of output value per acre throughout. NREGS is a dummy and takes on the value one if districts are from phases 1 and 2; otherwise, zero. Each column reports the coefficients of interest from separate regressions. The vector of controls includes rainfall for each village and survey year. It also includes time-constant pre-program village-level controls such as distance to the nearest town, access to the local market, distance to district HQ, log of the population schools, etc., constructed from census 2001 and REDS 1999, interacted with year. Standard errors are clustered at the district level and reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1

We also include sets of controls. These controls consist of time-varying rainfall at the village level and time-constant pre-program variables constructed from REDS99 and Census 2001 interacted with the year variable to account for divergent trends in village-level developments. We did not find a statistically significant effect of NREGS on aggregate productivity. Although coefficients are positive and economically meaningful, ranging from 0.107 to 0.127 percentage points, they are not precisely estimated.

1.5.1.1 By Farm-Size

To explore the heterogeneity in effects of NREGS by farm size, we divided farms into four categories closely following the government of India's definition [21]. We conducted similar regression analyses as presented in Table 1.2 for each farm size category. The results revealed that NREGS had a positive impact on agricultural productivity for small farmers, specifically those with land holdings of less than 2.5 acres. However, we did not find any significant effects of NREGS on the productivity of other farmer categories. For marginal farmers, the implementation of NREGS led to a remarkable 18 percent increase in agricultural productivity. This increase is equivalent to a gain of 1636 Rs per acre.

Table 1.2 Effects of NREGS on Agricultural Productivity by Farm Size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Marginal (< 2.5 ac.)		Small (2.5-5 ac.)		Medium (5-10 ac.)		Large (>10 ac.)	
NREGS	0.182**	0.177**	0.015	-0.044	0.128	0.071	0.131	0.083
	(0.088)	(0.081)	(0.132)	(0.125)	(0.106)	(0.107)	(0.137)	(0.131)
Observations	4,267	4,267	3,719	3,719	3,556	3,556	3,071	3,071
R-squared	0.595	0.601	0.576	0.585	0.604	0.609	0.584	0.594
Controls		Y		Y		Y		Y

Note: Dependent variable is the log of output value per acre throughout. NREGS is a dummy and takes on the value one if districts are from phases 1 and 2; otherwise, zero. Each column reports the coefficients of interest from separate regressions. Household and year-fixed effects as well are included throughout. Regressions are also adjusted for season, season-by-year, state-by-year, and crop-by-season fixed effects. The vector of controls includes rainfall for each village and survey year. It also includes time-constant pre-treatment village-level controls such as distance to the nearest town, access to the local market, distance to district HQ, log of the population schools, etc., constructed from census 2001 and REDS 1999, interacted with year. Standard errors are clustered at the district level and reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1

The coefficients are stable and effects size are similar for marginal farmers with and without the controls. We explore in more detail whether the incidence of benefits on marginal farmers can be attributed to the program being self-targeting, the fact that small

[21] Our definition of farm size categories closely mimics the definition used by the government of India. GOI defines farmers as follows marginal (<1 ha) farmer, small (1-2 ha), small-medium (2-4 ha), medium (4-10 ha).

farmers are likely to be net sellers of labor and thus benefit most from NREGS-induced wage increases, and the provision of implicit safety nets through program-supported public goods are disproportionately valuable for marginal farmers.

1.5.2 Effects on Agricultural Labor Use

Regression results of labor use separated by family and hired as well as farmer types are reported in Table 1.3. In Panel A of Table 1.3, we examine the effect of the NREGS program on hired labor days in agriculture. We did not observe any significant aggregate effect of NREGS on hired labor days. However, it is important to consider that the labor market in India is incomplete, and farmers with different land size holdings may face varying constraints (Deininger et al., 2018). After separating the sample by farm size groups, we find that large farms faces labor shortage in hiring labor due to NREGS with an estimated coefficient of 6.21 per acre. Which is equivalent to a 40 percent decrease in the hired labor use per acre with respect to control districts. The large farmers offset the decrease in the hired labor use with intensive use of fertilizer per acre (see Table 1.4).

In panel B of Table 1.3, we report the estimates for family labor use in agricultural production. The NREGS program is likely to affect the allocation of labor in both the agricultural and non-agricultural sectors through an increase in wages (Azam, 2011; Berg et al., 2012; Imbert and Papp, 2015; Merfeld, 2019). Our estimated coefficient for family labor is positive and statistically significant for marginal farmers, indicating that they are more likely to work on their own farms. This finding can be rationalized by the fact that NREGS reduced out-migration by 22 percent (Imbert and Papp, 2020). Moreover, as NREGS provides 100 days of job opportunities and supposes to complement agricultural activities by providing more work in the lean season, it may reduce the need for farmers to search for additional employment, allowing them to dedicate more time to their own farms.

The observed increase in family labor usage is primarily driven by more male members of the family working on their own farms. This aligns with the notion of reduced migration,

Table 1.3 NREGS and Agricultural Labor Use

VARIABLES	(1) All	(2) Marginal	(3) Small	(4) Medium	(5) Large
	Panel A: Hired Labor Days				
NREGS	-3.69	-4.15	-6.30	-1.13	-6.21**
	(2.74)	(3.54)	(4.16)	(3.57)	(2.90)
Observations	14,620	4,267	3,719	3,556	3,071
R-squared	0.60	0.62	0.63	0.63	0.58
	Panel B: Family Labor Days				
NREGS	4.43	12.34**	2.43	0.91	-4.57
	(5.00)	(5.58)	(7.16)	(5.85)	(3.04)
Observations	14,620	4,267	3,719	3,556	3,071
R-squared	0.59	0.62	0.54	0.53	0.58

Note: Dependent variable is labor days per acre throughout. NREGS is a dummy and takes on the value one if districts are from phases 1 and 2; otherwise, zero. Each column reports the coefficients of interest from separate regressions. Marginal, small, medium, and large farmers are defined as owning less than 2.5 acres, 2.5-5 acres, 5-10 acres, and more than 10 acres, respectively. Household and year-fixed effects as well are included throughout. Regressions are also adjusted for season, season-by-year, state-by-year, and crop-by-season fixed effects. The vector of controls includes rainfall for each village and survey year. It also includes time-constant pre-treatment village-level controls such as distance to the nearest town, access to the local market, distance to district HQ, log of the population schools, etc., constructed from census 2001 and REDS 1999, interacted with year. Standard errors are clustered at the district level and reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1

as males are typically more prone to out-migrate during lean agricultural seasons in search of employment (Table A.6). Furthermore, we also test whether it is true that marginal farmers are working on their farms more than the control groups in the appendix (Table A.8). We utilize the member-level labor data for all households and find that in districts with the program, marginal farmers are 7% more likely to work on their own farms.

1.5.3 Mechanization and Input intensification

While substituting family for hired labor is one way to respond to higher wages, it has limited scope and may be associated with undesirable side effects. As rental markets for ploughing services to support field preparation are active in India (Binswanger and Singh, 2018), mechanization is an option that can be pursued in parallel or as an alternative. In

Table 1.4, we present the estimated coefficient of the program on the intensification of input use. In Panel A of Table 1.4, we do not find any significant effects of NREGS on machinery use per acre overall. However, there was an observed increase in machinery usage per acre, specifically among marginal farmers. The estimated increase in machinery rental changes amounted to 70 Rs per acre, which is equivalent to a 23% increase compared to the mean in control districts.

Although the coefficients are positive for large farms as well but are not precisely estimated, this suggests that expansion of rental equipment use was more pronounced for marginal farmers than for large farmers, consistent with the notion that the marginal labor-saving effects of using small-scale equipment are highest for marginal producers (Bhargava, 2014). Such a finding is also consistent with the self-targeting nature of the program and the fact that farmers who primarily sell their labor will benefit the most from mechanization as the program could increase available liquidity for those who were previously constrained

In panel B of Table 1.4, we report the estimated coefficient of NREGS on fertilizer use. The estimated coefficient for fertilizer with respect to the program is positive across all farm size groups but only statistically significant for large farmers. The increase for large farms is about 129 Rs/acre, which is equivalent to an increase of 26 percent from the control group. The increase in fertilizer intensity for large farms net out the effects of a decrease in hired labor per acre for large farmers, which was about 33 percent (see Table 1.3). In panel C, we report the effects on irrigation intensity of the program and find that NREGS increases the intensive use of irrigation coupled with an increase in family labor use for marginal farmers. The increase in the family labor use for marginal farmers is almost entirely driven by more males working more days in agriculture in the family. This is also consistent with the notion that irrigation is almost entirely carried out by men since it requires complementary physical laborers.

Table 1.4 Agricultural Inputs

VARIABLES	(1) All	(2) Marginal	(3) Small	(4) Medium	(5) Large
			Panel A: Machinery		
NREGS	11.05	70.05*	1.93	-37.18	45.87
	(49.65)	(37.97)	(66.11)	(81.34)	(74.87)
Observations	14,620	4,267	3,719	3,556	3,071
R-squared	0.62	0.68	0.64	0.58	0.58
			Panel B: Fertilizer		
NREGS	61.83	99.74	32.11	8.47	129.20*
	(57.27)	(64.13)	(73.60)	(76.89)	(70.38)
Observations	14,620	4,267	3,719	3,556	3,071
R-squared	0.55	0.59	0.55	0.53	0.55
			Panel C: Irrigation		
NREGS	33.78	120.43*	50.78	-38.24	-7.35
	(44.18)	(64.99)	(44.23)	(40.41)	(22.40)
Observations	14,620	4,267	3,719	3,556	3,071
R-squared	0.60	0.67	0.56	0.54	0.53

Note: Dependent variables are per acre throughout. NREGS is a dummy and takes on the value one if districts are from phases 1 and 2; otherwise, zero. Each column reports the coefficients of interest from separate regressions. Marginal, small, medium, and large farmers are defined as owning less than 2.5 acres, 2.5-5 acres, 5-10 acres, and more than 10 acres, respectively. Household and year-fixed effects as well are included throughout. Regressions are also adjusted for season, season-by-year, state-by-year, and crop-by-season fixed effects. The vector of controls includes rainfall for each village and survey year. It also includes time-constant pre-treatment village-level controls such as distance to the nearest town, access to the local market, distance to district HQ, log of the population schools, etc., constructed from census 2001 and REDS 1999, interacted with year. Standard errors are clustered at the district level and reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1

1.5.4 High Value Crops

Public works programs can effectively mitigate income uncertainty by enhancing constant income support and increasing liquidity for farmers by providing additional employment opportunities. This improved financial stability, in turn, enables them to allocate a greater portion of their resources toward cultivating high-value crops Gehrke (2019). Diversification of output beyond the traditional staples of rice and wheat and increases in cropping intensity have long been identified as essential for Indian agriculture to achieve higher levels of productivity and competitiveness. Crops other than rice and wheat are, however, riskier as they are not covered by government-imposed floor price schemes and, especially if grown in the off-season, more vulnerable to climatic risk. Intensification via multi-cropping requires liquidity for the timely provision of inputs (including field preparation) and, where water is scarce, involves higher risk and the need to coordinate water supply. Changes in the number of crops or the share of area devoted to non-traditional crops could thus be interpreted as proxy for farmers' higher ability to assume risk or to manage a more complex production process better.

Table 1.5 NREGS Increased Share of High-Value Crops

	(1) All	(2) Marginal	(3) Small	(4) Medium	(5) Large
			High Value Crops		
NREGS	0.031*	0.027*	0.052**	0.034	0.030
	(0.018)	(0.015)	(0.025)	(0.028)	(0.027)
Observations	14,620	4,267	3,719	3,556	3,071
R-squared	0.821	0.802	0.834	0.815	0.835

Note: Dependent variable is dummy if the crops are of high value. These crops include pulses, oilseeds, fiber crops, and fruits and nuts. NREGS is a dummy and takes on the value one if districts are from phases 1 and 2; otherwise, zero. Each column reports the coefficients of interest from separate regressions. Marginal, small, medium, and large farmers are defined as owning less than 2.5 acres, 2.5-5 acres, 5-10 acres, and more than 10 acres, respectively. Household and year-fixed effects as well are included throughout. Regressions are also adjusted for season, season-by-year, state-by-year, and crop-by-season fixed effects. The vector of controls includes rainfall for each village and survey year. It also includes time-constant pre-treatment village-level controls such as distance to the nearest town, access to the local market, distance to district HQ, log of the population schools, etc., constructed from census 2001 and REDS 1999, interacted with year. Standard errors are clustered at the district level and reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Results in Table 1.5 suggest that NREGS helped increase the share of high-value crops grown by 3.1 percentage points. This is consistent with the notion of NREGS having enhanced farmers' liquidity and risk-bearing capacity Gehrke (2019). The size of estimated effects, especially for increases in high-value crops, is most pronounced among marginal and small farmers whose limited land endowment, together with the weakness of rental markets, may preclude output increases via expanding cultivated area.

1.6 Impacts of Public Goods Provision

A key potential advantage of NREGS as a public work rather than a cash transfer program is the scope for creating productivity-enhancing public good infrastructure such as watersheds, rural roads, land improvement, and minor irrigation works (see Figure 1.2). While they could generate high returns, the lack of data on the location and quality of such works and the rather unreliable nature of data on labor input provided by the NREGS management information system (Bose and Das, 2018) has traditionally made it difficult to study this issue.

We use primary data collected through the Socio-Economics Profile of India (SEPRI) survey- digitized from village records -that often are different from official MIS figures- for a subset of states in our sample. The data include detailed information on NREGS and contains information about the number of days spent under NREGS overall and by type of activity. We have data on employment generated and total investment (both capital and labor expenditure) in NREGS programs for 192 villages in 13 states. One way of assessing if public goods created under NREGS affect agricultural productivity is to include information on village-level employment generation and investment in the NREGS program as a shifter. To do so we estimated reduced form equation of the following form.

$$Y_{ivt} = \beta_0 + \beta_1 Infrastructure_{vt} + \lambda X_v \times Round_t + Z_{vt} + \mu_i + \phi_t + \epsilon_{ivt} \tag{1.2}$$

where Y_{ivt} is the log of output value per acre produced by farm i in village v at time t, $Infrastructure_{vt}$ is the log transformation of either employment generated under NREGS or per capita investment on NREGS-related projects at time t in village v. X is a vector of time-invariant controls that interacted with the survey round year. $Round$ is an indicator variable and takes on the value one if the survey round is 2015/16 and zero otherwise. Z is a vector of time-varying controls such as rainfall. μ_i is household fixed effects, ϕ_t is year fixed effects, season fixed effects, state-by-year fixed effects and ϵ_{ivt} is a white noise error term. The parameter of interest is β_1, which can be interpreted as the elasticity of agricultural output with respect to NREGS labor input (or investment per capita). It differs from the estimates provided earlier in that it reflects implementation arrangements and investment opportunities in a village and is thus more akin to an average treatment effect (a dose-response) rather than an intention to treat.

1.6.1 Results

We report the results estimated using equation 1.2 in Table 1.6. Panel A of Table 1.6 reports results for all farmers. In columns 1 and 2, We report the output elasticity with respect to investment per capita and employment generated. Since NREGS can induce demographic shifts, we use population from 2001 to convert expenditure into per capita. Results without controls are reported in columns 1 and 3 and with controls in columns 2 and 4. Results reported in Table 1.6 point towards a positive productivity effect of NREGS-related investment and employment days with an estimated elasticity of 0.037 (col. 2) to .018 (col. 4), contrary to what was found elsewhere (Deininger and Liu, 2019). In other words, We find that a 1 percent increase in NREGS related per capita investment increases agricultural productivity by 3.5-3.7 percent. While a one percent increase in total employment generated through NREGS, i.e., an increase of 42 days, increases agricultural productivity by 2 percent overall, which is equivalent to 853 Rs/ac. The results are robust to the inclusion of controls. If the program is implemented effectively, productivity improvements due to NREGS-generated public goods would thus recover some of the annual program costs.

1.6.1.1 By Farm Sizes

We further explore the heterogeneity in the impact of NREGS-related public goods investment. Panel B, C, D, and E report estimated results by farm size groups. The coefficients in Table 1.6 regarding the program effects from NREGS-provided public goods

Table 1.6 NREGS Related Investment and Productivity by Farm Size

	(1)	(2)	(3)	(4)
	Expenditure		Employment	
	Panel A: Overall			
Infrastructure	0.035**	0.037**	0.016*	0.018**
	(0.017)	(0.017)	(0.009)	(0.009)
Observations	12,949	12,949	12,949	12,949
R-squared	0.623	0.624	0.622	0.624
	Panel B: Marginal (<2.5 acre)			
Infrastructure	0.052**	0.061**	0.021	0.028*
	(0.024)	(0.029)	(0.015)	(0.016)
Observations	5,245	5,245	5,245	5,245
R-squared	0.674	0.680	0.673	0.679
	Panel C: Small (2.5-5 acre)			
Infrastructure	0.043***	0.048***	0.019*	0.027**
	(0.015)	(0.014)	(0.011)	(0.010)
Observations	3,198	3,198	3,198	3,198
R-squared	0.695	0.697	0.694	0.697
	Panel D: Medium (5-10 acre)			
Infrastructure	0.016	0.036**	0.013	0.021**
	(0.025)	(0.014)	(0.013)	(0.010)
Observations	2,541	2,541	2,541	2,541
R-squared	0.682	0.686	0.682	0.686
	Panel E: Large (>10 acre)			
Infrastructure	-0.001	-0.021	0.008	0.001
	(0.027)	(0.014)	(0.011)	(0.010)
Observations	1,348	1,348	1,348	1,348
R-squared	0.615	0.621	0.615	0.621
Controls		Y		Y

Note: Dependent variable is the log of output per acre throughout. *Infrastructure* is the log of per capita investment in NREGS projects such as watersheds, roads, etc., per person for columns 1 and 2, and NREGS generated total employment for columns 3 and 4. Each column reports the coefficients of interest from separate regressions. Household and year-fixed effects are included throughout. Regressions are also adjusted for season, season-by-year, state-by-year, and crop-by-season fixed effects. The vector of controls includes rainfall for each village and survey year. It also includes time-constant pre-treatment village-level controls such as distance to the nearest town, access to the local market, distance to district HQ, log of the population schools, etc., constructed from census 2001 and REDS 1999, interacted with year. Standard errors are clustered at the district level and reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1

do not significantly differ across farm size groups when using employment generated as the treatment variable. With an F-statistic of 1.48, we fail to reject the null hypothesis that the coefficients across all farm size groups are the same (col. 4). However, when we use NREGS-related per capita expenditure as our explanatory variable, we do find a significant difference in the incidence of productivity effects across farm types (with an F-statistic of 2.99). The estimated output elasticities with respect to total investment per capita are 6% for marginal farmers, 5% for small farmers, and 4% for medium farmers. The estimated coefficient is not statistically significant for large farmers. Moreover, the estimated output elasticities with respect to employment are 3% for marginal and small and 2% for medium size farmers.

1.7 Conclusions

We explore the short-term productivity effects of NREGS using a DID strategy on national farm-level data. We find weak support for aggregate productivity,i.e., estimated coefficients are imprecisely estimated, but strong evidence that NREGS increased agricultural productivity for marginal farms by 18 percent. The increase in productivity for marginal farmers appears to be due to the intensive use of family labor, a fact that is consistent with reduced migration and job search friction, mechanization, and more intensive use of irrigation. Given the nature and size of the program, such an indirect effect on agricultural productivity is large enough to recover a significant amount of program cost.

Furthermore, the positive effect of NREGS on agricultural productivity has the potential to stimulate additional economic activity. By increasing the demand for local non-tradable goods and services, the program can contribute to the growth of non-agricultural employment (Deininger et al., 2018; Emerick, 2018)), and promote ongoing development in rural areas (Cook and Shah, 2022). However, to what extent they will be sustained depend on various factors, such as the availability of investment opportunities for mechanization to replace labor and the functioning of the land market to facilitate the expansion of cultivated land. Moreover, local governance and supportive policies are essential to maintain the momentum of development and ensure that the positive impacts are sustained in the long term.

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APPENDIX

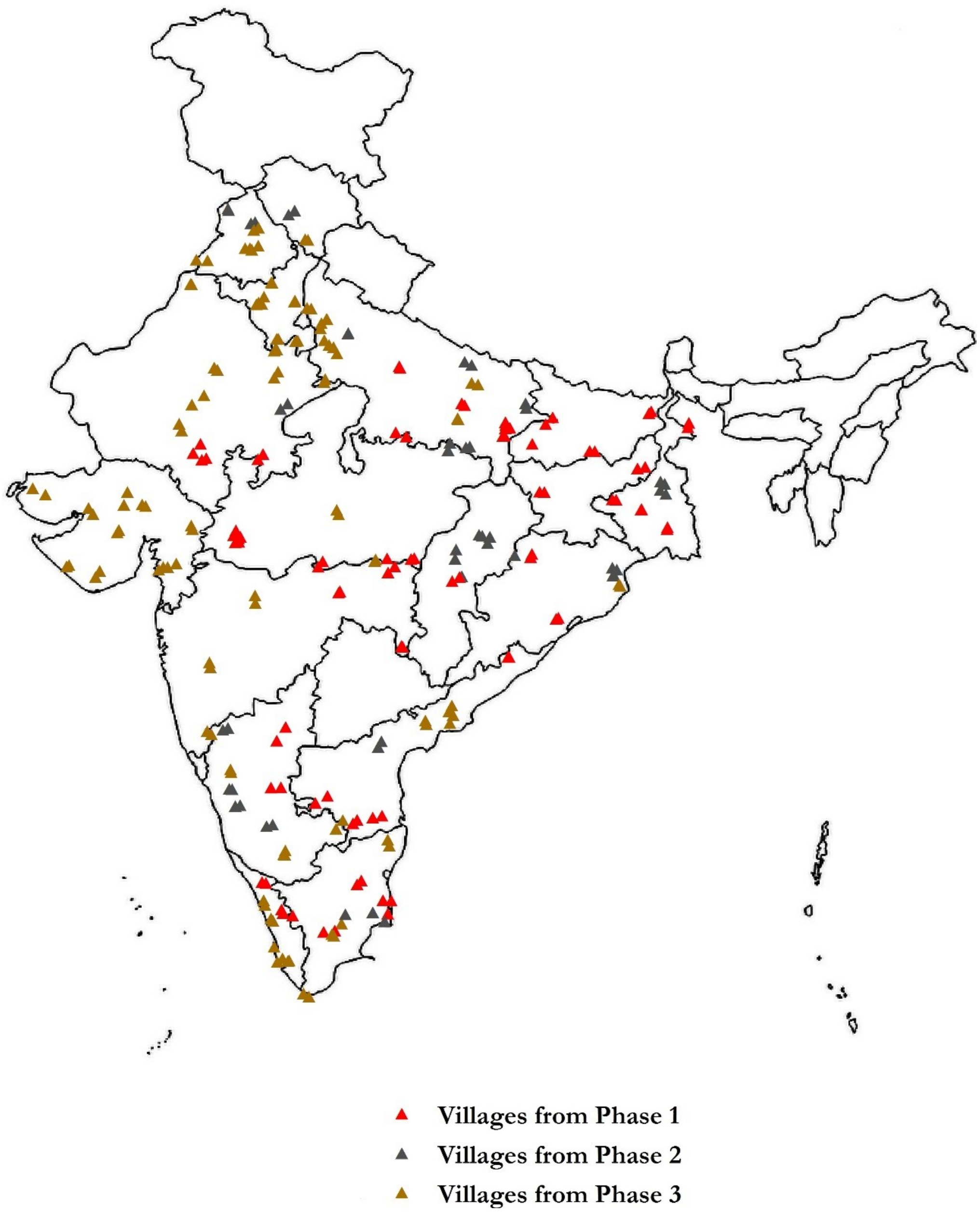

Figure 1.3 Location of Sample REDS Villages by Phase of NREGS Program Implementation

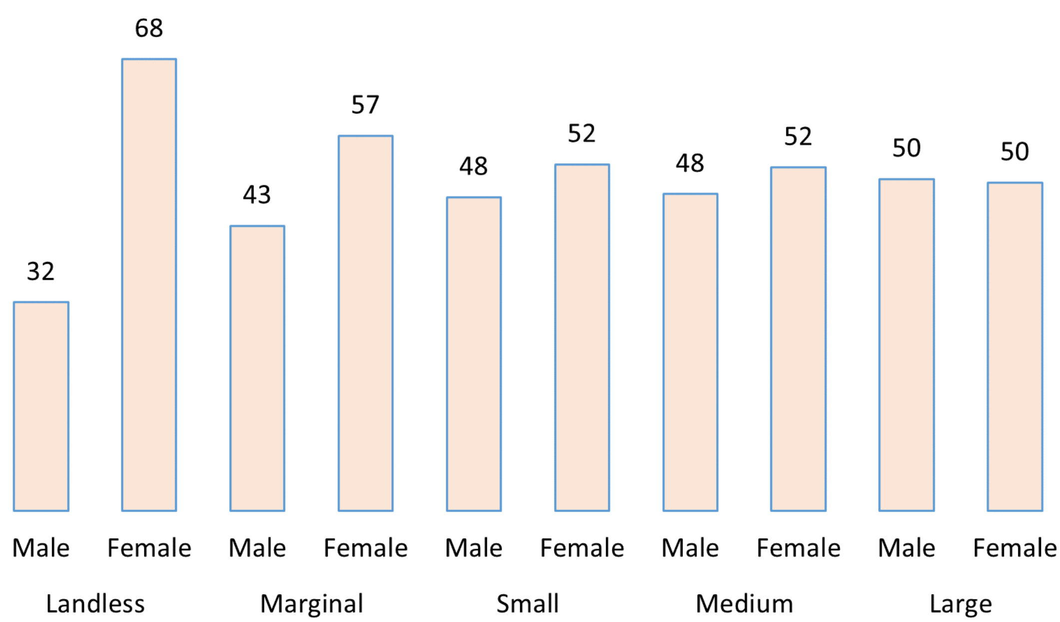

Figure 1.4 Gender Differences in NREGS Workers

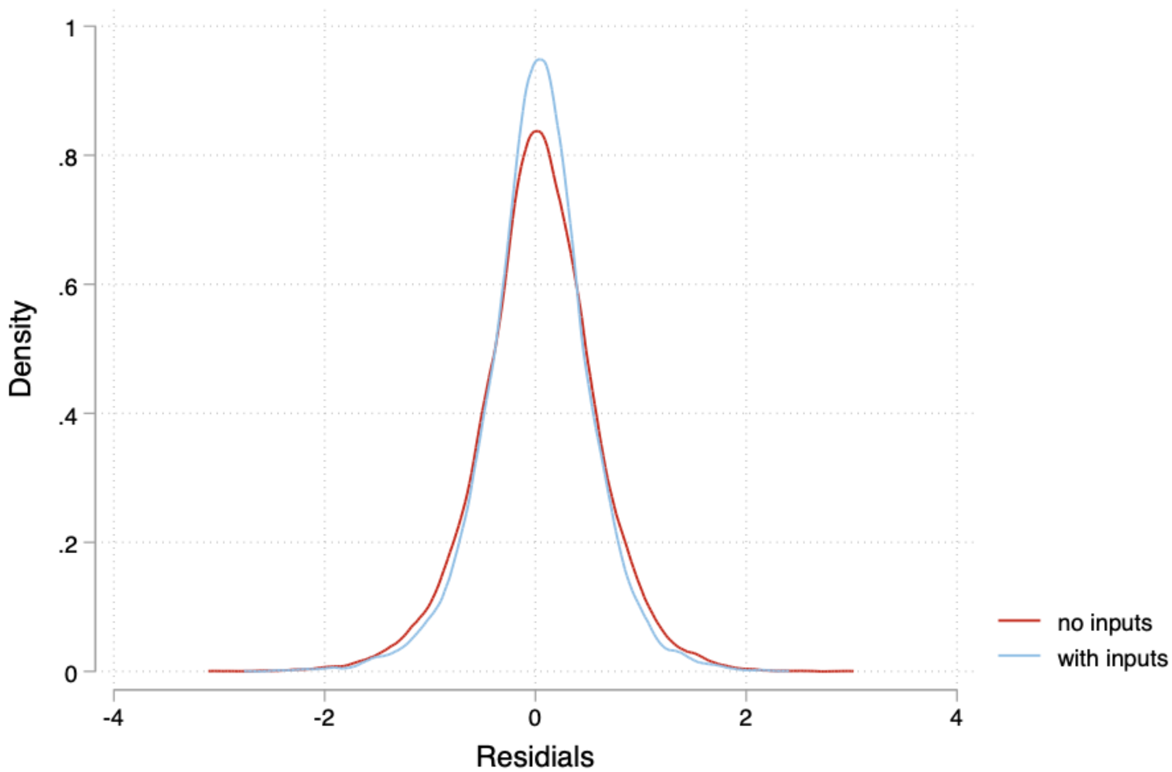

Figure 1.5 Residualized Value of Output Per Acre

Table A.1 Summary Statistics of Key Variables by Treatment Status and Year

	Control		Treatment		Year=1998-99		Year=2007-08	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Panel A: Household Characteristics								
Male head	0.95	0.22	0.94	0.24	0.96	0.19	0.93	0.26
head's age	50.82	13.21	51.29	13.06	49.76	13.35	52.54	12.71
Married head	0.90	0.31	0.89	0.31	0.90	0.30	0.88	0.32
Head'education	4.38	4.14	5.14	4.55	4.33	3.71	5.32	4.98
Household size	6.26	3.09	6.32	3.43	6.72	3.58	5.82	2.84
Working male	2.17	1.28	2.14	1.33	2.29	1.41	2.01	1.17
Working female	2.01	1.13	2.05	1.23	2.09	1.27	1.96	1.09
Total cropped area	8.10	9.39	6.69	10.79	7.21	9.34	7.44	11.09
Land owned	5.65	6.42	5.47	8.83	5.95	8.57	5.10	6.94
Irrigated land	3.86	5.65	2.69	4.76	3.30	5.47	3.12	4.90
Panel B: Agricultural Inputs and Output Per Acre								
Value (Rs.) per acre...								
Harvest value	9087.31	7805.25	7400.42	6400.45	7784.68	7049.41	8717.14	7253.44
Seed	724.49	1449.22	589.15	1117.20	425.58	785.04	928.03	1666.32
Manure	738.72	1926.10	543.63	1517.27	206.37	459.79	1153.88	2413.59
Machinery	314.74	368.17	226.09	327.79	206.45	290.74	343.07	398.35
Fertilizer	611.42	505.11	578.50	507.97	630.09	531.64	551.29	471.87
Irrigation	183.28	457.51	153.70	652.36	113.71	369.92	232.85	732.69
Bullock	49.34	152.96	49.03	147.56	55.39	157.02	41.73	141.17
Other inputs	256.04	470.68	134.34	307.40	257.11	470.99	115.29	269.73
Labor days per acre...								
Hired labor	16.07	21.43	20.51	24.07	16.21	20.90	20.98	24.94
Family labor	37.08	40.83	35.68	38.18	34.65	38.02	38.39	41.07
Total labor	55.65	52.32	58.40	50.15	53.11	47.45	61.84	55.02
Male hired labor	8.14	12.14	10.21	14.04	7.46	11.74	11.33	14.49
Female hired labor	7.17	11.88	9.17	12.98	7.73	12.00	8.80	13.06
Male family labor	29.07	32.12	29.27	31.26	25.60	28.43	33.47	34.69
Female family labor	12.50	17.41	11.03	15.41	11.91	15.63	11.53	17.31
Net seller of labor	0.36	0.48	0.40	0.49	0.40	0.49	0.36	0.48
Net buyer of labor	0.56	0.50	0.56	0.50	0.51	0.50	0.62	0.48
Share of high value crops	0.22	0.41	0.29	0.24	0.27	0.44	0.23	0.42

Note: Our own calculation from Rural Economics and Demographic Survey

Table A.2 Summary Statistics of Key Variables by Farm Size

	Marginal		Small		Medium		Large	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Panel A: Household Characteristics								
Male head	0.93	0.25	0.94	0.24	0.96	0.21	0.97	0.18
head's age	49.37	12.71	50.17	12.69	51.35	13.25	55.62	13.40
Married head	0.88	0.33	0.90	0.30	0.90	0.30	0.90	0.30
Head'education	4.35	4.19	4.48	4.29	5.03	4.61	5.89	4.40
Household size	5.85	2.91	6.04	2.96	6.33	3.09	7.54	4.25
Working male	1.96	1.17	2.06	1.23	2.21	1.30	2.61	1.56
Working female	1.84	1.08	1.95	1.11	2.06	1.15	2.48	1.44
Total cropped area	2.66	3.31	5.04	4.20	8.63	5.99	18.57	18.50
Land owned	1.74	1.93	3.51	2.09	6.31	3.85	15.45	14.06
Irrigated land	1.05	1.65	2.07	1.93	3.77	4.02	8.66	9.30
Panel B: Agricultural Inputs and Output Per Acre								
Net cropped area per season	1.24	1.46	2.02	1.94	3.21	2.80	5.84	6.36
Value (Rs.) per acre...								
Harvest value	8921.50	7569.69	8244.03	7275.13	7721.08	6320.51	7740.05	7257.26
Seed	797.00	1554.20	656.73	1314.09	591.36	1126.36	524.59	968.87
Manure	864.07	2116.29	713.48	1865.42	488.93	1377.30	400.93	1190.88
Machinery	321.58	377.62	282.06	353.58	263.01	341.71	184.98	298.11
Fertilizer	680.89	526.78	585.65	503.51	559.92	487.30	524.22	488.21
Irrigation	280.45	673.50	165.52	780.88	108.79	276.37	82.81	217.49
Bullock	71.87	180.82	53.96	158.65	32.25	113.99	31.48	122.42
Other inputs	156.99	394.63	185.08	382.97	205.58	386.39	236.34	431.42
Labor days per acre...								
Hired labor	18.54	24.66	18.68	23.78	18.92	22.35	17.17	19.91
Family labor	52.27	46.08	38.31	39.50	29.92	34.61	19.31	22.28
Total labor	73.74	56.94	58.88	50.61	50.16	46.48	39.78	40.40
Male hired labor	9.80	14.45	9.00	13.49	9.20	12.75	8.71	11.40
Female hired labor	7.66	12.97	8.59	12.80	8.79	12.69	7.86	11.15
Male family labor	40.37	36.04	30.15	31.14	24.18	27.45	18.24	24.49
Female family labor	16.46	19.55	12.55	16.23	9.71	14.10	6.54	11.76
Net seller of labor	0.58	0.49	0.44	0.50	0.27	0.44	0.17	0.37
Net buyer of labor	0.36	0.48	0.49	0.50	0.68	0.47	0.79	0.41
Share of high value crops	0.17	0.37	0.25	0.42	0.29	0.46	0.33	0.47

Note: Our own calculation from Rural Economics and Demographic Survey

Table A.3 Test of Parallel Pre-Trends for Key Outcome Variables Using REDS 1982-1999 Data

	(1)	(2)	(3)	(4)
VARIABLES	Cropped area	Crop Output	Crop Output/ac	Labor/ac
NREGS x Year(=1999)	1.185	0.196	-0.134	0.164
	(1.032)	(0.308)	(0.262)	(0.398)
	[0.253]	[0.526]	[0.610]	[0.682]
Observations	4,934	4,934	4,934	4,934
R-squared	0.778	0.716	0.732	0.686
	Machine/ac	Fertilizer/ac	Irrigation/ac	Seed/ac
NREGS x Year(=1999)	-0.210	0.311	-0.172	-0.122
	(0.469)	(0.324)	(0.365)	(0.207)
	[0.655]	[0.338]	[0.639]	[0.558]
Observations	4,934	4,934	4,934	4,934
R-squared	0.628	0.773	0.644	0.806
	Pesticide/ac	Manure/ac	Animal/ac	Other Inputs/ac
NREGS x Year(=1999)	-0.117	-0.268	0.204	0.080
	(0.410)	(0.291)	(0.354)	(0.380)
	[0.776]	[0.359]	[0.567]	[0.833]
Observations	4,934	4,934	4,934	4,934
R-squared	0.725	0.748	0.596	0.693

Note: NREGS is a dummy and takes on the value one if districts are from phases 1 and 2; otherwise, zero. Each column reports the coefficients of interest from separate regressions. Household and year-fixed effects as well are included throughout. Standard errors are clustered at the district level and reported in parentheses as well as associated p-value in the brackets; *** p<0.01, ** p<0.05, * p<0.1

Table A.4 Test of Parallel Pre-Trends by Farm Size Using REDS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
			Per Acre									
VARIABLES	Area	Output	Output/ac	Labor	Seed	Manure	Fertilizer	Pesticide	Irrigation	Animal	Machine	Other inputs
							Marginal					
NREGS x Year(=1999)	0.138	-0.398	-1.183	-0.084	-0.638	-0.615	-0.719	0.197	-0.922	0.440	-0.876	-0.649
	(0.152)	(0.963)	(0.995)	(1.065)	(0.655)	(0.654)	(0.683)	(0.664)	(0.945)	(0.743)	(1.128)	(0.739)
	[0.365]	[0.681]	[0.238]	[0.937]	[0.332]	[0.349]	[0.295]	[0.767]	[0.332]	[0.555]	[0.439]	[0.382]
Observations	1,564	1,564	1,564	1,564	1,564	1,564	1,564	1,564	1,564	1,564	1,564	1,564
R-squared	0.801	0.806	0.817	0.800	0.848	0.828	0.842	0.808	0.783	0.776	0.776	0.790
							Small					
NREGS x Year(=1999)	0.077	0.643	0.521	-0.141	0.463	0.535	0.995	0.000	0.058	0.178	0.219	0.459
	(0.249)	(0.679)	(0.596)	(0.967)	(0.649)	(0.816)	(0.922)	(0.959)	(0.961)	(1.109)	(1.059)	(0.810)
	[0.757]	[0.346]	[0.384]	[0.885]	[0.477]	[0.514]	[0.283]	[1.000]	[0.952]	[0.873]	[0.837]	[0.572]
Observations	1,130	1,130	1,130	1,130	1,130	1,130	1,130	1,130	1,130	1,130	1,130	1,130
R-squared	0.879	0.878	0.896	0.912	0.937	0.911	0.907	0.909	0.878	0.861	0.884	0.906
							Medium					
NREGS x Year(=1999)	0.118	0.222	0.104	0.196	-0.152	-0.686	0.377	-0.599	-0.318	-0.172	0.404	0.413
	(0.618)	(0.559)	(0.482)	(0.794)	(0.533)	(0.684)	(0.985)	(0.806)	(0.906)	(0.731)	(1.023)	(0.924)
	[0.849]	[0.692]	[0.830]	[0.806]	[0.776]	[0.319]	[0.703]	[0.459]	[0.726]	[0.815]	[0.694]	[0.656]
Observations	1,177	1,177	1,177	1,177	1,177	1,177	1,177	1,177	1,177	1,177	1,177	1,177
R-squared	0.859	0.913	0.927	0.893	0.947	0.919	0.912	0.923	0.860	0.849	0.863	0.886
							Large					
NREGS x Year(=1999)	2.313	-0.355	-0.407	0.296	-0.306	-0.123	0.282	-0.153	-0.635	0.290	-0.031	0.067
	(3.771)	(0.395)	(0.345)	(0.713)	(0.422)	(0.717)	(0.529)	(0.848)	(0.729)	(0.475)	(1.304)	(0.615)
	[0.541]	[0.370]	[0.242]	[0.679]	[0.471]	[0.864]	[0.596]	[0.857]	[0.386]	[0.543]	[0.981]	[0.914]
Observations	1,063	1,063	1,063	1,063	1,063	1,063	1,063	1,063	1,063	1,063	1,063	1,063
R-squared	0.829	0.942	0.948	0.871	0.936	0.866	0.936	0.878	0.813	0.731	0.738	0.879

Note: NREGS is a dummy and takes on the value one if districts are from phases 1 and 2; otherwise, zero. Each column reports the coefficients of interest from separate regressions. Household and year-fixed effects as well are included throughout. Standard errors are clustered at the district level and reported in parentheses as well as associated p-value in the brackets; *** p<0.01, ** p<0.05, * p<0.1

Table A.5 Test of Parallel Pre-Trends Using Cost of Cultivation Survey Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Per Acre					
VARIABLES	Wage	Crop Output	Crop Output	F. Labor	H. Labor	Machine	Fertilizer	Irrigation
NREGS x 2001-02	0.015	-0.021	-0.023	0.021	0.008	0.045	0.059	-0.045
	(0.016)	(0.054)	(0.037)	(0.052)	(0.073)	(0.049)	(0.078)	(0.103)
	[0.329]	[0.699]	[0.543]	[0.692]	[0.911]	[0.359]	[0.446]	[0.661]
NREGS x 2002-03	-0.008	0.043	0.044	-0.025	0.083	-0.013	0.128	0.054
	(0.022)	(0.078)	(0.054)	(0.083)	(0.127)	(0.081)	(0.145)	(0.138)
	[0.722]	[0.582]	[0.410]	[0.767]	[0.515]	[0.868]	[0.378]	[0.693]
NREGS x 2003-04	-0.015	-0.001	0.033	0.052	0.127	-0.018	-0.003	0.119
	(0.021)	(0.078)	(0.051)	(0.079)	(0.117)	(0.076)	(0.136)	(0.134)
	[0.477]	[0.988]	[0.526]	[0.511]	[0.279]	[0.817]	[0.981]	[0.373]
NREGS x 2004-05	-0.007	-0.010	0.021	0.041	0.163	0.025	0.135	0.152
	(0.021)	(0.072)	(0.050)	(0.080)	(0.120)	(0.078)	(0.128)	(0.140)
	[0.737]	[0.889]	[0.674]	[0.607]	[0.174]	[0.752]	[0.292]	[0.277]
Observations	123,614	139,211	139,211	139,211	139,211	139,211	139,211	139,211
R-squared	0.627	0.353	0.393	0.276	0.447	0.446	0.383	0.440

Notes: The cost of cultivation survey is administered by the Ministry of Agriculture, Government of India. The survey collects plot-level summaries for sub-districts. Each column presents separate regression using population weights; NREGS is a dummy for phase 1 or 2 districts. Dependent variables are measured in logs throughout, and units for casual wage, agricultural output, fertilizer, and manure are in Rs. whereas labor, machinery, and irrigation use are in hours. Each regression also includes crop fixed effects and state-by-year fixed effects, and standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.

Table A.6 Agricultural Labor Use by Male and Female

VARIABLES	(1) All	(2) Marginal	(3) Small	(4) Medium	(5) Large
	Male: Hired Labor Days				
NREGS	-1.41	-0.42	-3.33	-0.97	-2.40
	(1.76)	(2.22)	(2.55)	(1.98)	(1.86)
Observations	14,620	4,267	3,719	3,556	3,071
R-squared	0.59	0.59	0.61	0.62	0.58
	Female: Hired Labor Days				
NREGS	-3.15***	-4.09***	-4.72**	-0.81	-4.09**
	(1.12)	(1.45)	(1.83)	(1.81)	(1.62)
Observations	14,620	4,267	3,719	3,556	3,071
R-squared	0.61	0.62	0.63	0.63	0.58
	Male: Family Labor Days				
NREGS	3.65	12.72***	2.35	0.53	-7.03
	(4.13)	(4.39)	(5.76)	(4.27)	(4.24)
Observations	14,620	4,267	3,719	3,556	3,071
R-squared	0.57	0.59	0.54	0.54	0.54
	Female: Family Labor Days				
NREGS	-1.16	1.94	1.68	-2.05	-6.01***
	(2.27)	(2.27)	(3.30)	(2.15)	(2.15)
Observations	14,620	4,267	3,719	3,556	3,071
R-squared	0.58	0.64	0.54	0.53	0.53

Note: Dependent variable is labor days per acre throughout. NREGS is a dummy and takes on the value one if districts are from phases 1 and 2; otherwise, zero. Each column reports the coefficients of interest from separate regressions. Marginal, small, medium, and large farmers are defined as owning less than 2.5 acres, 2.5-5 acres, 5-10 acres, and more than 10 acres, respectively. Household and year-fixed effects as well are included throughout. Regressions are also adjusted for season, season-by-year, state-by-year, and crop-by-season fixed effects. The vector of controls includes rainfall for each village and survey year. It also includes time-constant pre-treatment village-level controls such as distance to the nearest town, access to the local market, distance to district HQ, log of the population schools, etc., constructed from census 2001 and REDS 1999, interacted with year. Standard errors are clustered at the district level and reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table A.7 No Effects on Total Cropped Area

VARIABLES	(1) All	(2) Marginal	(3) Small	(4) Medium	Large
			Total Cropped Area		
NREGS	-0.034	-0.147	-0.021	0.042	0.086
	(0.107)	(0.150)	(0.144)	(0.126)	(0.124)
Observations	14,620	4,267	3,719	3,556	3,071
R-squared	0.688	0.650	0.604	0.585	0.576

Note: NREGS is a dummy and takes on the value one if districts are from phases 1 and 2; otherwise, zero. Each column reports the coefficients of interest from separate regressions. Marginal, small, medium, and large farmers are defined as owning less than 2.5 acres, 2.5-5 acres, 5-10 acres, and more than 10 acres, respectively. Household and year-fixed effects as well are included throughout. Regressions are also adjusted for season, season-by-year, state-by-year, and crop-by-season fixed effects. The vector of controls includes rainfall for each village and survey year. It also includes time-constant pre-treatment village-level controls such as distance to the nearest town, access to the local market, distance to district HQ, log of the population schools, etc., constructed from census 2001 and REDS 1999, interacted with year. Standard errors are clustered at the district level and reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table A.8 Labor Supply Decision

	(1)	(2)	(3)	(4)	(5)
	Self Employed		Worked as Labor in Activities. . .		
VARIABLES	Agr.	Nonagr.	Daily wage	Salaried	HH
NREGS x landless	-0.013	-0.002	0.061*	0.002	-0.064
	(0.040)	(0.013)	(0.035)	(0.010)	(0.049)
NREGS x marginal	0.073**	-0.008	0.002	-0.003	-0.028
	(0.036)	(0.013)	(0.037)	(0.009)	(0.046)
NREGS x small	0.005	-0.006	0.017	-0.016*	-0.042
	(0.029)	(0.012)	(0.030)	(0.009)	(0.046)
NREGS x medium	-0.012	-0.018	0.023	0.006	-0.044
	(0.037)	(0.013)	(0.035)	(0.011)	(0.057)
NREGS x large	0.054	0.008	0.026	0.014	-0.043
	(0.049)	(0.012)	(0.021)	(0.012)	(0.053)
Observations	53,764	53,764	53,764	53,764	53,764
R-squared	0.452	0.322	0.461	0.253	0.555

Note: NREGS is a dummy and takes on the value one if districts are from phases 1 and 2; otherwise, zero. Each column reports the coefficients of interest from separate regressions. Household and year-fixed effects as well are included throughout. The vector of controls includes rainfall for each village and survey year. It also includes time-constant pre-treatment village-level controls such as distance to the nearest town, access to the local market, distance to district HQ, log of the population schools, etc., constructed from census 2001 and REDS 1999, interacted with year. Standard errors are clustered at the district level and reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table A.9 Estimated Productivity Effects Using Two-Stage DID Approach

VARIABLES	(1) All	(2) Marginal	(3) Small	(4) Medium	(5) Large
			Panel A: No Inputs		
NREGS	0.123**	0.184**	0.054	0.125*	0.134
	(0.061)	(0.073)	(0.074)	(0.068)	(0.111)
Observations	14,620	4,267	3,722	3,558	3,073
R-squared	0.007	0.011	0.003	0.009	0.010
			Panel B: With Inputs		
NREGS	0.115**	0.179***	0.050	0.115*	0.113
	(0.055)	(0.066)	(0.067)	(0.065)	(0.091)
Observations	14,620	4,267	3,722	3,558	3,073
R-squared	0.008	0.013	0.003	0.009	0.012

Note: Dependent variable is the adjusted log of output value per acre using Gardener's (2021) approach throughout. NREGS is a dummy and takes on the value one if districts are from phases 1 and 2; otherwise, zero. Each column reports the coefficients of interest from separate regressions. Clustered bootstrapped standard errors with 500 replications are reported in parentheses; *** p<0.01, ** p<0.05, * p<0.1

CHAPTER 2

EMPOWERING WOMEN: THE IMPACT OF FEMALE LEADERSHIP ON LABOR MARKET DYNAMICS

2.1 Introduction

Since the 1990s, India has experienced robust economic growth, declines in fertility, expansion of education, and improved access to infrastructure. These are all factors that are generally believed to be associated with sustained increases in female labor force participation (Klasen, 2019). Yet, India's female labor force participation rate was 48 percent in 1984, low by global standards to start with, and, in rural areas, declined further to 33 percent in 2012 (Andres et al., 2017). Reductions in rural women's labor force participation e.g. due to life cycle events or exogenous shocks that were intended to be temporary, often were difficult to reverse and led to permanent dropping out of the labor force (Sarkar, Sahoo, and Klasen, 2019). The effects of such shifts go well beyond foregone income as women's labor market participation status will affect their welfare as well as that of future generations through impacts on autonomy, outside options, and ability to invest in children's education and health (Afridi, Mukhopadhyay, and Sahoo, 2016). Women's labor market participation will thus reduce human and physical capital accumulation and India's ability to take advantage of its 'demographic dividend. Identifying ways to reverse or at least arrest this decline is thus a priority for policy (Fletcher, Pande, and Moore, 2017), especially in light of the devastation wrought by the COVID pandemic.

The economic literature identifies supply and demand factors as important determinants of labor market participation. Agricultural mechanization and manufacturing's rising capital intensity reduced female labor demand as many women lack the education and skills that

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would allow them to shift to higher-paying sectors. This interpretation and the importance of demand-side rationing are supported by women's strong response to workfare programs (Desai, 2018; Sarkar, Sahoo, and Klasen, 2019). On the other hand, some studies argue that social norms may have interacted with higher real wages in rural areas affected supply through an income effect (Mehrotra and Parida, 2017), a reaction that may be reinforced by changes in educated women's returns to home production vs. market participation (Afridi, Dinkelman, and Mahajan, 2018).

Beyond economic factors, laws and social norms affect female labor force participation. Regulations restricting factory work by women during certain hours, such as the 1948 Factory Act may reduce demand for female labor. In rural areas where the role of married women is widely perceived to be limited to taking care of domestic duties (Bernal, 2008), having married women work outside the home may reflect badly on their family (Eswaran, Ramaswami, and Wadhwa, 2013). This, together with the challenges it may pose to their role, may lead men to oppose their spouses participating in labor markets as reported from rural Madhya Pradesh (Bernhardt et al., 2018). Yet, social norms change only slowly (Kandpal and Baylis, 2019) and are closely aligned with broader shifts in culture, perception, and learning (Fernández, 2013) that often persist over time across generations (Dhar, Jain, and Jayachandran, 2019).

Recent studies suggest that social and economic change is most likely and sustainable if they affect demand and supply-side factors. Gender norms are closely linked to social attitudes (Dhar, Jain, and Jayachandran, 2022) that are not amenable to be changed through short-term interventions (Jensen, 2012) but can be modified through longer-term exposure (Dean and Jayachandran, 2019), as through initiatives that capitalize on other Government programs (Field et al., 2021). Yet, changes in such norms are likely to have far-reaching effects, including on the likelihood of labor force participation which, via the economic resources generated, could trigger a virtuous cycle of economic empowerment to reinforce changes in social norms.

A large body of literature has assessed the impact of reserving village leadership positions for women on the of village level resources across different types of public goods (Chattopadhyay and Duflo, 2004) and, possibly after incurring some learning costs (Afridi, Iversen, and Sharan, 2017), long-term outcomes. Studies suggest that by affecting gendered stereotypes and attitudes regarding women's ability (Beaman et al., 2012) the latter can give women a voice (Iyer et al., 2012), altering long-held beliefs on the status of girls vs. boys (Kalsi, 2017) and the importance of having adolescent girls enrolled in school (O'Connell, 2018) that can shift labor market outcomes including self-employment in the informal manufacturing sector (Ghani, Kerr, and O'Connell, 2014).

By providing predictable labor demand, the National Rural Employment Guarantee Scheme (NREGS) not only increased wages, especially for women (Azam, 2011), in the dry season (Imbert and Papp, 2015), and for the unskilled (Berg et al., 2014) but also reduced short-term migration (Imbert and Papp, 2020), encouraged diversification of cropping patterns (Gehrke, 2019), and reduced rural violence (Fetzer, 2020).

While India's combination of reservation of village leadership positions for women that aims to affect social norms with a direct increase in labor demand via workfare offers an opportunity to observe the interaction of supply and demand side factors, few studies have linked the two. In Uttar Pradesh, Bose and Das (2018) uses data from a large number of panchayats to show that having a female leader in a reserved position increased female interest in public works as measured by the number of job cards issued as well as actual demand for work under the National Rural Employment Guarantee Scheme (NREGS) but did not affect actual employment outcomes, possibly due to measurement error in the administrative data they use. In Andhra Pradesh, Afridi, Mukhopadhyay, and Sahoo (2016) use child-level panel data to show that shifts in female labor force participation brought about by NREGS improve children's educational outcomes but do not explicitly discuss reservation as a potential channel for such effects to materialize.

In this paper, we bring these two strands together more explicitly by using detailed individual data to assess the short- and medium-term impact of female reservation together with NREGS on female labor supply, identify mechanisms that might underpin such changes, and, for a sub-sample for which such data were collected, explore impacts on female empowerment. Identification relies on the fact that, in each period, villages to be reserved were randomly chosen. We analyze outcomes at the individual level by linking household data for India's 12 main states from the Rural Economics and Demographic Survey (REDS) to villages' reservation history. As our data were collected when NREGS was active, we can assess if exogenous exposure to female leadership improved women's ability to take advantage of the income-earning opportunities associated with this program, potentially catalyzing broader changes in their access to and use of resources. Moreover, by providing estimates of the impact of female reservation in the current and the previous local election period, we can assess longer-term effects on labor force participation, agency, demand for work, and involvement in household decision-making.

We find that female contemporaneous reservation of leadership for women affected local governance as measured by the quality of NREGS implementation but had no measurable impact on female labor supply. At the same time, female leadership reservation affected female labor supply to public workfare and private sector labor markets in the following period. Effects were quantitatively large (half a standard deviation) and most pronounced for married women. The past reservations also increased women's income, their demand for work, and -presumably due to increased access to resources- their participation in household decisions relating to spending on food items, health, and education.

This paper contributes to the literature in several respects: It points towards a role model effect as the avenue through which political reservation affects behavioral norms, women's economic participation, their control over resources, and their bargaining power.

Although a large part of this effect seems to be brought about by workfare participation (Deininger, Nagarajan, and Singh, 2020), there is also evidence of reservation-induced effects on participation in regular labor markets. We also add to the evidence regarding the impact of gender preferences, including quotas, by showing that, even if women leaders may lack experience or have to contend with male backlash (Gangadharan et al., 2016) so that pre-existing gaps cannot be fully closed (Iyer and Mani, 2019), politically empowering women can have positive effects in the medium term, consistent with the notion that agency problems may hinder female political participation (Casas-Arce and Saiz, 2015).

The rest of the paper is organized as follows. Section 2 describes institutional background and context by documenting the paradox of India's secular decline of female labor force participation and discusses the origin, nature, and evidence of the impact of the country's reservation policy as well as its Employment Guarantee Scheme. Section 3 describes data and estimation strategy, including balance tests to ascertain that random assignment of reservation status as mandated by legislation was indeed implemented. Section 4 presents results regarding the impacts of reservation on (i) female labor force participation (separately for NREGS-related and other employment) and the heterogeneity of these impacts by marital status and age; (ii) individual income, desire to work, and participation in household decision-making; and (iii) voice in terms of affecting the way NREGS is implemented and tests for their robustness. Section 5 concludes by discussing policy implications and suggestions for further research.

2.2 Background and Institutional Context

We show that despite favorable external conditions such as increased levels of income and education and declining fertility over the last decades, India's female labor force participation level has declined from an already low level. This is likely due to a combination of supply- and demand-side factors, including strong social norms. We discuss the background of political reservation and how reservation of local political leadership for women could possibly reverse this trend by altering social norms and, in interaction with other Government policies such as NREGS, generate mutually reinforcing feedback loops between economic and political empowerment.

2.2.1 India's Declining Female Labor Force Participation: Evidence and Policy Implications

A large literature explores the determinants and impact of female labor market participation within and across countries. Early studies often assumed that, due to growth-related changes in countries' economic structure, education, and fertility, labor force participation would increase and then possibly decline with income. While support for this hypothesis is weak (Gaddis and Klasen, 2014), female empowerment seems a key driver of labor force participation; from the 1970s, gender-friendly legal reforms consistently led to higher female labor force participation (Hyland, Djankov, and Goldberg, 2020) and greater female participation in legislative bodies is also found to be associated with higher female labor force participation (Lv and Yang, 2018).

India is characterized by some of the most glaring levels of gender inequality globally and very low female involvement in wage work. Although sustained growth of GDP, education, and access to key infrastructure (electricity, cooking gas, and piped water) vastly improved Indians' lives since the early 1990s, women's labor force participation stagnated in urban areas (Klasen and Pieters, 2015) and actually declined in rural ones. Drops in labor market participation have been pronounced after 2005 for those aged 15-24 years and married (Andres et al., 2017). While educated women may have decreased their labor market participation by choice, lack of opportunity and social stigma are key factors for the less educated.

Agricultural mechanization and increased capital intensity in manufacturing are argued to have limited opportunities for casual work by low-skilled females (Das et al., 2019). Lower labor force exit (Sarkar, Sahoo, and Klasen, 2019) and significantly increased workforce participation by women (Desai and Joshi, 2019) in response to workfare support this notion and point towards job creation, reinforced by overall growth Fletcher, Pande, and Moore (2017), as a key to higher female labor force participation (Chatterjee, Murgai, and Rama, 2015).

Social norms also affect female autonomy (Debnath, 2015). In central India, husbands have been shown to be opposed to their wives' taking up employment as this would reduce their social standing (Bernhardt et al., 2018). Changing such attitudes is difficult in the short term (Dean and Jayachandran, 2019) and requires a long-term approach (Jensen, 2012). Similarly, economically empowering women, e.g. via financial literacy training and transfer of wage payments to their own bank accounts, did help to increase their labor supply (Field et al., 2021).

2.2.2 Political Reservations and Females' Labor Market Outcomes

Together with a constitutional amendment to give more power to local Governments, reservation of village council leadership positions for women and scheduled castes (SCs) or tribes (STs) was introduced in India in 1993 to, among others, overcome long-standing inequalities and discrimination. The share of seats reserved for women was fixed at the state level. Unlike reservations for SCs, seats to be reserved for women are selected randomly in every election. Studies have shown that reservation-induced female leadership can directly alter the nature and quality of public goods supplied locally, e.g., by women leaders providing goods such as water and roads preferred by women Chattopadhyay and Duflo (2004). At the same time, their longer-term impact on creating role models (Beaman et al., 2009) may be equally or even more important as such role model effects can explain the effects of female leadership reservation on rates of breastfeeding and immunization, as well as higher child survival (Bhalotra and Clots-Figueras, 2014). Children's exposure to the reservation in utero or early in life has also been shown to be associated with better learning outcomes in primary school (Pathak and Macours, 2017) through role model effects.

Concerns have been raised that reserving leadership positions for females who lack relevant experience and connections may, in the short term, reduce the quality of program implementation (Afridi, Iversen, and Sharan, 2017), trigger male backlash (Gangadharan et al., 2016), or lead to females only standing in for their husbands. Yet, studies show that in the medium term, such issues may be sorted out, and the policy has considerable potential to affect a wide range of outcomes. Female leadership has been shown to increase the level and quality of women's political participation, their willingness to contribute to public goods, and their ability to hold leaders accountable (Deininger et al., 2015). Exposure to female leaders acting as role models triggered higher school enrollment by adolescent girls, 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 aspiration. Changes in beliefs regarding gender roles and greater voice by women are argued to be central reasons for the increased survival of higher-birth-order girls where local seats were reserved for women (Kalsi, 2017). Enhanced female participation in program oversight, civic engagement, and electoral participation in 'reserved' villages point towards potential complementarities between political and economic empowerment (Deininger, Nagarajan, and Singh, 2020).

The National Rural Employment Guarantee Scheme (NREGS) has been designed to expand the demand for unskilled work, especially among women. Building on the country's long tradition of food-for-work schemes (Dutta et al., 2012; Subbarao, 1997), this program guarantees up to 100 days per year to households with registered locally and established eligibility by obtaining a job card. Unskilled labor supplied by locals is expected to build productive assets (access roads, water harvesting structures, etc.) to increase agricultural productivity. NREGS explicitly encourages female participation by paying equal wages to men and women and requiring that a minimum share of work be performed by women.

While there is considerable heterogeneity in program implementation across states, e.g., the use of electronic payment of wages directly into beneficiaries' accounts (Muralidharan, Niehaus, and Sukhtankar, 2016), major program-induced effects have been confirmed in three areas. First, NREGS increased wages, especially for women (Azam, 2011), in the dry season Imbert and Papp (2015), and for the unskilled (Berg et al., 2014). Second, by providing a predictable source of income, it helped reduce seasonal short-term migration (Imbert and Papp, 2020), encouraged diversification of cropping patterns (Gehrke, 2019), and improved agricultural productivity Deininger et al. (2016). Finally, as the program is self-targeting, distributional effects have been largely positive: NREGS enhanced consumption (Bose 2017) and asset accumulation by the poor (Deininger et al., 2015). Through the provision of an effective safety net, it can reduce rural violence (Fetzer, 2020) and has been shown to have a positive impact on health Ravi and Engler (2015), primary school participation (Islam and Sivasankaran, 2015), learning outcomes in primary (Mani et al., 2014) although not secondary (Shah and Steinberg, 2021) school, gender-based violence (Amaral, Bandyopadhyay, and Sensarma, 2015).

Yet, despite far-reaching positive impacts (Duflo 2005; Iyer et al. 2012), the literature finds, at best tenuous links between political reservation and labor force participation. Using state-level data, Ghani et al. (2014) found that female reservation did not increase female manufacturing employment. A study closely related to ours is Bose and Das (2018), who use administrative data from 6,000 panchayats in the state of Uttar Pradesh to show that having a female leader increased the issuance of job cards and women's demand for NREGS work without affecting actual female employment. Detailed individual-level data on participation in labor markets and NREGS governance allow us to (i) assess gender-specific empowerment effects in greater detail, discerning in particular if reservation affected women's labor market participation beyond the increased labor demand by the NREGS program and expand the sample to cover more than a single state; (ii) reduce measurement error invariably associated with administrative data and control for covariates at the individual level; and (iii) explore potential synergies between a role model effect brought about by female leadership reservation and an empowerment effect arising from independent income earned through labor market participation.

2.3 Data and Empirical Approach

We use descriptive data to check for the balance in pre-program characteristics between ever and never reserved villages and differences in program-affected variables that, if allocation was random, can be interpreted as causal interpretation. Data are consistent with random allocation of reservation, suggest it brought to power leaders with less formal education, and point towards gender differences in the impact of reservation on labor market participation at the extensive and intensive margin

2.3.1 Data and Descriptive Statistics

To explore possible links between political and economic empowerment, we use individual data from a complete enumeration of all adult residents in 190 villages in 13 states implemented in 2014/15 as part of the long-running ARIS-REDS panel. Information was collected on 275,677 individuals in 91,984 households, of which 23,350, generally the most disadvantaged ones, had a job card allowing household members to apply for work under NREGS. As earlier studies have shown, access to job cards was not affected by a village's current or past reservation status (Deininger, Nagarajan, and Singh, 2020), and to obtain a conservative estimate of reservation-induced effects, our analysis focuses on these households.

In addition to standard demographic and socio-economic characteristics at individual and household levels, the survey obtained detailed information on actual and desired labor market participation at the individual level. Individuals who participated in NREGS were also asked about key program implementation features, including whether dated work receipts were issued, payment was deposited in beneficiaries' accounts, and wages were in line with regulations (or if not, whether a complaint was lodged). A village questionnaire was administered to, among others, elicit characteristics of all village leaders elected from 2005 together with election details, including if the election was 'reserved.' In a sub-sample of states with traditionally high levels of discrimination against women, an extra module was administered asking about individuals' involvement in key household-level decisions.

Following the constitutional mandate for female reservation, most states held local government elections in 5-year intervals, i.e. in 1995/96, 2000/01, and 2005/06. NREGS was rolled out in a phased manner starting in 2006 and administered by local governments that had been recently elected when NREGS was launched in 2006-2008. Another round of elections was held in 2010 or 2011, and the village council leaders elected then had just completed their terms when our data were collected. Under the assumption of pre-program balance, random assignment of female leadership reservation to villages provides an opportunity to assess if exposure to female leadership in the current or immediately preceding election period improved women's ability to take advantage of labor market opportunities in NREGS or the private sector although we are unable to analyze impacts of reservation and NREGS separately.

To check the balance in observables between treated and untreated villages before the reservation was mandated in 1993, supporting the notion of reservation having been assigned randomly so that results can be given a causal interpretation, we use 1991 Census data accessed via the Socioeconomic High-resolution Rural-Urban Geographic Platform (SHRUG) for India (Asher et al., 2021) together with 1990 Economic Census data. The latter is important as it differentiates the non-agricultural labor force by gender. We combine this with detailed information on individuals and village leaders from 2014/15. Household and village characteristics are reported in Tables B.1 and B.2 (see online appendix) separately for the entire sample (col. 1) and for villages that had or had not been reserved in the two previous election periods (cols. 2 and 3) with col. 4 reporting p-values for equality of means in the 2014/15 periods. None of the individual or household characteristics differs significantly between ever and never reserved villages, suggesting that the policy of random assignment of villages to female leadership was adhered to (please see the appendix for a detailed description of the data).

Table 2.1 presents information on individuals' actual and desired labor market participation, involvement in household decision-making, and if they participated in NREGS, program implementation, and governance with data for males in cols. 1-3 and for females in cols. 4-6. In line with the literature, data show that labor force participation rates and the number of days worked by men (87% participation with 189 days worked annually) exceed those for women (64% and 65 days). Significant gender differences are visible in the way labor days are allocated across sectors. Men spend close to 50% of their working time in non-agricultural casual employment, followed by agricultural self-employment (40%), casual labor in agriculture (34%), and salaried work (7%), and rather limited use of NREGS (23%) which accounts for less than 5% of their time. Women, by contrast, rely much more on employment in agriculture and workfare as they spend more than 60% of their time in agriculture (33% self-employed and 30% in casual labor), followed by NREGS (27%) and non-agricultural casual labor (10%). Such disproportional reliance on unskilled agricultural work makes women more susceptible to being displaced by agricultural mechanization (Mehrotra and Parida, 2017) with access to workfare possibly providing a safety net uptake of which could be affected by women's voice.

As these variables may be affected by female leadership reservation, tests for the significance of differences in cols. 4 and 8 are of interest. We find that reservation-induced effects are more pronounced for females than for males: while there is no difference in labor force participation for males between ever (88%) and never (87%) reserved villages, and males even work and earn significantly more in never (192 days and Rs. 66,000) vs. ever (186 days and Rs. 63,724) reserved villages, the opposite is true for women for whom labor force participation (68% vs. 59%), number of days worked per year (68 vs. 62), and total earnings (Rs. 22,490 vs. Rs. 19,804) are all significantly higher in ever vs. never reserved villages. At the same time, the willingness to work more is markedly higher for males and females in ever vs. never reserved villages. The difference is more prominent for women than for men (9.1 vs. 4.5 percentage points), possibly pointing toward greater rationing for female labor market participation (Desai, 2018).

The reservation also appears to affect adherence to program rules and, for indicators in which women were particularly disadvantaged, allowed them to achieve gender parity. In ever-reserved villages, the share of women who got a dated work receipt and were paid directly into their bank account increased from 62% to 68% and from 80% to 91%, respectively. Reservation does not seem to have affected the share of females who were underpaid (about 45% for ever and never reserved villages) and increased it for males (35% in never vs. 41% in ever reserved villages), though close to two-thirds of those who did not get paid the set amount did launch a complaint, much higher than those who did so in never reserved villages (39% of men, and 46% of women). For the smaller sample where such data was collected, evidence on involvement in decisions on food, non-food, health, and education suggests reservation led to significant, though quantitatively modest, increases in involvement in all these decisions by males as well as females; with 76% in ever vs 70% in never reserved villages, potential reservation-induced effects are largest for females' participation in education decisions.

Appendix Table B.1 panel A presents data on the 23,350 households with job cards and their 66,362 working-age members in sample villages. The average household includes 4.5 individuals, has a head who is aged 49 years, spent 3.8 years in school, is married in 85%, widowed in 13.6%, and female in 11.6% of cases. The data further show that 89% of sample households are Hindus, 42% belong to scheduled castes or tribes, 58% own agricultural land, and 48% have a proper (pucca) house. Panel B presents means at the individual level, highlighting that 21% had education at primary, 29% between primary and high school, and 11% above the high school level. None of the individual or household characteristics nor the timing of NREGS roll-out differs significantly between ever and never reserved villages, suggesting that the policy of random assignment of villages to female leadership was adhered to.

Table 2.1 Labor Force Participation by Reservation Status and Gender

	Men				Women			
	Total	Reservation status		Test	Total	Reservation status		Test
		Ever	Never			Ever	Never	
Panel A: Labor Supply								
Participated in labor market	0.877	0.882	0.872	0.008	0.638	0.682	0.590	0.000
... self-empl. in agric.	0.400	0.418	0.380	0.000	0.331	0.357	0.302	0.000
... self-empl. in non-agric.	0.057	0.051	0.064	0.000	0.013	0.012	0.014	0.053
... casual labor in agric.	0.340	0.353	0.326	0.000	0.303	0.315	0.291	0.000
... casual labor in non-agri.	0.502	0.501	0.504	0.387	0.099	0.103	0.094	0.010
... in NREGA	0.233	0.254	0.210	0.000	0.272	0.314	0.227	0.000
... regular salaried work	0.071	0.068	0.074	0.030	0.014	0.014	0.013	0.666
No of days worked	189.313	186.509	192.376	0.000	65.764	68.846	62.370	0.000
... self-empl. in agric.	20.197	22.095	18.122	0.000	12.490	14.537	10.238	0.000
... self-empl. in non-agric.	13.992	12.684	15.421	0.000	2.399	2.301	2.507	0.341
... casual labor in agric.	33.807	32.719	34.996	0.000	23.896	22.528	25.401	0.000
... casual labor in non-agri.	93.228	91.063	95.595	0.000	12.455	12.402	12.513	0.601
... in NREGA	6.672	7.525	5.740	0.000	10.943	13.461	8.171	0.000
... regular salaried work	21.417	20.423	22.503	0.013	3.581	3.617	3.540	0.974
Individual income (Rs.)	65,000	63,724	66,000	0.328	20,986	22,490	19,804	0.594
Would like to work more	0.274	0.296	0.251	0.000	0.316	0.359	0.268	0.000
If participated in NREGS work								
Got dated receipt	0.734	0.744	0.721	0.315	0.66	0.684	0.615	0.002
Paid directly to bank account	0.889	0.905	0.865	0.000	0.866	0.905	0.795	0.000
Was paid less than was due	0.388	0.412	0.35	0.001	0.454	0.453	0.463	0.459
If less, did complain	0.557	0.649	0.39	0.000	0.59	0.658	0.464	0.013
No. of obs.	33887	17694	16193		32475	17017	15458	
Panel B: Intra-Household Decision Making								
Participates in decisions on								
... food	0.655	0.669	0.638	0.002	0.839	0.851	0.824	0.037
... nonfood	0.828	0.835	0.819	0.138	0.761	0.769	0.751	0.423
... health	0.798	0.807	0.786	0.007	0.866	0.875	0.855	0.532
... education	0.854	0.862	0.844	0.495	0.737	0.763	0.704	0.001
No. of obs.	11,628	5,119	6,509		10,839	4,739	6,100	

Note: Author's own calculation from 2014/15 REDS follow-up survey. As discussed in the text, due to funding constraints data on intra-household decision-making was limited to the states of Gujarat, Uttar Pradesh, Maharashtra, Orissa, and West Bengal. To test differences in means, p values from regressions with district fixed effects and standard errors clustered by village panchayat are reported in the last column.

In Appendix Table B.2, village-level data from the 1991 Census (panel A) and the 1990 Economic Census (panel B) suggest that sample villages are typical of rural India with a population of 412 households (2,231 individuals of which 18% belonged to scheduled castes and 5% to scheduled tribes. Some 57% of villages can access a good road. An average village has about 1.4 elementary schools and 0.93 middle/high schools. About half of the residential buildings have access to electricity, and 40% of the agricultural area is electrified. Not surprisingly, there were limited nonfarm activities in 1990, which is supported by the fact that only 105.8 people (equivalent to 4.7% of the total population of an average village) worked in non-farm employment (panel B). Of the 105.8 employees working in the non-farm sectors, 89.9 were male, and 15.9 were female. None of these variables differ significantly between ever and never reserved villages. Pradhan characteristics in panel C suggest that, in ever-reserved villages, the share of pradhans who either held or contested the position of village leader before is slightly but not significantly lower in villages that had been reserved compared to those that had not. At the same time, we find significant differences in leaders' attributes between the two types of villages, consistent with the notion that female reservation opened the way for less educated non-Hindu leaders: while only 26% and 14% of leaders in ever reserved villages had secondary or, higher education and 48% were Hindus, corresponding figures for never reserved villages are 42%, 19%, and 64%, respectively.

2.3.2 Econometric Approach

To assess the impacts of political preferences 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 position reserved for a woman. Data on current and previous reservation status allows us to test for the persistence of such effects, i.e., if -in line with the notion that gender attitudes change slowly with individuals altering their attitude only after having been exposed to female leadership for some time (Beaman et al., 2012)- past reservation of a village for female leadership affects current outcomes. Synergies between political and economic empowerment (Deininger, Nagarajan, and Singh, 2020) would yield the same result. Letting v denote villages, i individuals, and t time, we assess the impacts of female reservation on outcome variables relating to individual i's labor force participation as well as other outcome variables by estimating the following equation.

$$Y_{iv} = \beta_0 + \beta_1 R_v^1 + \beta_2 R_v^2 + \beta_3 X_{iv} + \beta_4 V_v + \mu_d + \epsilon_{iv} \tag{2.1}$$

where Y_{iv} is the outcome variable of interest for individual i in village v, 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; X is a vector of household and individual controls; V is a vector of village and pradhan characteristics; μ_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 individual outcomes relative to the base category of a village having never been reserved.

Similarly, $\beta_1 + \beta_2$ denotes the impact of political leadership reservation in a village both in the present and the past, and it is straightforward to test for this joint impact either being significantly different from zero or from the estimated coefficients for the current and past reservation.

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

$$\begin{gathered} Y_i v = \beta_0 + \beta_1 R_v^1 + \beta_3 (R_v^1 \times f_{ivt}) + \beta_2 R_v^2 + \\ \beta_4 (R_v^2 \times f_{iv}) + \beta_5 X_{iv} + \beta_6 V_v + \mu_d + \epsilon_{iv} \end{gathered} \tag{2.2}$$

where parameters are as above, and the main difference here is that the parameters estimated are gender specific. In other words, β_1 and β_2 are the estimated impact of current or past reservations on men, and $\beta_1 + \beta_3$, as well as $\beta_2 + \beta_4$ are estimated impacts of current and past reservations on women. The significance of any linear combinations of estimated parameters can be tested via F-tests, which are reported in the results tables throughout.

An equivalent econometrics strategy that provides a more intuitive distinction between the effects of one-time vs. cumulative female reservation on individual $i's$ labor force participation and other outcome variables as above is:

$$\begin{aligned} Y_{iv} = \alpha_0 + \alpha_1 R_v^c + \alpha_2 R_v^p + \alpha_3 R_v^{cp} + \\ \alpha_4 X_{iv} + \alpha_5 V_v + \mu_d + \epsilon_{iv} \end{aligned} \tag{2.3}$$

where R_v^c, R_v^p, and R_v^{cp} are indicator variables of whether or not council leadership in village v was reserved for women in the most recent election only; the election previous to it; or both periods, respectively, and parameters of interest are α_1, α_2, and α_3, the estimated effect of current only, past only, both current and past reservation on individual outcomes relative to the base category of a village having never been reserved. In this specification, results of which are reported in the appendix tables, the gender dimension of the reservation are obtained by adding interaction terms between the respondent's gender and the reservation variables to (3)

$$\begin{aligned} Y_{iv} = \alpha_0 + \alpha_1 R_v^c + \gamma_1 (R_v^c \times f_{ivt}) + \alpha_2 R_v^p + \gamma_2 (R_v^p f_{ivt}) + \\ \alpha_3 R_v^{cp} + \gamma_3 (R_v^{cp} \times f_{ivt}) + \alpha_4 X_{iv} + \alpha_5 V_v + \mu_d + \epsilon_{iv} \end{aligned} \tag{2.4}$$

In this specification, α_1, α_2, and α_3 are the estimated impact of current only, or past only, or both current and past reservation on men, and $\alpha_1 + \gamma_1$, $\alpha_2 + \gamma_2$, as well as $\alpha_3 + \gamma_3$, are estimated impacts of current only, past only, and both current and past reservation on women. The significance of linear combinations of estimated parameters can be tested via F-tests, which are reported in the results tables throughout. The significance of linear combinations of estimated parameters can be tested via F-tests, which are reported in the results tables throughout.

2.4 Results and Discussion

Regressions at the household- and individual level suggest that reservation had no concurrent impact on female labor force participation but affected modalities of NREGS implementation, e.g., if work receipts were issued and those receiving less than the stipulated wage complained. Past reservation is estimated to have led to gains in female labor force participation at the extensive and intensive margins via a role model effect and associated increased female labor demand. Combined with an NREGS-induced shift in labor supply, it, in turn, triggered higher female labor market participation, leading to higher levels of income and intra-household bargaining power for women.

2.4.1 Impacts of Reservation on Female Labor Market Participation

Table 2.2 reports results from regressions of labor force participation without and with gender-differentiated effects that correspond to equations (1) and (2) in panels A and B, respectively. Beyond results for overall participation along the intensive (col. 1) and extensive (col. 4) margin, estimated coefficients are reported separately for NREGS-related activities (cols. 3 and 6) and all activities except NREGS (cols. 2 and 5). The equivalent specification with three indicators for the incidence of reservation as discussed above (equations (3) and (4)) is in Appendix Table B.3.

Concurrent reservation is estimated to have had no impact on participation at the extensive margin. At the intensive margin, there is some evidence (imprecisely estimated) that introduction of NREGS crowded out non-NREGS activities with a marginally significant increase in NREGS days (coefficient of 0.150 in col. 6 of panel A), substituting for a reduction in non-NREGS-related-labor supply (coefficient of -0.063 in col. 5). Differentiating by gender in panel B suggests that this is driven by a small contraction of male labor supply. Thus, we cannot reject the hypothesis that, during the reserved period, reservation has no impact on the extent or the intensity of overall female labor market participation.

By contrast, we find highly significant gender effects of reservation in the previous

Table 2.2 Estimated Effects of Political Reservation on Labor Supply at Extensive and Intensive Margin

	Participation			No. of days worked		
	Total	Other	NREGS	Total	Other	NREGS
			Panel A			
Reserved now β_1)	0.001	-0.009	0.035	-0.037	-0.063	0.150**
	(0.008)	(0.010)	(0.022)	(0.038)	(0.045)	(0.073)
Reserved before (β_2)	0.027***	0.025**	0.063***	0.168***	0.138***	0.202***
	(0.010)	(0.011)	(0.019)	(0.048)	(0.046)	(0.061)
Observations	66,362	66,362	66,362	66,362	66,362	66,362
R-squared	0.277	0.314	0.241	0.375	0.404	0.243
Test: (p values)						
F test ($\beta_1 + \beta_2 = 0$)	0.003	0.159	0.000	0.009	0.154	0.000
			Panel B			
Reserved now (β_1)	-0.016	-0.024	0.028	-0.116	-0.139	0.115
	(0.026)	(0.024)	(0.041)	(0.117)	(0.102)	(0.148)
Reserved before (β_2)	-0.029	-0.005	-0.019	-0.108	-0.013	-0.110
	(0.023)	(0.021)	(0.040)	(0.116)	(0.096)	(0.148)
Res now × fem (β_3)	0.035	0.030	0.015	0.164	0.159	0.076
	(0.049)	(0.045)	(0.062)	(0.228)	(0.190)	(0.225)
Res. before × fem (β_4)	0.118**	0.065	0.169**	0.574**	0.322	0.648**
	(0.046)	(0.043)	(0.072)	(0.228)	(0.196)	(0.276)
Observations	66,362	66,362	66,362	66,362	66,362	66,362
R-squared	0.282	0.316	0.248	0.379	0.405	0.254
Dep. Var Mean	0.75	0.69	0.25	3.65	3.324	1.466
... males	0.86	0.84	0.23	4.51	4.34	0.71
... females	0.62	0.51	0.27	2.69	2.11	0.92
Test: (p values)						
F test ($\beta_1 + \beta_2 = 0$)	0.116	0.283	0.855	0.146	0.226	0.983
F test ($\beta_3 + \beta_4 = 0$)	0.007	0.066	0.043	0.013	0.036	0.042
F test ($\beta_1 + \beta_3 = 0$)	0.480	0.791	0.200	0.700	0.847	0.106
F test ($\beta_2 + \beta_4 = 0$)	0.001	0.027	0.000	0.001	0.011	0.001
F test ($\beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$)	0.001	0.023	0.000	0.001	0.009	0.000

Note: 'Reserved now' and 'Reserved before' are indicator variables of whether village panchayats are reserved in the current or the previous panchayat periods and the sample is limited to NREGS job card holders. Control variables included throughout but not reported include household size, composition, land ownership, and the head's marital status, gender, age, and education; village-level access to road, distance to town and district HQ, population, share of SCs, STs, and key religions; years since the last village election; pradhan characteristics (education, caste, religion, previous tenure and candidacy for office) , district fixed effects and for individual-level regressions individuals' gender, marital status, age, education and their squared terms. Standard errors are clustered at village panchayat level Robust standard errors reported in parentheses and multiple hypotheses tests are adjusted using the Bonferroni method. p values of the F-test are reported in the table. $***p < 0.01$, $**\ p < 0.05$, $*p < 0.10$.

period: the likelihood of labor market participation overall is estimated to have increased by 2.7 percentage points or 3.6 percent at the mean, an effect comprised of estimated increases by 6.3 percentage points (25.2%) and 2.5 percentage points (3.6%) for NREGS and non-NREGS work, respectively (Table 2.2). A similarly significant effect -with an estimated size of 0.202 and 0.138 percentage points for NREGS- and non-NREGS-related work, equivalent to a 13.7% and 4.1% increase at the mean, respectively (cols. 6 and 5), emerges at the intensive margin.

Disaggregating these effects by gender (Table 2.2 panel B) highlights that virtually all long-term impacts can be attributed to changes in women's rather than men's labor market participation. F-tests in the bottom rows indicate that estimated impacts of past reservations on women's labor supply (β_2+β_4) are significant at the 1% level throughout. Past reservation is estimated to have led to an 8.9 percentage point increase in the likelihood of female labor force participation, equivalent to an 11.8% increase at the mean, comprised of estimated increases of 15 and 6 percentage points in women's likelihood of participating in NREGS and non-NREGS work, respectively. With 46 percentage points (12.7%) overall (col. 4) -31 percentage points (9.3%) for non-NREGS (col. 5), and 54 percentage points (37%) for NREGS work (col. 6)- estimated effects at the intensive margin are even larger.

While the evidence presented here is thus consistent with the lack of a reservation-induced impact on female participation in the wage labor force in the short term (Bose and Das, 2018; Ghani, Kerr, and O'Connell, 2014), in the medium term, it increased women's response to labor demand from NREGS and beyond. Estimated coefficients for non-NREGS work are significant throughout, consistent with the notion that, beyond potential effects on how workfare was provided and participation in NREGS work, reservation increased women's demand for wage work more broadly. While a cumulative income-induced empowerment effect of participation in NREGS could be consistent with these facts, we will below discuss evidence on changes in levels and quality of women's political participation in line with what was suggested by Deininger et al. (2015). It is suggestive that the reservation of panchayat leadership positions for women has provided a role model and thus performed an additional catalytic role.

2.4.2 Heterogeneity of Effects

If, as the literature suggests, the scope for labor market participation is particularly limited for married women (Eswaran, Ramaswami, and Wadhwa, 2013), reservation-induced effects may be more pronounced for this group, either by providing them with economic resources and social connections that they would not otherwise have access to or by helping to change their husbands' attitude to general gender roles and particularly female labor force participation Bernhardt et al. (2018). To test this, we run the above regressions separately for the sub-samples of married and unmarried individuals.

Results from doing so in Table 2.3 indeed support this notion, suggesting that estimated effects are consistently more significant and larger for married than for unmarried individuals: First, in contrast to insignificant aggregate effects of concurrent reservation on labor supply in the total sample, current reservation is estimated to increase married women's likelihood of labor force participation by 2.6 percentage points with marginal significance. The main channel for concurrent effects to materialize is via NREGS-related work, which is estimated to increase by 4.5 percentage points due to reservation (panel A, col. 3), largely by substituting for self-employment by males and, with a slightly smaller point estimate, females. Appendix Table B.4 includes the equivalent specification with three reservation indicators. Aggregate effects of current reservation on unmarried individuals' participation are insignificant (panel B col. 1): while the negative effect on self-employment (col. 2 and 3) is consistent with findings for married individuals, reservation has no significant effect on NREGS participation by unmarried ones, consistent with the notion that they have access to different opportunities in the labor market or different returns to work at home (Afridi, Dinkelman, and Mahajan, 2018).

Table 2.3 Estimated Effects of Reservation Status on Labor Force Participation by Marital Status

	Participation			No. of Days Worked		
	Total	Other	NREGS	Total	Other	NREGS
	Panel A: Married					
Reserved now (β_1)	-0.013	-0.024	0.037	-0.099	-0.122	0.150
	(0.032)	(0.031)	(0.051)	(0.143)	(0.130)	(0.181)
Reserved before (β_2)	-0.035	-0.008	-0.033	-0.131	-0.018	-0.166
	(0.027)	(0.027)	(0.050)	(0.136)	(0.124)	(0.181)
Res now × fem (β_3)	0.039	0.038	0.008	0.157	0.159	0.049
	(0.059)	(0.055)	(0.075)	(0.271)	(0.237)	(0.271)
Reserved before × fem (β_4)	0.126**	0.071	0.203**	0.613**	0.332	0.776**
	(0.054)	(0.053)	(0.085)	(0.267)	(0.246)	(0.325)
Observations	50,323	50,323	50,323	50,323	50,323	50,323
R-squared	0.285	0.338	0.248	0.419	0.464	0.249
DepMean	0.808	0.742	0.286	3.885	3.567	0.917
Test:						
F test ($\beta_1 + \beta_2 = 0$; p val)	0.144	0.344	0.945	0.182	0.361	0.945
F test ($\beta_3 + \beta_4 = 0$; p val)	0.008	0.082	0.048	0.017	0.078	0.045
F test ($\beta_1 + \beta_3 = 0$; p val)	0.386	0.638	0.241	0.673	0.767	0.133
F test ($\beta_2 + \beta_4 = 0$; p val)	0.003	0.049	0.000	0.001	0.030	0.001
F test ($\beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$; p val)	0.000	0.021	0.000	0.001	0.015	0.000
	Panel A: Unmarried					
Reserved now (β_1)	-0.026	-0.030	0.008	-0.168	-0.182*	0.030
	(0.018)	(0.019)	(0.021)	(0.103)	(0.107)	(0.073)
Reserved before (β_2)	-0.005	0.007	0.014	-0.007	0.041	0.024
	(0.021)	(0.018)	(0.021)	(0.115)	(0.102)	(0.078)
Res now × fem (β_3)	0.020	0.012	0.030	0.177	0.133	0.132
	(0.031)	(0.026)	(0.030)	(0.149)	(0.115)	(0.118)
Reserved before × fem (β_4)	0.079**	0.043	0.057	0.367*	0.236	0.233
	(0.038)	(0.029)	(0.038)	(0.187)	(0.143)	(0.155)
Observations	16,039	16,039	16,039	16,039	16,039	16,039
R-squared	0.256	0.265	0.273	0.282	0.286	0.300
DepMean	0.612	0.556	0.145	2.913	2.650	0.491
Test:						
F test ($\beta_1 + \beta_2 = 0$; p val)	0.251	0.336	0.410	0.231	0.260	0.596
F test ($\beta_3 + \beta_4 = 0$; p val)	0.051	0.116	0.077	0.035	0.033	0.073
F test ($\beta_1 + \beta_3 = 0$; p val)	0.789	0.392	0.092	0.938	0.639	0.046
F test ($\beta_2 + \beta_4 = 0{=}0$; p val)	0.004	0.024	0.010	0.008	0.016	0.014
F test ($\beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$; p val)	0.035	0.248	0.001	0.028	0.103	0.002

Note: 'Reserved now' and 'Reserved before' are indicator variables of whether village panchayats are reserved in the current or the previous panchayat periods and the sample is limited to NREGS job card holders. Control variables included throughout but not reported include household size, composition, land ownership, and the head's marital status, gender, age, and education; village-level access to road, distance to town and district HQ, population, share of SCs, STs, and key religions; years since the last village election; pradhan characteristics (education, caste, religion, previous tenure and candidacy for office) , district fixed effects and for individual-level regressions individuals' gender, marital status, age, education and their squared terms. Standard errors are clustered at village panchayat level Robust standard errors reported in parentheses and multiple hypotheses tests are adjusted using the Bonferroni method. $***p < 0.01, **p < 0.05, *p < 0.10$.

Second, past reservation is estimated to have had a gender-differentiated impact whereby a reduction in the likelihood of married males' participation -by 3.5 percentage points- is more than compensated for by an increase in married females' propensity to participate to yield a net increase of 9.1 percentage points overall due to female reservation. Disaggregating by type of labor suggests that most of this effect can be attributed to increased participation in NREGS activities, estimated to increase by 17 percentage points, versus a 6.3 percentage point gain in non-NREGS activities. The comparison of estimated elasticities at the intensive margin between NREGS and non-NREGS demonstrates an even large difference between the two types of job activities (0.66 for NREGS vs. 0.31 for non-NREGS activities).

By comparison, for unmarried individuals, past reservation is estimated to have had smaller effects that do not differ by gender and are less dominated by NREGS. For example, the past reservation would increase females' probability to participate in non-NREGS and NRGES by 5 percentage points and 7.1 percentage points, respectively. The gains in intensive margins are even more similar as the estimated effects for NREGS and non-NREGS activities are 0.26 and 0.27 percentage points, respectively.

2.4.3 Exploring Impact Pathways

To explore if reservation affected supply- or demand-side factors, we report effects on modalities of NREGS implementation that are likely to have affected the supply of jobs and women's bargaining power within the household separately.

Results from regressions (1) and (2) with the key indicators of program implementation in Table 2.4 (or the equivalent regressions in Appendix Table B.5) suggest that current and past reservations helped improve the quality of program implementation in several dimensions: The share of those who received a dated receipt for work performed under NREGS (col. 1) increased significantly during the reserved period and beyond (with elasticities of 27 percentage points (38%) and 50 percentage points (70%), respectively).

Table 2.4 Estimated Effects of Reservation on NREGS Governance

	Get dated receipt	Payment to account	Payment less than assessed	If less, did complain	Complaint addressed
			Panel A		
Reserved now (β_1)	0.298***	0.032	-0.057	0.152***	0.071**
	(0.039)	(0.030)	(0.043)	(0.034)	(0.027)
Reserved before (β_2)	0.740***	0.227***	-0.530***	0.313***	0.188***
	(0.041)	(0.034)	(0.033)	(0.035)	(0.031)
Observations	6,712	6,712	6,712	6,712	6,712
R-squared	0.605	0.698	0.386	0.254	0.199
Test:					
F test ($\beta_1 + \beta_2 = 0$; p val)	0.453	0.322	0.492	0.430	0.394
F test ($\beta_1 + \beta_2 = 0$; p val)	0.000	0.000	0.000	0.000	0.000
			Panel B		
Reserved now (β_1)	0.292***	0.037	-0.049	0.169***	0.089***
	(0.037)	(0.029)	(0.039)	(0.031)	(0.024)
Reserved before (β_2)	0.736***	0.227***	-0.535***	0.316***	0.188***
	(0.043)	(0.035)	(0.033)	(0.035)	(0.029)
Res now × fem (β_3)	0.019	-0.018	-0.036	-0.061**	-0.070**
	(0.019)	(0.021)	(0.036)	(0.025)	(0.028)
Reserved before × fem (β_4)	0.039*	-0.011	0.023	-0.051***	-0.033
	(0.022)	(0.008)	(0.040)	(0.018)	(0.021)
Observations	6,712	6,712	6,712	6,712	6,712
R-squared	0.606	0.699	0.387	0.256	0.201
Dep. var mean	0.712	0.882	0.409	0.245	0.192
Test:					
F test ($\beta_1 + \beta_2 = 0$; p val)	0.000	0.000	0.000	0.000	0.000
F test ($\beta_3 + \beta_4 = 0$; p val)	0.061	0.266	0.837	0.002	0.012
F test ($\beta_1 + \beta_3 = 0$; p val)	0.000	0.587	0.200	0.027	0.673
F test $\beta_2 + \beta_4 = 0$; p val)	0.000	0.000	0.000	0.000	0.002
F test ($\beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$)	0.000	0.002	0.000	0.000	0.043

Note: 'Reserved now' and 'Reserved before' are indicator variables of whether village panchayats are reserved in the current or the previous panchayat periods and the sample is limited to NREGS job card holders. Control variables included throughout but not reported include household size, composition, land ownership, and the head's marital status, gender, age, and education; village-level access to road, distance to town and district HQ, population, share of SCs, STs, and key religions; years since the last village election; pradhan characteristics (education, caste, religion, previous tenure and candidacy for office) , district fixed effects and for individual-level regressions individuals' gender, marital status, age, education and their squared terms. Standard errors are clustered at village panchayat level Standard errors are clustered at village panchayat level. Robust standard errors reported in parentheses and multiple hypotheses tests are adjusted using the Bonferroni method. $***p < 0.01, **\ p < 0.05, *p < 0.10$.

The likelihood of lodging complaints in case of under-payment also increased in the reserved period (with an elasticity of about 27 percentage points, or 47% at the mean level), though no longer thereafter (col. 4). Significant lagged effects are observed for an increased likelihood of wages being paid directly into beneficiaries' account (col. 2) with an estimated elasticity of 23 percentage points (25.8%); the likelihood of complaints for underpayment being addressed (col. 6 with an elasticity of 24 percentage points (57.3%)) and possibly as a result, a reduction in the likelihood of under-payment (col. 4). While reservation undeniably improved program governance and enhanced females' ability to access jobs under NREGS, none of these effects are gender-specific; to the contrary, for some, mainly lodging and response to complaints, regression results suggest that women still lag behind men, consistent with the notion that changes in social norms are not instantaneous.

Results from regressions in Table 2.5 (equivalent to Appendix Table B.6) allow us to explore if reservation increased women's demand for work as well as their income and bargaining power. As one would expect, if reservation relaxed constraints to female labor supply, it triggers significant lagged increases in women's individual income, estimated to have increased by some 76 percentage points (8.4%), and females' (but not males') demand for work by some 16 percentage points. Although not available for the entire sample, data on intra-household bargaining power support the notion of a role-model effect of reservation having, with a lag, led to higher levels of female autonomy: The share of women who participate in decision-making on food, health, and education is estimated to have increased by 18, 15, and 8 percentage points, respectively.

We conclude that, beyond improving the supply of suitable and attractive jobs for females, reservation enhanced female decision-making autonomy and their potential and actual participation in the labor force. A plausible interpretation of this evidence is that the role model effect provided by female leaders had an enduring effect that enhanced women's ability to take advantage of changing labor demand, irrespective of whether such demand

Table 2.5 Reservation and Women's Participation in Households' Day to Day Decision Making

VARIABLES	Income	Work More	Participation in Household Decisions on			
			Food	Nonfood	Health	Education
Reserved now (β_1)	0.009	0.014	0.013	-0.005	0.000	0.018
	(0.226)	(0.044)	(0.039)	(0.038)	(0.031)	(0.025)
Reserved before (β_2)	-0.221	0.005	-0.029	0.143**	0.085*	0.044
	(0.211)	(0.047)	(0.045)	(0.054)	(0.045)	(0.032)
Res now × fem (β_3)	0.072	0.025	-0.094	-0.004	-0.014	-0.001
	(0.425)	(0.059)	(0.063)	(0.045)	(0.025)	(0.028)
Reserved before × fem (β_4)	0.983**	0.163**	0.204***	-0.009	0.057**	0.040*
	(0.388)	(0.071)	(0.048)	(0.042)	(0.022)	(0.023)
Observations	66,362	66,362	22,467	22,467	22,467	22,467
R-squared	0.286	0.251	0.277	0.220	0.229	0.290
Dep. Var Mean	9.118	0.296	0.733	0.781	0.816	0.336
Test:						
F test ($\beta_1 + \beta_2 = 0$)	0.399	0.724	0.713	0.000	0.001	0.009
F test ($\beta_3 + \beta_4 = 0$)	0.009	0.046	0.098	0.800	0.135	0.212
F test ($\beta_1 + \beta_3 = 0$)	0.747	0.302	0.068	0.833	0.693	0.561
F test ($\beta_2 + \beta_4 = 0$)	0.003	0.000	0.000	0.016	0.002	0.014
F test ($\beta_1 + \beta_2 + \beta_3 + \beta_4 = 0$)	0.002	0.000	0.036	0.002	0.000	0.000

Note: 'Reserved now' and 'Reserved before' are indicator variables of whether village panchayats are reserved in the current or the previous panchayat periods. Regressions for the desire to work in column 2 and individual income in column 1 include the entire sample whereas those for intra-household bargaining are limited to the states of Gujarat, Uttar Pradesh, Maharashtra, Orissa, and West Bengal where a supplemental questionnaire on intra-household bargaining was administered. Control variables included throughout but not reported include household size, composition, land ownership, and the head's marital status, gender, age, and education; individuals' gender, marital status, age, education, and their squared terms; village-level access to road, distance to town and district HQ, population, the share of SCs, STs, and key religions; years since the last village election; pradhan characteristics (education, caste, religion, previous tenure, and candidacy for office) and district fixed effects. Standard errors are clustered at the village panchayat level Robust standard errors are reported in parentheses and multiple hypotheses tests are adjusted using the Bonferroni method. $***p < 0.01, **p < 0.05, *p < 0.10$.

came from Government programs with preferential treatment for females or not.

2.5 Conclusion

Motivated by the recent decline of female labor force participation in India, this paper explores if the random reservation of political leadership positions for women affects women's labor force participation as well as supply- and demand-related factors. While there is no contemporaneous effect, past leadership reservations for women significantly increased females' labor supply by allowing individuals to join the labor force and increasing the amount of time spent working by those already in work.

While a large part of the observed effects is attributable to females' improved ability to take advantage of public workfare under NREGS, female participation in non-NREGS labor markets (especially non-agricultural casual and self-employment) expands as well. Estimated effects are stronger for married than for unmarried women. Labor force participation allows women to obtain higher levels of individual income, increases their demand for work, and affects bargaining power by enhancing their participation in intra-household decision-making on spending for consumption, health, and education. Avenues to enhance these effects by combining them with the targeted provision of information and training to change norms regarding women's labor force participation and equip them with the skills to adapt to changing labor market conditions are a priority area for further research.

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APPENDIX

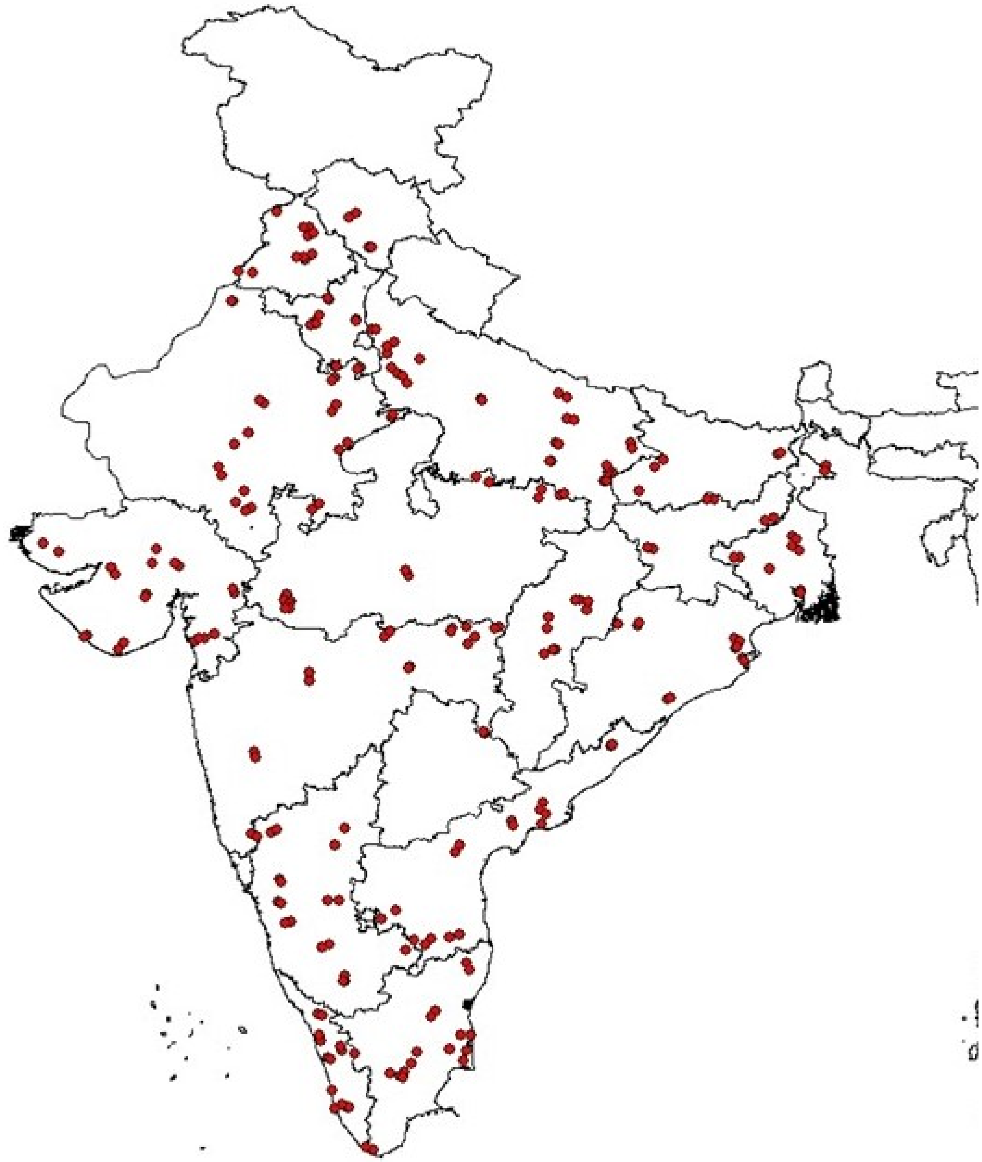

Figure 2.1 Location of Sample Villages

Table B.1 Household and Individual Characteristics in Ever and Never Reserved Villages

	Total	Reservation status Ever	Reservation status Never	Test (p-values)
		Panel A: Household characteristics		
Female head	0.116	0.115	0.117	0.138
Head's age	49.200	49.300	48.900	0.748
Head's education	3.800	3.830	3.750	0.593
Head married	0.848	0.842	0.855	0.588
Head widowed /separated	0.136	0.143	0.128	0.252
Household size	4.480	4.480	4.470	0.428
Members 15-65 years	1.650	1.660	1.630	0.489
Members 15-65 years	1.560	1.570	1.550	0.363
Children <15 years	1.080	1.070	1.110	0.496
Female children <15 years	0.530	0.520	0.540	0.440
Max. educ. in hh (years)	14.260	14.420	14.070	0.144
Hindu	0.888	0.882	0.894	0.907
SC/ST	0.419	0.404	0.438	0.882
Owns agricultural land	0.579	0.599	0.555	0.034
Has pucca house	0.476	0.483	0.467	0.043
No. of obs.	23,350	12,678	10,672	
		Panel B: Individual characteristics		
Female	0.490	0.491	0.489	0.184
Age	39.800	39.900	39.700	0.108
Educ. primary.	0.213	0.208	0.218	0.723
Educ. up to high school	0.291	0.291	0.292	0.019
. . . up to graduate	0.110	0.116	0.102	0.111
Others	0.015	0.015	0.014	0.513
Married	0.748	0.742	0.754	0.115
Unmarried	0.174	0.176	0.172	0.268
No. of obs.	66,362	34,707	31,655	

Note: Author's own calculation from 2014/15 REDS follow-up survey. To test difference in means, p values from regressions with district fixed effects and standard errors clustered by village panchayat are reported in the last column.

Table B.2 Village and Pradhan Characteristics in Ever and Never Reserved Villages, 1990/1991, and 2014/2015

	Total	Reservation status		Test
		Ever	Never	(p-values)
	Panel A: 1991 Census Information			
No. of households	412	443	379	0.676
Population	2231	2400	2058	0.804
Scheduled caste pop.	410	439	380	0.702
Scheduled tribe pop.	115	105	125	0.699
Literate population	840	917	762	0.871
No. of primary schools	1.398	1.522	1.267	0.214
No. of middle schools	0.551	0.597	0.507	0.813
No. of high schools	0.320	0.435	0.200	0.158
No. of secondary schools	0.061	0.088	0.034	0.282
Good road	0.574	0.567	0.581	0.591
Bad road	0.614	0.633	0.593	0.412
Cultivated area (ha.)	565.2	590.4	533.9	0.614
Habitations w. access to electricity	0.503	0.521	0.487	0.921
Agriculture w. access to electricity	0.433	0.373	0.486	0.735
	Panel B: 1990 Economic Census Information			
Non-farm employment	105.80	115.10	96.80	0.60
of which male	89.90	97.10	82.80	0.64
of which female	15.90	17.90	14.00	0.44
of which in manufacturing	67.40	79.10	56.10	0.30
of which in services	38.40	36.00	40.70	0.81
Observations	176	90	86	
	Panel C: 2014/15 REDS Information			
Earlier contested	0.158	0.137	0.179	0.088
Held position before	0.474	0.442	0.505	0.087
Up to high school	0.263	0.263	0.263	0.486
High sec. & above	0.342	0.263	0.421	0.007
Higher education	0.163	0.137	0.189	0.29
SC	0.537	0.579	0.495	0.583
ST	0.116	0.116	0.116	0.458
OBC	0.126	0.105	0.147	0.838
OC	0.216	0.2	0.232	0.735
Hindu	0.563	0.484	0.642	0.629
Muslim	0.089	0.084	0.095	0.147
No. of obs.	190	95	95	

Note: Author's own calculation from 2014/15 REDS follow-up survey. Numbers in panel A and B are calculated from The Socioeconomic High-resolution Rural-Urban Geographic Platform for India (SHRUG). To test difference in means, p values from regressions with district fixed effects and standard errors clustered by village panchayat are reported in the last column.

Table B.3 Estimated Effects of Reservation on NREGS Governance

	Participation			No. of Days Worked		
	Total	Other	NREGS	Total	Other	NREGS
			Panel A			
Res. now only (α_1)	-0.002	-0.018	0.062**	-0.031	-0.075	0.246**
	(0.013)	(0.016)	(0.028)	(0.060)	(0.071)	(0.095)
Res. before only (α_2)	0.024***	0.015	0.095***	0.176***	0.123**	0.315***
	(0.009)	(0.012)	(0.023)	(0.047)	(0.056)	(0.073)
Res. now and before (α_3)	0.030**	0.024	0.070**	0.124*	0.086	0.256***
	(0.013)	(0.015)	(0.028)	(0.072)	(0.064)	(0.090)
Observations	66,362	66,362	66,362	66,362	50,592	66,362
R-squared	0.277	0.314	0.241	0.375	0.404	0.244
Test:						
(α_1=(α_2)=(α_3)=0)	0.198	0.118	0.550	0.0103	0.0245	0.743
(α_1=(α_2)==0)	0.147	0.0892	0.822	0.158	0.118	0.930
(α_2=(α_3)==0)	0.694	0.638	0.455	0.538	0.653	0.569
			Panel B			
Res. now only (α_1)	-0.015	-0.044	0.096**	-0.078	-0.192	0.377**
	(0.033)	(0.032)	(0.047)	(0.144)	(0.133)	(0.165)
Res. before only (α_2)	-0.024	-0.043	0.106***	-0.027	-0.121	0.388***
	(0.032)	(0.030)	(0.030)	(0.159)	(0.144)	(0.100)
Res. now and before (α_3)	-0.045	-0.012	-0.041	-0.249	-0.109	-0.188
	(0.030)	(0.027)	(0.057)	(0.164)	(0.125)	(0.211)
Res. now only x female (γ_1)	0.026	0.054	-0.068	0.096	0.238	-0.269
	(0.063)	(0.056)	(0.068)	(0.279)	(0.230)	(0.237)
Res. before only x female (γ_2)	0.099	0.121**	-0.023	0.418	0.507*	-0.149
	(0.065)	(0.061)	(0.042)	(0.329)	(0.299)	(0.149)
Res. now and before x female (γ_3)	0.157***	0.077	0.231**	0.777***	0.421*	0.921**
	(0.055)	(0.050)	(0.096)	(0.295)	(0.225)	(0.375)
Observations	66,362	50,592	66,362	66,362	50,592	66,362
R-squared	0.282	0.316	0.255	0.379	0.406	0.263
Test:						
$\gamma_1 = \gamma_2 = \gamma_3 = 0$	0.124	0.615	0.013	0.099	0.646	0.009
$\gamma_1 = \gamma_2 = 0$	0.319	0.329	0.445	0.355	0.402	0.544
$\gamma_1 = \gamma_3 = 0$	0.042	0.700	0.005	0.032	0.474	0.003
$\gamma_2 = \gamma_3 = 0$	0.377	0.491	0.005	0.319	0.786	0.003

Note: 'Reserved now only' and 'Reserved before only' are indicator variables of whether village panchayats are reserved in the current or the previous panchayat periods. "Reserved now and before" is an indicator variable if panchayats were reserved in both elections. The sample is limited to those who are eligible to work under NREGS. Control variables are included throughout but coefficients that are not reported include household size, composition, land ownership, and the head's marital status, gender, age, and education; village-level access to road, distance to town and district HQ, population, share of SCs, STs, and key religions; years since the last village election; pradhan characteristics (education, caste, religion, previous tenure, and candidacy for office) and for individual-level regressions individuals' gender, marital status, age, education, and their squared terms. Robust standard errors reported in parentheses and multiple hypotheses tests are adjusted using the Bonferroni method. $***p < 0.01, **p < 0.05, *p < 0.10$.

Table B.4 Reservation and Labor Force Participation by Marital Status and Types of Employment

	Participation			No. of Days Worked		
	Total	Other	NREGS	Total	Other	NREGS
			Panel A: Married			
Res. now only (α_1)	-0.019	-0.056	0.117**	-0.100	-0.239	0.461**
	(0.042)	(0.040)	(0.058)	(0.180)	(0.165)	(0.204)
Res. before only (α_2)	-0.044	-0.069*	0.115***	-0.133	-0.245	0.427***
	(0.038)	(0.037)	(0.037)	(0.190)	(0.175)	(0.123)
Res. now and before (α_3)	-0.044	-0.005	-0.057	-0.228	-0.044	-0.251
	(0.032)	(0.033)	(0.069)	(0.178)	(0.153)	(0.252)
Res. now only x female (γ_1)	0.045	0.075	-0.086	0.158	0.306	-0.341
	(0.075)	(0.068)	(0.085)	(0.336)	(0.287)	(0.291)
Res. before only x female (γ_2)	0.139*	0.160**	-0.015	0.614	0.680*	-0.133
	(0.075)	(0.073)	(0.051)	(0.383)	(0.354)	(0.180)
Res. now and before x female (γ_3)	0.162***	0.077	0.266**	0.770**	0.369	1.054**
	(0.059)	(0.061)	(0.113)	(0.320)	(0.275)	(0.437)
Observations	50,323	36,802	50,323	50,323	36,802	50,323
R-squared	0.285	0.339	0.255	0.419	0.465	0.261
Test:						
$\gamma_1 = \gamma_2 = \gamma_3 = 0$	0.282	0.496	0.0174	0.237	0.615	0.0111
$\gamma_1 = \gamma_2 = 0$	0.281	0.307	0.340	0.280	0.341	0.406
$\gamma_1 = \gamma_3 = 0$	0.114	0.982	0.00564	0.0968	0.848	0.00353
$\gamma_2 = \gamma_3 = 0$	0.756	0.282	0.00876	0.704	0.418	0.00449
			Panel A: Unmarried			
Res. now only (α_1)	-0.005	-0.018	0.037	-0.022	-0.069	0.141*
	(0.022)	(0.025)	(0.025)	(0.120)	(0.133)	(0.085)
Res. before only (α_2)	0.041*	0.033	0.071***	0.290**	0.250*	0.247***
	(0.024)	(0.026)	(0.019)	(0.142)	(0.147)	(0.062)
Res. now and before (α_3)	-0.042	-0.031	0.003	-0.270*	-0.230*	-0.020
	(0.029)	(0.025)	(0.029)	(0.159)	(0.129)	(0.111)
Res. now only x female (γ_1)	-0.031	-0.015	-0.021	-0.096	-0.020	-0.069
	(0.033)	(0.029)	(0.030)	(0.157)	(0.125)	(0.121)
Res. before only x female (γ_2)	-0.033	-0.016	-0.056*	-0.243	-0.110	-0.215**
	(0.042)	(0.041)	(0.031)	(0.209)	(0.203)	(0.108)
Res. now and before x female (γ_3)	0.126**	0.073**	0.114**	0.687***	0.469***	0.471**
	(0.050)	(0.036)	(0.049)	(0.251)	(0.177)	(0.208)
Observations	16,039	13,790	16,039	16,039	13,790	16,039
R-squared	0.257	0.265	0.276	0.283	0.287	0.304
Test:						
$\gamma_1 = \gamma_2 = \gamma_3 = 0$	0.005	0.053	0.003	0.002	0.017	0.003
$\gamma_1 = \gamma_2 = 0$	0.950	0.970	0.292	0.451	0.665	0.214
$\gamma_1 = \gamma_3 = 0$	0.002	0.023	0.007	0.001	0.008	0.010
$\gamma_2 = \gamma_3 = 0$	0.004	0.062	0.001	0.001	0.018	0.001

Note: ‘Reserved now’ and ‘Reserved before’ are indicator variables of whether village panchayats are reserved in the current or the previous panchayat periods and the sample is limited to those who worked under NREGS. Control variables are included throughout but coefficients that are not reported include household size, composition, land ownership, and the head’s marital status, gender, age, and education; village-level access to road, distance to town and district HQ, population, the share of SCs, STs, and key religions; years since the last village election; pradhan characteristics (education, caste, religion, previous tenure, and candidacy for office) and for individual-level regressions individuals’ gender, marital status, age, education, and their squared terms. Robust standard errors reported in parentheses and multiple hypotheses tests are adjusted using the Bonferroni method. $***p < 0.01, **p < 0.05, *p < 0.10$.

Table B.5 Estimated Effects of Reservation on NREGS Governance

	Get dated receipt	Payment to account	Payment less than assessed	If less, did complain	Complaint addressed
			Panel A		
Res. now only (α_1)	0.106***	-0.012	0.048	0.145***	0.061**
	(0.034)	(0.027)	(0.052)	(0.033)	(0.029)
Res. before only (α_2)	0.300***	0.126***	-0.287***	0.297***	0.166***
	(0.029)	(0.027)	(0.033)	(0.029)	(0.028)
Res. now and before (α_3)	0.961***	0.242***	-0.544***	0.462***	0.255***
	(0.077)	(0.062)	(0.065)	(0.065)	(0.055)
Observations	6,712	6,712	6,712	6,712	6,712
R-squared	0.605	0.698	0.386	0.254	0.199
Test:					
α_1=(α_2)=(α_3)=0	0.000	0.000	0.000	0.000	0.000
α_1=(α_2)==0	0.000	0.000	0.000	0.000	0.000
α_2=(α_3)==0	0.000	0.003	0.000	0.000	0.003
			Panel B		
Res. now only (α_1)	0.101***	-0.006	0.059	0.166***	0.084***
	(0.033)	(0.027)	(0.048)	(0.030)	(0.025)
Res. before only (α_2)	0.308***	0.132***	-0.274***	0.316***	0.187***
	(0.032)	(0.027)	(0.027)	(0.026)	(0.024)
Res. now and before (α_3)	0.950***	0.247***	-0.542***	0.483***	0.274***
	(0.078)	(0.062)	(0.061)	(0.063)	(0.051)
Res. now only x female (γ_1)	0.007	-0.022	-0.052	-0.069**	-0.084**
	(0.021)	(0.025)	(0.040)	(0.031)	(0.034)
Res. before only x female (γ_2)	-0.013	-0.027	-0.046	-0.087***	-0.089***
	(0.025)	(0.024)	(0.035)	(0.024)	(0.029)
Res. now and before x female (γ_3)	0.064**	-0.027	-0.005	-0.107***	-0.096***
	(0.030)	(0.024)	(0.063)	(0.029)	(0.033)
Observations	6,712	6,712	6,712	6,712	6,712
R-squared	0.606	0.699	0.387	0.256	0.202
Test:					
$\gamma_1 = \gamma_2 = \gamma_3 = 0$	0.007	0.707	0.666	0.159	0.849
$\gamma_1 = \gamma_2 = 0$	0.210	0.471	0.795	0.367	0.766
$\gamma_1 = \gamma_3 = 0$	0.036	0.444	0.378	0.097	0.596
$\gamma_2 = \gamma_3 = 0$	0.002	0.999	0.389	0.093	0.623

Note: 'Res. now only' and 'Res. before only' are indicators of whether village panchayats are reserved in the current or the previous panchayat periods. "Reserved now and before" is an indicator variable if panchayats were reserved in both elections. The sample is limited to those who are eligible to work under NREGS. Control is included throughout but coefficients that are not reported include household size, composition, land ownership, and the head's marital status, gender, age, and education; village-level access to road, distance to town and district HQ, population, the share of SCs, STs, and key religions; years since the last village election; pradhan characteristics (education, caste, religion, previous tenure, and candidacy for office) and for individual-level regressions individuals' gender, marital status, age, education, and their squared terms. Robust standard errors reported in parentheses and multiple hypotheses tests are adjusted using the Bonferroni method. $***p < 0.01, **p < 0.05, *p < 0.10$.

Table B.6 Estimated Effects of Reservation on Decision Making

VARIABLES	Income	Work more	Participation in Household Decisions on Food	Nonfood	Health	Education
Res. now only (α_1)	-0.084	0.065	-0.011	-0.077*	-0.054	0.021
	(0.287)	(0.048)	(0.046)	(0.039)	(0.034)	(0.033)
Res. before only (α_2)	-0.376	0.112***	-0.052	0.036	0.013	-0.006
	(0.257)	(0.039)	(0.048)	(0.052)	(0.050)	(0.033)
Res. now and before (α_3)	-0.135	-0.014	0.034	0.240***	0.178***	0.102**
	(0.262)	(0.062)	(0.046)	(0.040)	(0.034)	(0.040)
Res. now only x female (γ_1)	0.154	-0.061	-0.122	0.006	-0.032	-0.074**
	(0.547)	(0.065)	(0.081)	(0.048)	(0.030)	(0.032)
Res. before only x female (γ_2)	1.173**	-0.034	0.165***	0.050	0.054	0.068*
	(0.479)	(0.042)	(0.057)	(0.040)	(0.036)	(0.038)
Res. now and before x female (γ_3)	1.009**	0.236**	0.124**	-0.030	0.040*	-0.022
	(0.388)	(0.099)	(0.060)	(0.042)	(0.023)	(0.044)
Observations	66,362	66,362	22,571	22,571	22,571	22,571
R-squared	0.286	0.256	0.260	0.207	0.216	0.187
Test:						
$\gamma_1 = \gamma_2 = \gamma_3 = 0$	0.225	0.0114	0.000316	0.128	0.00733	0.00319
$\gamma_1 = \gamma_2 = 0$	0.106	0.601	6.33e-05	0.332	0.0194	0.000731
$\gamma_1 = \gamma_3 = 0$	0.128	0.00450	0.000730	0.454	0.00235	0.267
$\gamma_2 = \gamma_3 = 0$	0.740	0.00343	0.323	0.0450	0.635	0.0773

Note: 'Res. now only' and 'Res. before only' are indicators of whether village panchayats are reserved in the current or the previous panchayat periods. "Reserved now and before" is an indicator variable if panchayats were reserved in both elections. The sample is limited to those who are eligible to work under NREGS. Regressions for the desire to work in column 2 and individual income in column 1 include the entire sample whereas those for intra-household bargaining are limited to the states of Gujarat, Uttar Pradesh, Maharashtra, Orissa, and West Bengal where a supplemental questionnaire on intra-household bargaining was administered. Control is included throughout but coefficients that are not reported include household size, composition, land ownership, and the head's marital status, gender, age, and education; village-level access to road, distance to town and district HQ, population, the share of SCs, STs, and key religions; years since the last village election; pradhan characteristics (education, caste, religion, previous tenure, and candidacy for office) and for individual-level regressions individuals' gender, marital status, age, education, and their squared terms. Robust standard errors reported in parentheses and multiple hypotheses tests are adjusted using the Bonferroni method. $***p < 0.01, **p < 0.05, *p < 0.10$.

CHAPTER 3

IDENTITY-BASED POLITICAL CONNECTIONS AND PUBLIC EMPLOYMENT

3.1 Introduction

A growing literature suggests that identity-based political and social connections play an important role in the distribution of public goods and services. The political connections between politicians and beneficiaries who share similar identities can have both positive and negative impacts on the local labor market and economy. While identity-based political connections can help ensure that historically marginalized groups have a voice in decision-making processes (Jaspal, 2011; Sankaran, Sekerdej, and Von Hecker, 2017), which can lead to a more equitable distribution of wealth and social mobility (Cassan, 2019; Deshpande, 2018, 2019; Hoff, Kshetramade, and Fehr, 2011), affect hiring practices and can facilitate the exchange of information in the absence of efficient market institutions (Schmutte, 2016; Munshi and Rosenzweig, 2015).

It may also run the risk of perpetuating group favoritism. Politicians may use their network and identity for preferential targeting (Besley et al., 2004; Chattopadhyay and Duflo, 2004; Banerjee and Somanathan, 2007), and reward those who actively support their party and candidacy (Besley et al., 2004; Chau, Liu, and Soundararajan, 2018; Moser, 2008; Sheahan et al., 2016). Such favoritism may distort the allocation of resources and jobs and disproportionately favor specific groups of local citizens over others (Colonnelli, Prem, and Teso, 2020; Gille, 2018; Markussen and Tarp, 2014), which can lead to increasing group inequality, misallocation of resources (Hsieh et al., 2019; Colonnelli, Prem, and Teso, 2020), and corruption (Bardhan and Mookherjee, 2005).

Identity and connections are particularly important in rural India, where caste, religion, and ethnicity play a crucial role in shaping political dynamics and resource distribution (Chattopadhyay and Duflo, 2004; Banerjee and Somanathan, 2007). In recent years, identity-based political connections have become an important aspect of politics, reflecting the interests and concerns of various social and demographic groups in the allocation of resources in India (Gaikwad, 2022). However, the role of political connections in public employment remains relatively understudied. We study the effects of identity-based political connections in India's largest national workfare program, the National Rural Employment Guarantee Scheme (NREGS). The program offers an interesting case study to examine the effects of identity-based political connections, as local leaders possess significant discretion in the allocation of benefits. However, estimating the effects of political connections is challenging due to the non-random election of local leaders, which we overcome by leveraging the unique caste (*jati*) system in India[1].

We constructed a measure of political connections between local leaders, including village council chairs and members, and individuals based on a shared identity determined by their surnames. In India, surnames typically reflect people's caste, although they can vary across different states and regions. To ensure accurate matching, we carried out the matching procedure conditional at the village, district, and state levels. We then proceed to estimate the labor-supply effects of political connections in NREGS.

Matching surnames enables us to compare individuals of different castes within the same caste group and village, providing valuable insights into social dynamics and relationships[2]. We also adjust for village-level unobservables and caste (*jati*) compositional differences across villages by including village dummies and controls at the level of caste-by-village. While this approach helps to mitigate potential bias, it does not completely eliminate the concerns about endogeneity. Some unobservable factors at the caste (*jati*) level, such as political influence within villages and caste-level hierarchy, could still be correlated

[1]Jati and caste are used interchangeably

[2]Caste groups such as Scheduled Caste (SC), Scheduled Tribe (ST), and Other Backward Castes (OBC) are designations used by the Indian government for affirmative action; caste-group membership determines reservations for jobs and elections. Caste is a much finer delineation than caste group, which usually subsumes many sub-castes.

with both NREGS participation and political leadership. Therefore, we also employ an instrumental variables (IV) approach in the main regression analysis. In this context, we utilize political reservations for lower-caste groups, which are mandated by constitutional amendments 73 and 74. These amendments require a share of the panchayat chairperson seat to be reserved for lower caste groups (SC/ST) that is proportional to the state population share of the lower caste group. The panchayats are then selected on a rotational basis for the reservation[3].

The main results suggest that political connection based on shared caste identity with local leaders increased individual labor supply on both the intensive and extensive margins. The estimates suggest an 11.5 percent increase in labor supply on the extensive margin. The effect increases to 17 percent when reservation policy is used as an instrument. We also find an increase in the number of days worked by individuals when local leaders are of the same caste. The increase in labor supply on the intensive margin mirrors the increase on the extensive margin. We find evidence that results are partially driven by favoritism by local leaders.

Related literature NREGS is a decentralized program where the implementation is carried out at the village level, with the village-council head playing a crucial role in the distribution of public works and ensuring effective oversight of projects in consultation with the elected council members. Since the power to distribute jobs rests with the local leaders, their identities—gender, caste, religion—are likely to affect programs, and the political connec-

[3]The details are given at https://pib.gov.in/PressReleaseIframePage.aspx?PRID=1658145. The following is the exact description of the clause. "As per clause (4) of Article 243D of the Constitution, the positions of Chairpersons within Panchayats, whether at the village level or any other tier, are mandated to be reserved for Scheduled Castes, Scheduled Tribes, and women. This reservation is to be carried out in the manner prescribed by the State Legislature, and it is specified that the number of Chairperson positions reserved for Scheduled Castes and Scheduled Tribes in Panchayats at each level in any State should, to the best extent possible, be proportional to the total number of such positions in the Panchayats at each level. This proportion is to be determined based on the population of Scheduled Castes in the State or Scheduled Tribes in the State in relation to the overall population of the State. Moreover, it is further stipulated that a minimum of one-third of the total number of Chairperson positions at each level in the Panchayats must be reserved for women. Additionally, the allocation of positions reserved under this clause is to be rotated among different Panchayats at each level".

tions of these local leaders can have an impact on the distribution of jobs in NREGS, which may also allow local politicians to reap political gains (Bardhan and Mookherjee, 2000) through allocation. Local leaders might prefer to allocate some resources to their own kind, who have similar preferences (Chattopadhyay and Duflo, 2004) and shared identity [4].

This paper makes a significant contribution to the extensive literature on political economy by examining the role of political connections, specifically focusing on the effects of caste identity in public works programs in India that are not yet studied in the literature. This paper contributes to the studies on reelection incentives and political targeting that have shed light on the motivations and strategies employed by politicians to secure their positions in office (Besley and Case, 1995; Besley et al., 2004; Chau, Liu, and Soundararajan, 2018; Persson, Roland, and Tabellini, 2000). Research on identity and representation that has explored the influence of identity factors, such as gender and ethnicity, on political processes and labor market outcomes (Beaman et al., 2012; Chattopadhyay and Duflo, 2004; Cassan, 2015; Oh, 2019). And, political connections and allocation, which have demonstrated how political networks and affiliations impact resource distribution(Choi, Penciakova, and Saffie, 2021; Fafchamps and Labonne, 2017; Gille, 2018; Gagliarducci and Manacorda, 2020; Caeyers and Dercon, 2012; Markussen and Tarp, 2014).

The paper also builds on previous findings by Besley et al. (2004) and Kumar, Somanathan et al. (2017), who have demonstrated the importance of political connections and the role of caste identity in the intra-village allocation of public goods and the distribution of benefits from social programs, respectively. However, this paper goes a step further by providing novel evidence on how political connections formed around caste identity impact

[4]If the local leaders are elected from historically minority groups, they can also generate demand effects in public work via role model channel (Chattopadhyay and Duflo, 2004; Beaman et al., 2009; Jaspal, 2011; Sankaran, Sekerdej, and Von Hecker, 2017). A large literature has studied the effects of having female leaders in many different settings and documents that they are more likely to invest more in water and health (Chattopadhyay and Duflo, 2004), they raise aspiration and education among women (Beaman et al., 2012; Clots-Figueras, 2012), and they increase participation in the labor market (Bose and Das, 2018; Deininger, Nagarajan, and Singh, 2020). Hence it's important to disentangle if the effects are through the demand channel (role model effects) or the supply channel (preferential allocation).

the labor supply of workers in public works programs in India. By exploring this previously unaddressed aspect, the study contributes to a more comprehensive understanding of the intricate dynamics between political connections, identity, and labor outcomes in the context of public works programs.

The rest of the paper is organized as follows. In section 3.2, we present a survey on the history of caste, the role of caste identity in the Indian economy, and briefly document the background of the public program and its effects. We describe my data and descriptive statistics in 3.3. Empirical strategy, the estimation and identification challenges are discussed in section 3.4. Section 3.5 presents my main results and discusses heterogeneity in the results. In section 3.5.1, we test whether the strength of the connections matters. We check for robustness of the results and lay out the mechanism that is likely to be driving the results in section 3.6. In section 3.7, we conclude and discuss the policy implications.

3.2 Background

3.2.1 The Caste System

The roots of caste in India can be traced back to 1500-500 BCE. Historically, caste was a system of social stratification that was hereditary and based on a person's occupation. During this time, society was divided into four main varnas or social classes: the Brahmins, the Kshatriyas, the Vaishyas, and the Shudras. Dalits were not included in the original varnas (Munshi, 2019; Macdonell, 1914). Within these varnas and dalits, there exist hundreds of subgroups known as castes or jatis[5] [6]. Each varna had its distinct roles and responsibilities, and social mobility between them was limited. Over time, this rigid social structure became more complex and the caste (jati) system became deeply ingrained in Indian society. Jati, meaning "birth group" or "sub-caste," became an essential aspect of the social organization

[5]The main castes were further divided into about 3,000 castes and 25,000 sub-castes, each based on their specific occupation

[6]Macdonell (1914) emphasizes that without a historical examination of the caste system in contemporary India, one cannot gain a comprehensive understanding of the institution itself in India. For a more detailed account of the caste system's history, please see Macdonell (1914); Dumont (1980)

[7], with strict rules governing social interaction, marriage, and even occupation (Macdonell, 1914).

This hierarchical nature of the caste system, where individuals were assigned a specific caste at birth, determined their social standing and opportunities in life. Those born into higher castes enjoyed privileges and access to resources, while those in lower castes faced discrimination, social exclusion, and limited opportunities for upward mobility. The caste system influenced the division of labor, with each caste being associated with specific occupations and skills. The division of labor facilitated economic specialization and contributed to various sectors of the economy. However, it also led to the perpetuation of occupational stereotypes and restricted social mobility (Deshpande, 2011, 2010; Dumont, 1980). The lack of social mobility resulted in economic disparities and inequalities, with lower-caste individuals often experiencing poverty and limited access to resources and economic opportunities. With an amendment to the constitution, the Government of India (GoI) has made an effort to address the social and economic inequities perpetuated by the caste system. Affirmative action policies, such as reservations in education and government jobs, have been implemented to provide opportunities for historically disadvantaged castes. However, the caste system continues to influence social and economic dynamics in India (Deshpande and Newman, 2007; Munshi, 2019). And, political mobilization along caste lines, especially during elections, remains a common phenomenon (Blakeslee et al., 2013).

3.2.2 The Role of Caste Identity in the Indian Economy

Scholars in the fields of sociology and psychology have extensively explored the influence of identity on human behavior (Sharma, 1984; Vaid, 2014). However, the formalization of identity in economics began with the seminal work of Akerlof and Kranton (2000). More recently, economists have shown increasing interest in measuring the impact of identity on

[7]The main castes were further divided into about 3,000 castes and 25,000 sub-castes, each based on their specific occupation (BBC 2019)

various economic and non-economic outcomes. Munshi (2019) emphasizes the significance of caste in shaping everyday economic and non-economic decision-making among Indians. Caste identity plays a crucial role in determining an individual's social and economic status, as well as influencing the overall social structure and norms within a group or society.

India has made significant economic and social progress by integrating lower-caste groups through affirmative action. However, barriers continue to limit occupational mobility, as caste-based occupations abound (Das, 2013; Deshpande, 2011; Oh, 2019). A large proportion of low-caste households remain employed in menial services (Deshpande, 2018; Hoff, Kshetramade, and Fehr, 2011; Iversen et al., 2016). This low mobility could be due to the historical oppression of, and social stigma associated with the social identity of, a particular caste (Hoff, Kshetramade, and Fehr, 2011).

For example, Hoff and Pandey (2006) conducted an experiment in Uttar Pradesh, India, and found that public disclosure about the caste impedes the ability to solve the puzzles if the caste has been historically discriminated against, pointing towards the persistent effect of discrimination. Similar results are found in an experimental study in the Indian state of Orissa by Oh (2019). Specifically, Oh (2019) finds that caste identity is a significant determinant of individuals' decision whether to accept a job offer, as it reduces participation in a job-offer experiment by 23 percentage points when the job offer is associated with other caste. In a related study, Iyer, Khanna, and Varshney (2013) found that caste is related to entrepreneurship in India and that lower-caste groups are severely underrepresented in entrepreneurship in India despite progressive policies.

Caste also plays an important role in the Indian marriage market. More than 80 percent of marriages in India are arranged, and 90 percent of marriages are within the same caste. In addition, caste reservations granted to individuals may disproportionately favor men from lower-caste groups over their female counterparts. For example, due to affirmative action, government agencies differ in their hiring of men and women belonging to the OBC caste group. Because men are more likely to be hired, they have greater household bargaining power in decisions regarding the number of children or the spacing between children (Desai, 2016).

Barriers between caste groups create inefficiencies in Indian villages and affect the overall productivity and surplus of villages. For example, Anderson (2011) finds that lower-caste farmers with land in villages dominated by higher castes have lower productivity than farmers in villages dominated by lower castes. The productivity difference could reflect the fact that trade in irrigation water is limited across castes in villages, making caste an important cause of market inefficiency.

3.2.3 Public Works Program

The National Rural Employment Guarantee Scheme (NREGS) is a flagship workfare program in India. It was enacted by the Indian government in 2005 and implemented in 2006[8]. The program was rolled out in a phase-wise manner, initially covering India's 200 poorest districts in February 2006, in phase 1. Gradually it expanded to the next poorest 130 districts in April 2007 in phase 2. And finally, in April 2008, the remaining districts were added to the program.[9]

To work under the program, one must apply for a job card, which serves as the basis for work. All job-card holders are eligible to work under the program. Eligible households can apply for work and receive employment within 15 days of submitting an application. If work is not provided within 15 days' time, the applicant is entitled to an unemployment allowance. The wages paid under the scheme are determined by the state governments but

[8] India's national workfare program, NREGS, is the world's largest cash-for-work program. India spent about $9.98 billion on NREGS in 2020, double the $4.8 billion spent in 2015. And the program employed 87 million people in 2020 alone (Economic Times, October 22, 2020).

[9] In the 2001 census, there were 593 districts. In the 2011 census, that number increased to 640. The Indian Planning Commission selected districts for the program based on poverty rates calculated using data from 1993–94 National Sample Survey and the 2001 census. The Planning Commission of India created a development index based on the poverty headcount ratio, agricultural wages, and productivity and population share of lower-caste groups such as SC and ST.

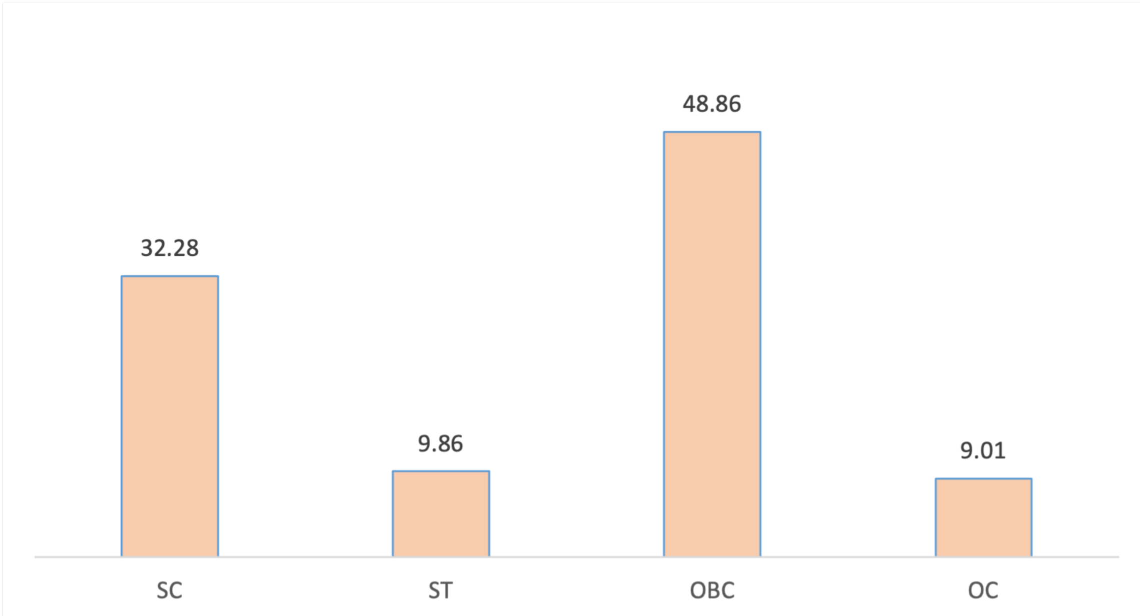

Figure 3.1 Distribution of Workers in NREGS

must not be less than the minimum wage rates prescribed by the government. The wage under NREGS is equal for men and women. The program also stipulates that one-third of all jobs be reserved for women to increase their participation. Most of the work performed under the program is low-skilled. However, people of all skill levels and caste groups can apply for jobs.

The scheme focuses on labor-intensive works that contribute to rural development and builds productive assets, such as roads and water-harvesting structures (Deininger and Liu, 2013). Over the years, the NREGS has witnessed several improvements and modifications to enhance its effectiveness. These include the use of technology for better implementation and increased transparency through social audits. Local NGOs are expected to audit the program, report it to district panchayats on the audit, and make the report public. Wages are supposed to be paid directly to the applicant's bank account, and if the applicant does not have an account, they are encouraged to open one under NREGS to minimize corruption (Muralidharan, Niehaus, and Sukhtankar, 2016).

A significant body of work has studied the effects of NREGS. The NREGS has had a significant impact on rural India. It has provided a safety net for the poor (Bose, 2017; Rao, 2019), reduced rural-urban distressed migration(Imbert and Papp, 2020), and increased the income and purchasing power of rural households (Cook and Shah, 2022). The scheme has also contributed to empowering women and marginalized communities by ensuring their participation in the workforce and decision-making processes (Azam, 2011; Desai, 2018). NREGS increased private and public wages for unskilled workers in the dry season (Berg et al., 2018; Imbert and Papp, 2015), helps diversify crop income (Gehrke, 2019), and improves health and education outcomes (Dasgupta, 2017; Shah and Steinberg, 2019).

Studies examining the effects of gender identity on village panchayat heads have yielded mixed findings. While having a female panchayat head enhances the demand for work under NREGS (Bose and Das, 2018; Deininger, Nagarajan, and Singh, 2020), there is evidence suggesting inefficiencies and leakages in the program within village councils reserved for women (Afridi, Iversen, and Sharan, 2017). Notably, the efficiency of public services improves as female leaders accumulate more experience (Afridi, Iversen, and Sharan, 2017). Nonetheless, the available evidence remains quite limited regarding the impact of village leaders' caste identity and its implications for NREGS outcomes.

3.3 Data

The main data comes from the Socio-Economic Profile of India (SEPRI), which is a follow-up to the Rural Economics and Demographic Survey (REDS)[10]. The SEPRI was enumerated for all households in 192 villages across 13 states from 2014 to 2016. The survey had two components: village and household. It utilizes the complete set of questions from the REDS village module, along with additional information on NREGS-related activities.

[10]REDS builds on the Additional Rural Income Survey, a panel survey first conducted in 1969 and 1971 that provides comprehensive information about more than 4,200 households in 259 villages in 17 major Indian states; the households are meant to be representative of India's rural population. In 1981–82, surviving households, any descendants from the original sample who resided in the same village, and a random sample of new households were surveyed (Vashishtha, 1989). A similar procedure was employed in subsequent survey rounds, resulting in samples of 7,474 and 8,659 households in 1998–99 and 2007–8 across 242 villages in 17 major states.

The village-level module collects information on every political candidate who participated in the past two panchayat elections for the position of village council head. Additionally, the village-level module also gathers detailed information on political candidates running for the office of village council members. In contrast to the village module, the SEPRI survey utilizes only a subset of questions from the REDS but provides detailed information on NREGS activities for each member[11].

The SEPRI data are particularly suited for my analysis since the survey collects detailed household- and individual-level information as well as information on the characteristics of each political candidate who ran for the office of village council head and memberships in the last two elections. In particular, the information on political candidates includes their name, education, gender; whether a candidate is from the SC, ST, OBC, or Forward caste group; vote received, and whether the candidate ran for the village-council position in past elections, etc. The household component of the survey includes a standard consumption and income module, employment, religion, social group, and jati. Additionally, the household module provides information on detailed NREGS activities for each worker. The information on NREGS includes seasonal allocation and demand for jobs, possession of job cards by households, household preferences for more work, total wages earned through NREGS, and details on transparency and accountability.

As all households in the REDS villages were enumerated, we were able to match the surnames of the village council heads to households and individuals, allowing us to construct a measure of political connection. Figure 3.2 show the distribution of the population of age 18 and above with the same subcaste (jati) as the village council head by caste group in the data. We restrict the sample to households with job cards since job cards determine

[11]Because of conflicts, no data were collected in Jammu and Kashmir in 1998/99 and 2007/08 survey rounds (11 villages). For the same reason, no data were collected in Assam in 1981/82 and 2007/08 rounds of surveys (8 villages). The 242 villages for which data were collected in all three rounds are distributed across 15 states. These states became 17 states after the formation of the new states of Chhattisgarh and Jharkhand in 2000, which split from Madhya Pradesh and Bihar.

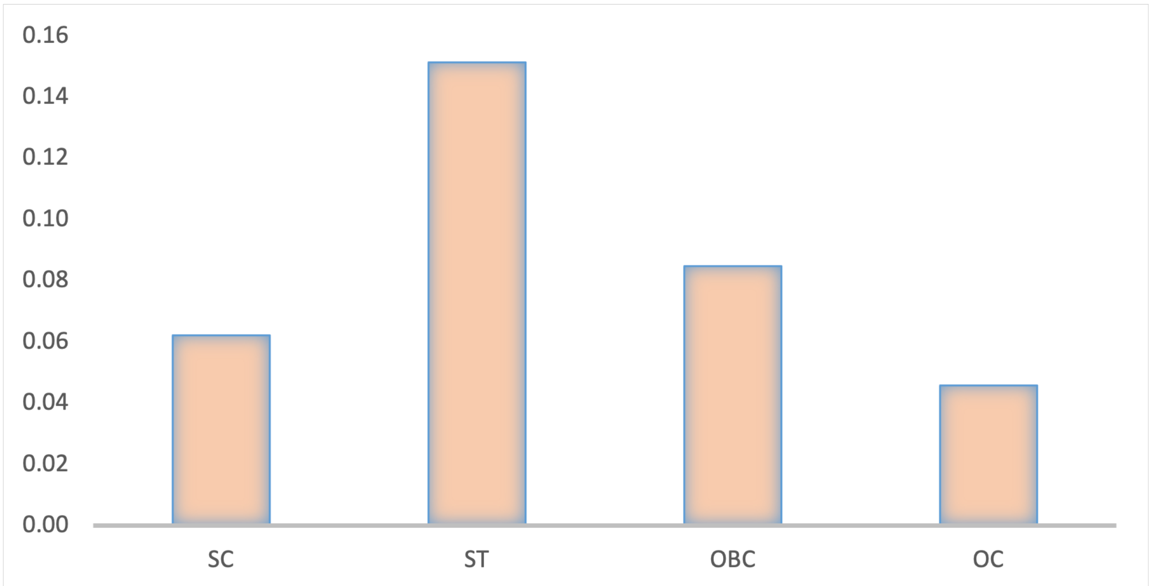

Figure 3.2 Share of Same Caste Population as Village Head

eligibility to work in NREGS.

3.3.1 Summary Statistics

Table 3.1 presents the descriptive statistics of the household-level variables in panel A and the individual-level statistics in panel B. The data suggest that 27 percent of households have a job card. Of those who have job cards, only 24 percent worked in NREGS; that subset worked an average of 38 days for NREGS in a year.

The data indicates that about 29 and 49 percent of households belong to the SC/ST and OBC caste groups, respectively, while 22 percent of the households in the dataset are from the Other (Forward) caste group, which is consistent with the findings of the 2011 census. Among all households, around 56 percent own some agricultural land (Table C.2). There is no significant statistical difference in land ownership between individuals possessing a job card and those without one. However, landownership among the SC/ST caste group is 10 percentage points lower compared to the Forward caste group. On average, each household in the dataset possesses about 2.6 acres of land. There is a significant difference in landholding sizes between the SC/ST and Forward caste groups, with the former having

Table 3.1 Summary Statistics

Variable	All	HHs with Job card SC-ST	OBC	OTHER	Total
		Panel A			
Female head	0.128	0.124	0.110	0.106	0.115
Head's age	50.028	48.653	49.490	49.392	49.130
Head's education	4.779	3.267	4.009	4.587	3.776
Share of hhs					
. . . with no education	0.399	0.504	0.414	0.358	0.444
. . . up to 5th grade	0.172	0.205	0.239	0.220	0.223
. . . up to high school	0.379	0.260	0.321	0.392	0.305
. . . college or above	0.044	0.015	0.018	0.023	0.017
Married	0.823	0.838	0.854	0.858	0.848
Household Size	4.561	4.481	4.529	4.142	4.459
Prime male	1.671	1.639	1.664	1.603	1.646
Prime female	1.565	1.545	1.587	1.471	1.554
Children	1.103	1.121	1.085	0.870	1.072
Own Any land	0.562	0.505	0.655	0.517	0.575
. . . Size of land	2.625	1.839	2.353	1.853	2.116
Jobcard	0.271				1.000
#Obs	79646	8959	9806	2817	21582
		Panel B			
Age	40.081	39.527	40.129	40.184	39.886
Female	0.491	0.489	0.494	0.479	0.490
Education	5.58	4.223	4.894	5.512	4.696
Formally Applied	0.115	0.346	0.325	0.322	0.333
Worked in NREGS	0.064	0.247	0.263	0.168	0.244
# of Days worked	37.059	34.412	42.694	28.146	37.894
Received allowance	0.008	0.022	0.020	0.017	0.020
Wanted to work more	0.032	0.139	0.111	0.094	0.120
Attended GS meetings	0.181	0.190	0.197	0.217	0.197
#Obs	249923	25991	28327	8208	62526

Notes: Author's own calculation from survey data

an average of two acres and the latter having an average of three acres per household. Importantly, over 50 percent of households have either no formal education or have received education only up to grade 5. This is true for both household heads and members of the households.[12]

The percentage of job-card holders is calculated based on the total number of households in the sample. There is considerable variation in job-card ownership among different caste groups. For instance, 33 percent of job card holders belong to the SC caste group, 9 percent are from the ST group, 45 percent are OBC, and 13 percent are from other caste groups. Our primary analysis focuses exclusively on job card holders since possessing a job card is a prerequisite for employment. Detailed descriptive statistics can be found in Table C.1.

Panel B of Table 3.1 presents summary statistics pertaining to individuals and their participation in NREGS activities. We find that approximately 11 percent of household members applied for a job under the NREGS program. While, among households with job cards, approximately 33 percent of individuals applied for jobs. The average age of individuals willing to work under NREGS is 40 years. Around 2 percent of household members applied for a job but were unable to secure one within 15 days, resulting in them receiving an allowance. The average number of days worked across all caste groups, and job-card holders ranges from 28 to 43 (Table 3.1).

Approximately 24 percent of individuals holding job cards were engaged in NREGS employment. Although this number is quantitatively similar for SC/ST and OBC caste groups, it is 17 percent for the forward caste group. Additionally, 12 percent of job card holders expressed a desire for more work, indicating the possibility of some form of rationing at an extensive margin. This disparity becomes even more pronounced when considering the entire sample of households. There are no significant variations in attendance rates at village-council meetings among different caste groups or job card holders. Detailed

[12] In the sample I analyzed, 59 percent of individuals have either no formal education or have received education only up to grade 5.

descriptive statistics for households with job cards can be found in Table C.2. For the descriptive statistics of all households, see Table C.2.

3.4 Empirical Strategy

We are interested in estimating the effects of political connections on the employment status of the beneficiaries. However, identifying the effects of political connections is not straightforward since leaders are not randomly elected. We capitalize on India's historical caste system and construct a measure of connections using the last names of leaders and beneficiaries, which reflect their caste (jati). There are hundreds of castes within caste groups. We compare beneficiaries with the same caste (jati) as the village council chairperson (*pradhan*) to those that do not have the same jati as pradhan. We estimate the equation of the following form.

$$\mathrm{Y}_{ijcv} = \beta Same_{ijcv} + \theta' X_{ijcv} + \alpha_v + \eta_c \times \delta_{v,d} + \epsilon_{ijcv} \tag{3.1}$$

Where, Y is an outcome variable, i.e., application for the job, whether work and the number of days worked in NREGS for individual (i) of jati or caste (j), caste group (c) in a village (v). $Same$ is a measure of political connection that takes on the value one if the village council chairperson and beneficiaries are of the same jati and zero otherwise.

X is a vector of household- and individual-level characteristics, including gender; age; religion, and marital status of the head of the household and individuals. We also include caste-village-level controls that aggregate household and individual time-invariant variables by caste group and village to control for differences in caste-group composition within a village. α is the village-level fixed effect that captures village-level differences, $\eta_c \times \delta_{v,d}$ denotes the caste-by-district fixed effect, and/or the caste-by-village fixed effect captures the differences in caste composition across districts and villages. ϵ is the random error term.

Identification To identify β, we take advantage of the caste system in India. Caste (jati) is assigned at birth, and there are numerous castes in a caste group. The role roll-out of the NREGS program in a district and village is unlikely to affect the caste composition of villages in the short run. However, the caste (*jati*) composition of the village can affect the election results. Therefore, we also conduct a robustness analysis by dropping the villages with higher shares of a caste since leaders might face little competition in those villages and may have little incentive to reward voters at the margin.

3.4.1 Instrumental Variable Framework

We can estimate the equation 3.1 using OLS and incorporate village and caste group dummies to account for unobservable variables at the village level. However, the estimated β may still be potentially biased if certain unobservable factors at the village-caste or subcaste level, such as hierarchy and norms, are correlated with political connections and influence the election of leadership. To address this potential endogeneity problem, we leverage a distinctive institutional feature of political reservation in India, introduced through the 73rd and 74th constitutional amendments.

The 73rd and 74th amendments require that the seats of the local village council heads be reserved for lower caste groups, such as the Scheduled Caste and Scheduled Tribe[13]. The proportion of seats reserved for the Scheduled Caste and Scheduled Tribe is determined based on their population share. At each panchayat level, reservation is then on a rotation basis. We leverage this procedure and employ an instrument that takes a value of 1 if the village panchayat is reserved for the same caste group, and 0 otherwise. Using this instrument, we estimate the first-stage equation in the following form.

$$Same_{ijcv} = \gamma \mathrm{RS}_{c,v} + \theta' X_{ijcv} + \alpha_v + \eta_c \times \delta_{v,d} + \mu_{ijcv} \tag{3.2}$$

Where *Same* is a dummy variable that takes the value 1 if the beneficiaries and village council chairperson share the same caste (*jati*) as in equation (3.1). *RS* is an indicator

[13]The amendments also mandated that 1/3 of the total gram panchayat head seats must be reserved for women, and in some states, this reservation was later extended to 50%.

variable that takes on the value 1 if the village council seats were reserved for the same caste group as the beneficiaries, and 0 otherwise[14]. X is a vector of controls that includes household- and individual-level characteristics, as well as caste-by-village-level controls as specified in equation (3.1). α represents the village fixed effect, η captures the caste-by-district and/or caste-by-sub-district fixed effects, and μ represents the error term.

The validity of the instrument depends on whether it is correlated with the political connection, i.e., $\gamma \neq 0$, and is uncorrelated with the error term of the structural equation 3.1. The first-stage estimates reject the null of $\gamma = 0$ satisfying the relevance condition for the instrument. And for the exclusion restriction, We argue that the instrumental variable satisfies exclusion restriction since the 73rd and 74th suggest that village councils (panchayats) must be reserved for lower caste groups on a rotational basis and the reserved seats must be proportional to the state population share of the lower caste group

3.5 Results

In this section, we present the main findings on the relationship between political connections and employment in NREGS. Table 3.2 presents the results of our main analysis. Our main sample of analysis is restricted to job-card holders as it is a prerequisite for working in NREGS. We also present the analysis of political connections and job card holding rate in table C.6. Columns 1 and 3 of Table 3.2 present the coefficient of political connections using the OLS, and columns 2 and 4 present estimates using 2SLS. Furthermore, we provide different versions of the specifications with alternative fixed effects in Table C.4 using the OLS method and in Table C.5 using the 2SLS method.

In Panel A of Table 3.2, we present our findings on the impact of having a village council chairperson (*pradhan*) from the same caste (jati) on the application for work in the National Rural Employment Guarantee Scheme (NREGS). Our OLS estimate suggests that having a

[14] See Gille (2018) which utilized a similar instrument. For a more detailed review of the caste system and its impact on the economy, refer to Munshi (2019).

village council chairperson from the same jati increases the probability of applying for work in NREGS by 4.2 percentage points (column 2), indicating a 13 percent increase relative to the mean application rate. Estimated effects are slightly smaller in size if we don't include the district by caste fixed effects that allow us to control for cross-district differences in cast composition that might be correlated with the caste of candidates running for election in the village council.

In Panels B and C of Table 3.2, we present the estimated coefficients pertaining to the likelihood of employment in NREGS (extensive margin) and the number of days worked in the NREGS (intensive margin) when the village council chairperson belongs to the same caste (jati). In Columns 1 and 3, we provide the OLS estimates. Overall, we find a statistically significant relationship between the political connection measured as having the same caste village council chairperson as beneficiaries and the likelihood of employment in the NREGS. This is consistent with findings on the effects of political connection in other contexts (Choi, Penciakova, and Saffie, 2021). The results are robust to the inclusion and exclusion of controls, fixed effects, and different employment measures.

The Individuals who share the same caste as the chairperson experience a noticeable increase of 3 percentage points in the probability of working in NREGS, which corresponds to a 12 percent increase from the mean. This finding highlights the substantial influence of caste dynamics on employment opportunities within the program. Furthermore, the effect of having the same jati chairperson on the number of days worked in NREGS is 11 percentage points, representing an 11 percent increase from the mean of NREGS days (Panel C). The effect sizes across all three measures, including the likelihood of application, working, and the number of days worked, are strikingly similar. This increase in employment in NREGS when the village council belongs to the same jati highlights the influential role of caste dynamics in shaping employment patterns within the NREGS program in India.

2SLS Estimates As discussed in section 3.4, unobservables at the caste level, such as hier-

Table 3.2 Political Connections and Its Effects on Employment

	(1)	(2)	(3)	(4)
	Panel A: Applied for Work			
Same Caste Chairperson	0.038**	0.083**	0.042***	0.080**
	(0.019)	(0.034)	(0.016)	(0.035)
Observations	62,526	62,526	62,526	62,526
R-squared	0.217	0.067	0.209	0.067
Dep. Var Mean	0.333	0.333	0.333	0.333
Dep. Var Std	0.471	0.471	0.471	0.471
Sanderson-Windmeijer F stat		16.57		18.74
	Panel B: Worked			
Same Caste Chairperson	0.023**	0.048*	0.030***	0.054**
	(0.011)	(0.027)	(0.010)	(0.027)
Observations	62,526	62,526	62,526	62,526
R-squared	0.260	0.081	0.256	0.080
Dep. Var Mean	0.244	0.244	0.244	0.244
Dep. Var Std	0.430	0.430	0.430	0.430
Sanderson-Windmeijer F stat		16.57		18.74
	Panel C: Days			
Same Caste Chairperson	0.083**	0.205**	0.110***	0.201**
	(0.042)	(0.082)	(0.039)	(0.083)
Observations	62,526	62,526	62,526	62,526
R-squared	0.263	0.081	0.259	0.080
Dep. Var Mean	0.982	0.982	0.982	0.982
Dep. Var Std	1.776	1.776	1.776	1.776
Sanderson-Windmeijer F stat		16.57		18.74
Village FEs	Y	Y	Y	Y
District by caste FEs	N	N	Y	Y
Controls	Y	Y	Y	Y
Estimation methods	OLS	2SLS	OLS	2SLS

Notes: *Same* is an indicator variable that takes on the value of 1 if a local and the village-council chief share the same surname and belong to the same caste (jati). Each column reports results from a separate regression. Dependent variables Applied" and Worked" are dummy variables that take the value of 1 if a household member applied for a job and worked in NREGS, respectively, and 0 otherwise. "Days" is the number of days that an individual worked in NREGS. Controls include household-level variables such as head of the household's age, sex, marital status, religion, individual-level variables such as gender, age, education, and caste group by village-level variables such as average age, education, sex, etc. Standard errors are clustered at village council and caste-group levels. Robust standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

archy and influence in a village, might be correlated with a measure of political connections. This might result in bias in our OLS estimates. To correct this endogeneity bias, we use an instrumental variable (IV) framework where the instrument is a dummy if the village council is reserved for the same caste group. We argue that the instrumental variable satisfies exclusion restriction since the 73rd and 74th suggest that village councils (panchayats) must be reserved for lower caste groups on a rotational basis and is proportional to the state population share of the lower caste group. The first-stage coefficients are reported in Panel D of Table C.5. The first stage coefficients are statistically significant across all specifications, with F stat ranging from 16 to 19, indicating the relevance of the instrument.

We report the results of the 2sls estimates from various specifications in Table C.5. However, our preferred specification is shown in columns 2 and 4 of Table 3.2, which correspond to columns 2 and 4 of Table C.5. In Panel A, column 4, we find that the likelihood of applying to work in NREGS increased by 8 percentage points when the village council chairperson belongs to the same caste. This is a 24 percent increase from the mean. Similarly, we find that the likelihood of working in NREGS increased by 5.4 percentage points, and NREGS days increased by 20 percentage points, which are equivalent to 22 percent and 20 percent increases from the mean, respectively (Table 3.2, Panel B and C). 2SLS estimates are almost twice as large as OLS estimates across all measures, including application to work in NREGS, if got a work, and number of days worked. These differences in coefficient can partially be justified by the fact that NREGS is a self-targeting poverty-alleviating workfare program that requires manual labor, and the low caste group population is more likely to work coupled with the nature of instrumental variable that requires village councils to be reserved for the low caste group.

3.5.1 Role of the Village Council Members

In this subsection, we test whether the strength of political connections affects the likelihood of obtaining public employment. As defined earlier, a direct connection, i.e.,

Table 3.3 Village Council Members of *Same* Caste (Jati) and Employment

	(1)	(2)	(3)
		Panel A: Applied for Work	
Same Caste VC Member	0.001	-0.001	-0.006
	(0.014)	(0.013)	(0.012)
Observations	62,524	62,526	62,526
R-squared	0.171	0.217	0.209
Dep. Var Mean	0.333	0.333	0.333
Dep. Var Std	0.471	0.471	0.471
		Panel B: Worked	
Same Caste VC Member	0.016	0.015	0.016*
	(0.011)	(0.010)	(0.009)
Observations	62,526	62,526	62,526
R-squared	0.207	0.260	0.256
Dep. Var Mean	0.244	0.244	0.244
Dep. Var Std	0.430	0.430	0.430
		Panel C: Days	
Same Caste VC Member	0.056	0.052	0.056
	(0.043)	(0.041)	(0.038)
Observations	62,526	62,526	62,526
R-squared	0.210	0.263	0.259
Dep. Var Mean	0.982	0.982	0.982
Dep. Var Std	1.776	1.776	1.776
Village FE	Y	Y	Y
District-by-caste FE	N	Y	Y
Controls	N	Y	Y

Notes: *Same* is an indicator variable that takes on the value of 1 if a local and the village-council chief share the same surname and belong to the same caste (jati). Each column reports results from a separate regression. Dependent variables Applied" and Worked" are dummy variables that take the value of 1 if a household member applied for a job and worked in NREGS, respectively, and 0 otherwise. "Days" is the number of days that an individual worked in NREGS. Controls include household-level variables such as head of the household's age, sex, marital status, religion, individual-level variables such as gender, age, education, and caste group by village-level variables such as average age, education, sex, etc. Standard errors are clustered at village council and caste-group levels. Robust standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

shared identity, with a village council leader is considered a strong connection, since the village council chairperson has discretionary power over the allocation of resources, including NREGS jobs. However, beneficiaries may also share the same caste as council members, which we consider to be weaker connections as village council members act as advisors but have little authority[15]. Based on this distinction, we construct a measure of political connection using the jati or the caste of the village council members and whether they are the same as the beneficiaries.

Using this measure, we estimate a variant of equation 3.1 that includes a measure of political connection based on the same caste as village council members and report the results in Table 3.3. We find no effect of having a village council member from the same caste on the application for NREGS work. This holds across all specifications. However, we find a small and marginally significant increase in the probability of working in NREGS if a village council member belongs to the same caste. The increase is 1.6 percentage points, which corresponds to an increase of 6.5 percent from the mean, much smaller than the effect size estimated for the chairperson (Panel B of Table 3.3). The results for the number of days worked in NREGS are reported in panel C of table 3.3. Although coefficients are positive for the number of days, they are not precisely estimated.

Furthermore, since we find positive effects of connections with village-council members, a natural question is whether these members act as substitutes for or complements to the chairperson. To test this, we interact with the dummy for having a village-council chairperson from the same caste as a dummy for having a member of the council from the same caste. If the members complement the village-council head, then we should expect the interaction term to be positive and statistically significant. We report the results in the Panel

[15] A village council in India typically consists of 5–15 members, depending on the size of the village. Village council members may also play a role in allocating job cards, and working days since, in making decisions about public programs, the village-council chairperson must take the council members into consideration. For example, in NREGS, the types of projects and the number of jobs are determined by the village-council chairperson in consultation with village council members and other stakeholders such as self-help groups.

B of Table C.8: for all outcome variables, although coefficients are positive, the interaction term is not statistically different from zero.

Table 3.4 Village Council Head of *Same* Caste Group and Employment

	(1)	(2)	(3)
		Panel A: Applied for Work	
Same Caste Group Chairperson	0.007	0.007	-0.001
	(0.015)	(0.011)	(0.011)
Observations	62,526	62,526	62,526
R-squared	0.151	0.200	0.209
Dep. Var Mean	0.333	0.333	0.333
Dep. Var Std	0.471	0.471	0.471
		Panel B: Worked	
Same Caste Group Chairperson	0.017	0.017**	0.009
	(0.011)	(0.009)	(0.009)
Observations	62,526	62,526	62,526
R-squared	0.195	0.251	0.256
Dep. Var Mean	0.244	0.244	0.244
Dep. Var Std	0.430	0.430	0.430
		Panel C: Days	
Same Caste Group Chairperson	0.057	0.054	0.025
	(0.044)	(0.034)	(0.034)
Observations	62,526	62,526	62,526
R-squared	0.199	0.254	0.259
Dep. Var Mean	0.982	0.982	0.982
Dep. Var Std	1.776	1.776	1.776
Village FE	Y	Y	Y
District-by-caste FE	N	Y	Y
Controls	N	N	Y

Notes: *Same* is an indicator variable and takes on the value of 1 if both beneficiary and village council head (*chairperson*) are from the same caste group such as SC, ST, OBC, and Others. Each column reports results from a separate regression. Dependent variables Applied" and Worked" are dummy variables that take the value of 1 if a household member applied for a job and worked in NREGS, respectively, and 0 otherwise. "Days" is the number of days that an individual worked in NREGS. Controls include household-level variables such as head of the household's age, sex, marital status, religion, individual-level variables such as gender, age, education, and caste group by village-level variables such as average age, education, sex, etc. Standard errors are clustered at village council and caste-group levels. Robust standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

3.5.2 Same Caste Group

Another way to test the strength of political connections in our setting is to construct a measure of connections based on caste group instead of caste (jati). The caste group is a much larger group, and its members are likely to belong to different castes. Caste groups are defined as SC, ST, OBC, and Others.

We construct the measure of political connection based on these groups and construct a dummy if the village council chairperson is of the same caste group as the beneficiaries. We then run a similar regression as in equation 3.1.

Results are reported in Table 3.4. In Panel A of 3.4, we report the estimated effect on the probability of applying for NREGS work and find no effects of having a village council head from the same caste group. In Panel B, when we do not include caste by village level controls, there is a small and marginally significant effect of having a chairperson from the same caste group, but it becomes zero once we include caste by village level controls. We also find no effects on the number of days worked in NREGS. Although coefficients for the number of days are positive, none of the coefficients reported in Table 3.4, Panel C are significantly different from zero, suggesting that political connections based on caste groups are weaker than those based on caste or jati

3.6 Robustness Checks and Mechanism

In this section, we present various robustness checks. We present the main analysis with different sample restrictions and also a falsification test with placebo treatment, i.e., a measure of connections constructed based on similar-sounding first names instead of last names and jati.

3.6.1 Sample Restrictions

In our first robustness check, we dropped the villages that have the largest share of sub-caste. One of the primary concerns we had was that if a subcaste dominated a village, the chairperson of the village council might not face competition, especially if the caste shapes the political dynamics in the elections, and might be less inclined to distribute benefits to individuals from the same caste.

Table 3.5 Effects of Political Connections by Sample Restrictions

VARIABLES	(1) Applied	(2) Worked	(3) Days	(4) Applied	(5) Worked	(6) Days
	Panel A: OLS Estimates					
Same Caste Chairperson	0.038*	0.028**	0.085**	0.040**	0.026***	0.078**
	(0.020)	(0.011)	(0.034)	(0.017)	(0.010)	(0.031)
Observations	59,937	59,937	59,937	59,937	59,937	59,937
R-squared	0.233	0.273	0.277	0.223	0.268	0.273
	Panel B: 2SLS Estimates					
Same Caste Chairperson	0.070*	0.052**	0.194**	0.082**	0.066**	0.192**
	(0.037)	(0.026)	(0.080)	(0.038)	(0.030)	(0.082)
Observations	59,937	59,937	59,937	59,937	59,937	59,937
R-squared	0.075	0.087	0.087	0.074	0.086	0.087
Dep. Var Mean	0.336	0.248	0.823	0.336	0.248	0.823
Dep. Var Std	0.472	0.432	1.493	0.472	0.432	1.493
Sanderson-Windmeijer F stat	15.86	15.86	15.86	16.68	16.68	16.68
Village FE	Y	Y	Y	Y	Y	Y
Caste by State FE	N	N	N	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

Notes: *Same* is an indicator variable that takes on the value of 1 if a local and the village-council chief share the same surname and belong to the same caste (jati). Each column reports results from a separate regression. Dependent variables Applied" and Worked" are dummy variables that take the value of 1 if a household member applied for a job and worked in NREGS, respectively, and 0 otherwise. "Days" is the number of days that an individual worked in NREGS. Controls include household-level variables such as head of the household's age, sex, marital status, religion, individual-level variables such as gender, age, education, and caste group by village-level variables such as average age, education, sex, etc. Standard errors are clustered at village council and caste-group levels. Robust standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

However, in situations where a village comprises multiple castes (*jati*), candidates could potentially face competition during elections and are more likely to reward voters of the same caste in order to gain their votes (Munshi and Rosenzweig, 2013). To ensure that our results are not driven by the largest caste share in the village, we dropped those villages. This leaves us with a sample of villages with multiple castes within a caste group. We report the results in Table 3.5. In panel A of Table 3.5, we present OLS estimates and 2SLS estimates in panel B. We find the results presented in the main analysis are not driven by villages with the largest share of castes.

3.6.2 Falsification Test

Since our measure of political connection is based on shared jati or surname, we conduct a falsification test with a placebo treatment to ensure that our results are indeed driven by shared jati. To conduct a falsification test, we create a placebo measure of political connection. The placebo connection is constructed using probabilities matching of similar sounding first names only. We also do not include any restrictions, such as matching within villages or states. First names do not typically reveal an individual's caste or occupation in India or elsewhere, so this test serves as a falsification test against our main results.

Table 3.6 Placebo Regression of Political Connection's Effects

VARIABLES	(1) Applied	(2) Worked	(3) Days	(5) Applied	(6) Worked	(7) Days
Same (Placebo)	-0.006	0.003	0.011	-0.005	0.000	-0.000
	(0.004)	(0.004)	(0.014)	(0.004)	(0.004)	(0.013)
Observations	62,524	62,524	62,524	62,526	62,526	62,526
R-squared	0.217	0.207	0.209	0.209	0.256	0.258
Dep. Var Mean	0.333	0.244	0.812	0.333	0.244	0.812
Dep. Var Std	0.471	0.430	1.487	0.471	0.430	1.487
Village FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Caste by State FE	N	N	N	Y	Y	Y

Notes: $Same(Placebo)$ is an indicator variable that takes on the value of 1 if a local and the village-council chief have similar-sounding first names. Each column reports results from a separate regression. Dependent variables Applied" and Worked" are dummy variables that take the value of 1 if a household member applied for a job and worked in NREGS, respectively, and 0 otherwise. "Days" is the number of days that an individual worked in NREGS. Controls include household-level variables such as head of the household's age, sex, marital status, religion, individual-level variables such as gender, age, education, and caste group by village-level variables such as average age, education, sex, etc. Standard errors are clustered at village council and caste-group levels. Robust standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

We run a similar regression as in 3.1 but replace our original measure of political connection with a placebo measure of political connection. The results are reported in

Table 3.6. As expected, we do not reject the null and indeed find evidence in favor of our main results. This suggests that caste identity and political connections play an important role in public works programs and that political leaders may be systematically allocating jobs to their own groups.

3.6.3 Mechanism

The main results show a positive association between political connections and job allocations in NREGS. This could be because local leaders aim to win votes by favoring individuals of the same caste, a supply-side channel, or because individuals are more likely to reach out to elected leaders when they share the same caste identity, a demand-side channel. It is important to distinguish between the demand and supply channels, but doing so can be challenging in this context.

To elucidate the mechanism behind the main effects, we analyze information on local meetings in the village council and whether village residents approached the chairperson for any problems. If increased confidence serves as the dominant channel and the rise in participation in NREGS is mediated by it, we should observe a positive and significant correlation between having the same jati chairperson and attendance at meetings, as well as approaching the leader. Similarly, we anticipate a similar correlation between political connections and individuals' contact with local decision-makers to address their own problems. The corresponding results are presented in Table 3.7. Panel A of Table 3.7 presents the estimated coefficient from the OLS, while Panel B reports the coefficient estimated via 2SLS. The first two columns in Table 3.7 display the coefficient of the same caste chairperson on village council meeting attendance. Our findings reveal no significant effect of caste-based political connections on attendance at village council meetings. Additionally, we find no effects of the same caste (*jati*) council chairperson on whether an individual approached the chairperson for a problem. The results are consistent for both OLS and 2SLS estimates, suggesting that the demand channel operating through self-confidence when having the

Table 3.7 Local Panchayat Meetings and Leaders Accessability

	(1)	(2)	(3)	(6)
	GS meetings		Approched	Solved local
VARIABLES	Any	Total	leaders	problems
		Panel A: OLS		
Same Caste Chairperson	0.015	0.001	0.001	0.013
	(0.012)	(0.054)	(0.012)	(0.008)
Observations	60,208	60,208	60,208	60,208
R-squared	0.144	0.048	0.111	0.107
		Panel B : 2SLS		
Same Caste Chairperson	-0.068	-0.253	-0.024	0.015
	(0.057)	(0.171)	(0.038)	(0.019)
Observations	60,208	60,208	60,208	60,208
R-squared	0.106	0.024	0.067	0.028
Dep. Var Mean	0.199	0.362	0.126	0.0435
Dep. Var Std	0.400	1.430	0.333	0.204
Sanderson-Windmeijer F stat	12.62	12.62	12.62	12.62
Village FE	Y	Y	Y	Y
Caste by Dist FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y

Note: *Same* is an indicator variable that takes on the value of 1 if a local and the village-council chief share the same surname and belong to the same caste (jati). Each column reports results from a separate regression. Controls include household-level variables such as head of the household's age, sex, marital status, religion, individual-level variables such as gender, age, education, and caste group by village-level variables such as average age, education, sex, etc. Standard errors are clustered at village council and caste-group levels. Robust standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

same caste village council chairperson might not be the dominant one.

However, if patronage politics are at play in the village election, we should expect local leaders to allocate employment to individuals who may support the voter as a reward for their backing during the election (Chandra, 2007). Although we lack data on individuals' support for the local leader of the same jati in the election, we do have information for each political candidate who ran for election in the last two panchayats and whether they received caste support during the election. We construct a dataset with candidate and election year information and run a regression of the following form:

Table 3.8 Likelihood of Winning and Caste Support in Elections

VARIABLES	(1) All	(2) All	(3) All	(4) SC/ST	(5) SC/ST	(6) SC/ST
Female	-0.072***	-0.057***	-0.074***	-0.011	-0.022	-0.028
	(0.017)	(0.018)	(0.022)	(0.017)	(0.020)	(0.025)
Education	0.033***	0.039***	0.049***	-0.012	-0.009	-0.006
	(0.011)	(0.012)	(0.012)	(0.008)	(0.009)	(0.010)
Same Caste Support	0.074**	0.098***	0.103**	0.044**	0.068***	0.074***
	(0.030)	(0.035)	(0.042)	(0.020)	(0.023)	(0.027)
Observations	1,432	1,432	1,432	1,432	1,432	1,432
R-squared	0.300	0.362	0.395	0.169	0.290	0.358
Dep. Var Mean	0.255	0.255	0.255	0.0977	0.0977	0.0978
Dep. Var Std	0.436	0.436	0.436	0.297	0.297	0.297
Year FEs	Y	Y	Y	Y	Y	Y
State by Year FEs	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
State FEs	Y	N	N	Y	N	N
District FFs	N	Y	N	N	Y	N
Village FEs	N	N	Y	N	N	Y

Note: Dependent variable is a dummy and takes on value one if the candidate won the local election and zero otherwise. Observations are candidates by election years. *Same caste support* is a dummy and takes on value 1 if the candidate in the election received support from the same caste zero otherwise. Controls include caste groups, religion, support of the political party, and wealthy people. Standard errors are clustered at the village council level. Robust standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

$$Win_{ivt} = \alpha_0 + \alpha_1 CS_{ivt} + \beta' X_{ivt} + \alpha_{v,d,s} + \tau_t + \epsilon_{ivt} \tag{3.3}$$

Win is a dummy if the candidate is a winner, α_1 measures the effects of having the same caste support in the election on the likelihood of winning the election, X is a vector of controls that includes candidate characteristics. α is a fixed effect for either village, districts, or state, τ is year-fixed effects as well as state-by-year fixed effects, and ϵ is an error term.

The estimation results using equation 3.3 are reported in Table 3.8. Across all specifications, we find a significant association between same-caste support and the probability of winning. The first three columns report the results for all candidates, while columns 4-6 present results for SC and ST candidates. We find that caste plays a significant role in determining electoral success, not only for all candidates but also for lower caste groups such as SC/ST. Based on these observations, we propose that caste is an important factor in local elections, and local leaders systematically allocate resources to fellow caste members as a reward for their votes. Considering the combined results from Table 3.8 and the main results from 3.2, we find substantial evidence of preferential targeting in NREGS at the jati level.

3.7 Conclusion

We study the impact of identity-based political connections on job applications, employment probability, and workdays in NREGS. We measure political connections by comparing the surnames of local residents with those of local leaders (village council chairperson). The results show that the probability of working in NREGS increases by 3-5.4 percentage points if the village head is of the same caste as the applicant. This corresponds to a 12-20 percent increase relative to the mean at the extensive margin. The effect of political connections on the number of days worked in NREGS is 11-20 percentage points higher for workers who share the same caste as the village-council chair. This represents an 11-20 percent increase relative to the mean worked day.

Our results suggest that the effects of political connections are more pronounced among lower-caste groups, indicating strategic targeting and favoritism within these groups. When we exclude surnames and consider only caste groups (SC, ST, OBC, and Forward) of residents and village leaders, we find no evidence of targeting. These results are consistent with findings on affirmative action but also reveal significant heterogeneity at the jati level in the strategic targeting of public goods. Overall, the results are in line with the literature on patronage politics, which can generate inequality and misallocation in the labor market. Providing better information to all residents about the program might address this issue and could help to mitigate patronage politics.

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APPENDIX

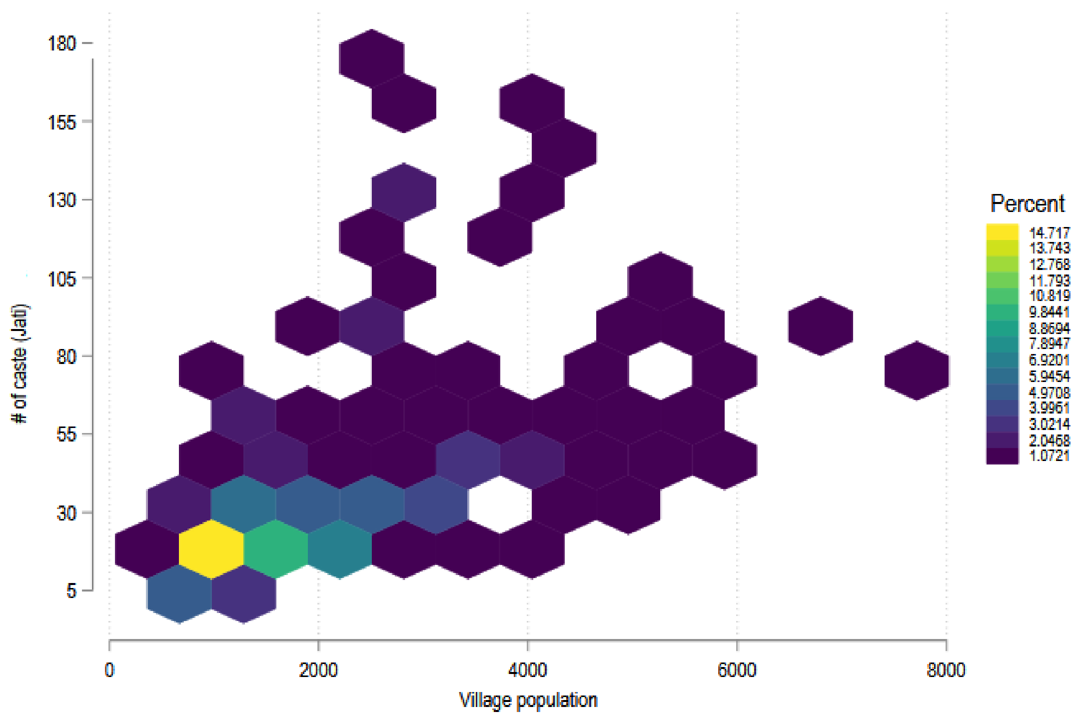

Figure 3.3 Same Caste Population by Village

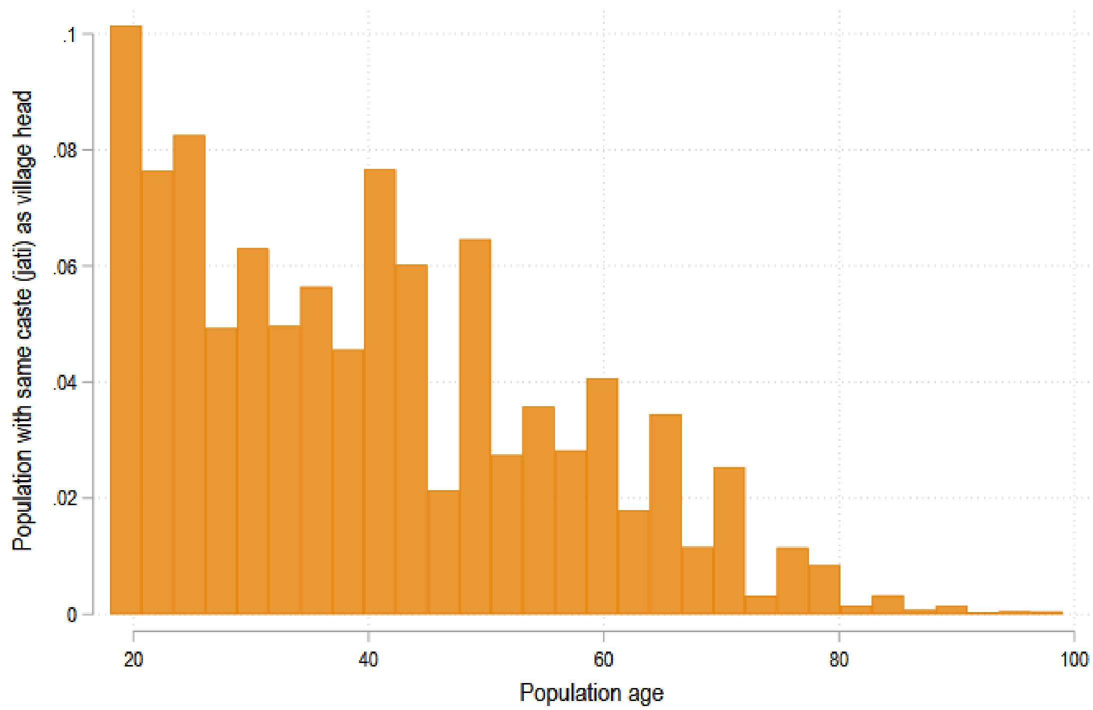

Figure 3.4 Same Caste Population by Age

Table C.1 Household Characteristics by Job Card Holding

	Total		Jobcard			
			Yes		No	
Variable	Mean	SD	Mean	SD	Mean	SD
Female head	0.128	0.334	0.115	0.319	0.132	0.339
Head's age	50.028	13.843	49.130	12.812	50.361	14.192
Head's education	4.779	4.613	3.776	4.077	5.152	4.743
Share of hhs						
. . . with no education	0.399	0.490	0.444	0.497	0.383	0.486
. . . up to 5th grade	0.172	0.377	0.223	0.416	0.153	0.360
. . . up to high school	0.379	0.485	0.305	0.460	0.407	0.491
. . . college or above	0.044	0.205	0.017	0.131	0.054	0.226
Married	0.823	0.381	0.848	0.359	0.814	0.389
Household size	4.561	2.442	4.459	2.201	4.598	2.525
Prime male	1.671	1.109	1.646	1.038	1.680	1.134
Prime female	1.565	0.956	1.554	0.883	1.569	0.982
Children	1.103	1.375	1.072	1.312	1.115	1.398
Hindu	0.886	0.318	0.906	0.292	0.878	0.327
Muslim	0.093	0.290	0.077	0.267	0.099	0.298
SC	0.216	0.411	0.326	0.469	0.175	0.380
ST	0.071	0.257	0.089	0.285	0.064	0.245
OBC	0.493	0.500	0.454	0.498	0.507	0.500
OC	0.221	0.415	0.131	0.337	0.254	0.435
Own any land	0.562	0.496	0.575	0.494	0.557	0.497
. . . Size of land	2.625	3.703	2.116	2.620	2.816	4.019
job card	0.271	0.444	1.000	0.000	0.000	0.000
# Obs.	79,646		21582		58,064	

Notes: Author's own calculation from survey data

Table C.2 Summary Statistics by Caste Groups for Job Card Holders

	Total		SC-ST		OBC		OTHER	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
				Panel A: Household				
Female head	0.115	0.319	0.124	0.329	0.110	0.313	0.106	0.308
Head's age	49.130	12.812	48.653	12.887	49.490	12.812	49.392	12.526
Head's education	3.776	4.077	3.267	3.969	4.009	4.098	4.587	4.137
Share of hhs								
. . . with no education	0.444	0.497	0.504	0.500	0.414	0.493	0.358	0.479
. . . up to 5th grade	0.223	0.416	0.205	0.404	0.239	0.426	0.220	0.415
. . . up to high school	0.305	0.460	0.260	0.439	0.321	0.467	0.392	0.488
. . . college or above	0.017	0.131	0.015	0.123	0.018	0.132	0.023	0.149
Married	0.848	0.359	0.838	0.369	0.854	0.353	0.858	0.349
Household size	4.459	2.201	4.481	2.181	4.529	2.315	4.142	1.801
Prime male	1.646	1.038	1.639	1.037	1.664	1.063	1.603	0.949
Prime female	1.554	0.883	1.545	0.879	1.587	0.915	1.471	0.776
Children	1.072	1.312	1.121	1.325	1.085	1.359	0.870	1.061
Own any land	0.575	0.494	0.505	0.500	0.655	0.475	0.517	0.500
. . . Size of land	2.116	2.620	1.839	2.608	2.353	2.654	1.853	2.395
#Obs	21582		8959		9806		2817	
			Panel B: NREGS & Individuals					
Age	39.886	15.651	39.527	15.521	40.129	15.786	40.184	15.573
Female	0.490	0.500	0.489	0.500	0.494	0.500	0.479	0.500
Education	4.696	4.538	4.223	4.496	4.894	4.561	5.512	4.429
Formally applied	0.333	0.471	0.346	0.476	0.325	0.468	0.322	0.467
Worked in NREGS	0.244	0.430	0.247	0.431	0.263	0.440	0.168	0.373
# of Days worked	37.894	29.836	34.412	25.982	42.694	33.138	28.146	22.205
Received allowance	0.020	0.141	0.022	0.145	0.020	0.141	0.017	0.129
Wanted to work more	0.120	0.325	0.139	0.346	0.111	0.314	0.094	0.292
Attended GS meetings	0.197	0.398	0.190	0.392	0.197	0.398	0.217	0.412
	62526		25991		28327		8208	

Notes: Author's own calculation from survey data

Table C.3 Summary Statistics by Caste Groups for All Households

Variable	Total Mean	Total SD	SC-ST Mean	SC-ST SD	OBC Mean	OBC SD	OTHER Mean	OTHER SD
				Panel A: Household				
Female head	0.128	0.334	0.131	0.338	0.126	0.332	0.127	0.333
Head's age	50.028	13.843	48.736	13.738	50.146	13.859	51.442	13.792
Head's education	4.779	4.613	3.969	4.426	4.748	4.585	5.903	4.682
Share of hhs								
. . . with no education	0.399	0.49	0.47	0.499	0.402	0.49	0.303	0.46
. . . up to 5th grade	0.172	0.377	0.175	0.38	0.174	0.379	0.162	0.369
. . . up to high school	0.379	0.485	0.313	0.464	0.38	0.485	0.464	0.499
. . . college or above	0.044	0.205	0.033	0.177	0.04	0.197	0.067	0.251
Married	0.823	0.381	0.815	0.388	0.826	0.379	0.828	0.378
Household size	4.561	2.442	4.487	2.297	4.674	2.542	4.403	2.384
Prime male	1.671	1.109	1.633	1.078	1.696	1.131	1.663	1.1
Prime female	1.565	0.956	1.519	0.913	1.594	0.979	1.559	0.958
Children	1.103	1.375	1.142	1.353	1.16	1.432	0.925	1.254
Own any land	0.562	0.496	0.468	0.499	0.615	0.487	0.564	0.496
. . . Size of land	2.625	3.703	1.933	2.606	2.765	3.895	2.95	4.026
Jobcard	0.271	0.444	0.392	0.488	0.25	0.433	0.16	0.367
#Obs	79646		22826		39256		17564	
				Panel B: NREGS & Individuals				
Age	40.081	16.139	39.418	15.765	40.066	16.189	40.936	16.438
Female	0.491	0.5	0.488	0.5	0.495	0.5	0.487	0.5
Education	5.58	4.895	4.757	4.779	5.501	4.887	6.783	4.822
Formally applied	0.115	0.319	0.161	0.368	0.103	0.305	0.083	0.275
Worked in NREGS	0.064	0.245	0.099	0.298	0.062	0.242	0.026	0.159
# of Days worked	37.059	29.85	33.685	26.248	41.507	33.01	28.528	22.76
Received allowance	0.008	0.088	0.01	0.101	0.008	0.09	0.004	0.06
Wanted to work more	0.032	0.176	0.055	0.228	0.027	0.161	0.015	0.122
Attended GS meetings	0.181	0.385	0.179	0.383	0.173	0.379	0.202	0.401
#Obs	249923		68398		126341		55184	

Notes: Author's own calculation from survey data

Table C.4 Political Connections and Its Effects on Employment

	(1)	(2)	(3)	(4)
	Panel A: Applied for Work			
Same Caste Chairperson	0.040*	0.038**	0.044**	0.042***
	(0.021)	(0.019)	(0.018)	(0.016)
Observations	62,526	62,526	62,526	62,526
R-squared	0.172	0.217	0.162	0.209
Dep. Var Mean	0.333	0.333	0.333	0.333
Dep. Var Std	0.471	0.471	0.471	0.471
	Panel B: Worked			
Same Caste Chairperson	0.025**	0.023**	0.029**	0.030***
	(0.013)	(0.011)	(0.011)	(0.010)
Observations	62,526	62,526	62,526	62,526
R-squared	0.207	0.260	0.201	0.256
Dep. Var Mean	0.244	0.244	0.244	0.244
Dep. Var Std	0.430	0.430	0.430	0.430
	Panel C: Days			
Same Caste Chairperson	0.091*	0.083**	0.105**	0.110***
	(0.053)	(0.042)	(0.046)	(0.039)
Observations	62,526	62,526	62,526	62,526
R-squared	0.210	0.263	0.205	0.259
Dep. Var Mean	0.982	0.982	0.982	0.982
Dep. Var Std	1.776	1.776	1.776	1.776
Village FE	Y	Y	Y	Y
District-by-caste FE	N	N	Y	Y
Controls	N	Y	N	Y

Notes: *Same* is an indicator variable that takes on the value of 1 if a local and the village-council chief share the same surname and belong to the same caste (jati). Each column reports results from a separate regression. Dependent variables Applied" and Worked" are dummy variables that take the value of 1 if a household member applied for a job and worked in NREGS, respectively, and 0 otherwise. "Days" is the number of days that an individual worked in NREGS. Controls include household-level variables such as head of the household's age, sex, marital status, religion, individual-level variables such as gender, age, education, and caste group by village-level variables such as average age, education, sex, etc. Standard errors are clustered at village council and caste-group levels. Robust standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

Table C.5 IV Estimates of Political Connections Impacts

	(1)	(2)	(3)	(4)
	Panel A: Applied for Work			
Same Caste Chairperson	0.091*	0.083**	0.085**	0.080**
	(0.052)	(0.034)	(0.034)	(0.035)
Observations	62,526	62,526	62,526	62,526
R-squared	0.066	0.067	0.067	0.067
Dep. Var Mean	0.333	0.333	0.333	0.333
Dep. Var Std	0.471	0.471	0.471	0.471
Sanderson-Windmeijer F stat	15.84	16.57	18.25	18.74
	Panel B: Worked			
Same Caste Chairperson	0.077**	0.048*	0.060**	0.054**
	(0.035)	(0.027)	(0.027)	(0.027)
Observations	62,526	62,526	62,526	62,526
R-squared	0.080	0.081	0.079	0.080
Dep. Var Mean	0.244	0.244	0.244	0.244
Dep. Var Std	0.430	0.430	0.430	0.430
Sanderson-Windmeijer F stat	15.84	16.57	18.25	18.74
	Panel C: Days			
Same Caste Chairperson	0.292**	0.205**	0.225***	0.201**
	(0.115)	(0.082)	(0.083)	(0.083)
Observations	62,526	62,526	62,526	62,526
R-squared	0.080	0.081	0.079	0.080
Dep. Var Mean	0.982	0.982	0.982	0.982
Dep. Var Std	1.776	1.776	1.776	1.776
Sanderson-Windmeijer F stat	15.84	16.57	18.25	18.74
	Panel D: First Stage			
Same Caste Reserved	0.106***	0.108***	0.132***	0.137***
	(0.028)	(0.024)	(0.032)	(0.029)
Observations	62,526	62,526	62,526	62,526
R-squared	0.487	0.490	0.511	0.513
Village FE	Y	Y	Y	Y
District-by-caste FE	N	N	Y	Y
Controls	N	Y	N	Y

Notes: *Same* is an indicator variable that takes on the value of 1 if a local and the village-council chief share the same surname and belong to the same caste (jati). Each column reports results from a separate regression. Dependent variables Applied" and Worked" are dummy variables that take the value of 1 if a household member applied for a job and worked in NREGS, respectively, and 0 otherwise. "Days" is the number of days that an individual worked in NREGS. Controls include household-level variables such as head of the household's age, sex, marital status, religion, individual-level variables such as gender, age, education, and caste group by village-level variables such as average age, education, sex, etc. Standard errors are clustered at village council and caste-group levels. Robust standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

Table C.6 Political Connections and Jobcard

VARIABLES	(1)	(2)	(3)	(4)
	Panel A: OLS Estimates			
Same Caste Chairperson	0.011	0.020	0.019	0.027
	(0.022)	(0.020)	(0.024)	(0.024)
Observations	79,646	79,646	79,646	79,646
R-squared	0.356	0.387	0.388	0.402
Dep. Var Mean	0.271	0.271	0.271	0.271
Dep. Var Std	0.444	0.444	0.444	0.444
	Panel B: 2SLS Estimates			
Same Caste Chairperson	0.159**	0.072	0.099*	0.093*
	(0.065)	(0.052)	(0.055)	(0.054)
Observations	79,646	79,646	79,646	79,646
R-squared	0.024	0.046	0.019	0.021
Dep. Var Mean	0.271	0.271	0.271	0.271
Dep. Var Std	0.444	0.444	0.444	0.444
Village FEs	Y	Y	Y	Y
Caste by districts FEs	N	N	Y	Y
Controls	N	Y	N	Y

Notes:*Same* is an indicator variable that takes on the value of 1 if a local and the village-council chief share the same surname and belong to the same caste (jati). Each column reports results from a separate regression. The dependent variable is a dummy variable that takes the value of 1 if a household has a job card and zero otherwise. Controls include household-level variables such as head of the household's age, sex, marital status, religion, individual-level variables such as gender, age, education, and caste group by village-level variables such as average age, education, sex, etc. Standard errors are clustered at village council and caste-group levels. Robust standard errors are reported in parentheses $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

Table C.7 Caste Heterogeneity in Effects of Political Connections

	(1) Applied	**(2)** Worked	**(4)** Days
		Panel A: SC-ST	
Same Caste Chairperson	0.060***	0.029**	0.103**
	(0.014)	(0.013)	(0.050)
Observations	25,911	25,911	25,911
R-squared	0.252	0.283	0.284
		Panel B: OBC	
Same Caste Chairperson	0.026**	0.025**	0.098**
	(0.013)	(0.012)	(0.047)
Observations	28,327	28,327	28,327
R-squared	0.210	0.249	0.250
		Panel C: Other	
Same Caste Chairperson	0.087***	-0.015	-0.061
	(0.030)	(0.023)	(0.092)
Observations	8,208	8,208	8,208
R-squared	0.224	0.164	0.170
Village FsE	Y	Y	Y
District-by-caste FEs	Y	Y	Y
Controls	Y	Y	Y

Notes: *Same* is an indicator variable that takes on the value of 1 if a local and the village-council chief share the same surname and belong to the same caste (jati). Each column reports results from a separate regression. Dependent variables Applied" and Worked" are dummy variables that take the value of 1 if a household member applied for a job and worked in NREGS, respectively, and 0 otherwise. "Days" is the number of days that an individual worked in NREGS. Controls include household-level variables such as head of the household's age, sex, marital status, religion, individual-level variables such as gender, age, education, and caste group by village-level variables such as average age, education, sex, etc. Standard errors are clustered at village council and caste-group levels. Robust standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

Table C.8 Do Village Council Members and Chairperson Have Differential Effects?

VARIABLES	(1) Applied	(2) Worked	(3) Days
		Panel A	
Same Caste Village Council Member	-0.006	0.016*	0.044
	(0.012)	(0.009)	(0.031)
Same Caste Village Council Chairperson	0.042***	0.029***	0.088***
	(0.016)	(0.010)	(0.032)
Observations	62,526	62,526	62,526
R-squared	0.209	0.256	0.258
		Panel B	
Same Caste Village Council Member	0.000	0.015	0.039
	(0.013)	(0.010)	(0.035)
Same Caste Village Council Chairperson	0.031	0.028**	0.089**
	(0.021)	(0.012)	(0.040)
Same Caste Member × Chairperson	0.024	0.017	0.022
	(0.041)	(0.025)	(0.084)
Observations	62,526	62,526	62,526
R-squared	0.217	0.260	0.261
Village FEs	Y	Y	Y
Districts by Caste FEs	Y	Y	Y
Controls	Y	Y	Y

Notes: *Same* is an indicator variable that takes on the value of 1 if a local and the village-council chief as well as village council members share the same surname and belong to the same caste (jati). Each column reports results from a separate regression. Dependent variables Applied" and Worked" are dummy variables that take the value of 1 if a household member applied for a job and worked in NREGS, respectively, and 0 otherwise. "Days" is the number of days that an individual worked in NREGS. Controls include household-level variables such as head of the household's age, sex, marital status, religion, individual-level variables such as gender, age, education, and caste group by village-level variables such as average age, education, sex, etc. Standard errors are clustered at village council and caste-group levels. Robust standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$

Table C.9 Different Caste (Jati) but Same Caste Group

	(1)	(2)	(3)	(4)
	Panel A: Applied for Work			
Different Caste (Jati) Same Caste Group	0.001	0.004	-0.002	-0.016*
	(0.014)	(0.010)	(0.011)	(0.009)
Observations	62,526	62,526	62,526	62,526
R-squared	0.151	0.200	0.207	0.209
Dep. Var Mean	0.333	0.333	0.333	0.333
Dep. Var Std	0.471	0.471	0.471	0.471
	Panel B: Worked			
Different Caste (Jati) Same Caste Group	0.011	0.013	0.004	-0.007
	(0.010)	(0.008)	(0.011)	(0.007)
Observations	62,526	62,526	62,526	62,526
R-squared	0.195	0.251	0.254	0.256
Dep. Var Mean	0.244	0.244	0.244	0.244
Dep. Var Std	0.430	0.430	0.430	0.430
	Panel C: Days			
Different Caste (Jati) Same Caste Group	0.030	0.032	0.009	-0.027
	(0.035)	(0.027)	(0.034)	(0.024)
Observations	62,526	62,526	62,526	62,526
R-squared	0.199	0.253	0.256	0.258
Dep. Var Mean	0.982	0.982	0.982	0.982
Dep. Var Std	1.776	1.776	1.776	1.776
Village FEs	Y	Y	Y	Y
Caste by districts FEs	N	N	Y	Y
Controls	N	Y	N	Y

Notes: Each column reports results from a separate regression. Dependent variables Applied” and Worked” are dummy variables that take the value of 1 if a household member applied for a job and worked in NREGS, respectively, and 0 otherwise. “Days” is the number of days that an individual worked in NREGS. Controls include household-level variables such as head of the household’s age, sex, marital status, religion, individual-level variables such as gender, age, education, and caste group by village-level variables such as average age, education, sex, etc. Standard errors are clustered at village council and caste-group levels. Robust standard errors are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$