

Does Participating in Public Works Increase Wage Bargaining Power in Private Sectors?

— Evidence from National Rural Employment Guarantee Scheme in India

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August 26, 2016

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Abstract

This paper estimates labor market effects of public works for participating households. Our research question has two folds. First, does working in public work program increase individual own wage bargaining power in private sectors (mostly as agricultural labor)? Second, does husband's (or wife's) participation increase spouses' wage bargaining power in private sectors? we use Dif-in-Dif method to estimate NREGS's effect on participating households' labor market outcomes.

Results show that men tend to receive a 10% higher wage and work less in private market if they participate in NREGS program in agricultural main season; and at the same time, their wives who are not working in public works tend to reduce labor supply by about 6-10 agricultural working days, and gain 7% higher agricultural daily wage. This result is consistent with a unitary household utility model and wage bargaining story. Intuitively, when husbands participate in public works program, the benefit obtained from this program may transmit to their wives as well, hence leading to a higher reservation wage for the latter.

Two interesting findings include heterogeneous effect by season, and by participation intensity. Specifically, men's own wage effect and spousal wage effect only exist in agricultural main season, not in off season, which means NREGS works may bring competition for labor in agricultural main season. Another interesting pattern is as husbands work more days and receive more payment from NREGS work, wives' labor supply show a stronger negative effect. This pattern may indicate income effect underlying these wage effects.

On the other hand, women's response is in similar magnitude but only appears in agricultural off season.

Key Words: Public works; NREGS; Wage effect; Rural labor market

1 Introduction

Public works is an important and widely used anti-poverty policy in developing countries, aside from cash transfer. Existing studies have documented different aspects related to public works, such as poverty targeting effectiveness, cost-benefit analysis, agricultural productivity improvement, and labor market effects and so on (e.g. Subbarao, 1997; Del Ninno et al., 2009; Zimmermann, 2014). An important but understudied secondary effect is wage bargaining effect in private sector of introducing public works programs.

Examining the effect on workers' wage bargaining power in private sector is important. On the one hand, by providing the rural poor with unskilled employment, especially in agricultural lean season, this policy is intended to help workers negotiate for a higher wage and better work environment with rural landlords who are usually oligopolists in rural labor market (Gaiha, 1996). On the other hand, however, there are also concerns that wage pressure may hurt employers, distort labor market, and worsen investment environment, etc. Therefore, it is important to examine whether public works program leads to a higher wage bargaining power and pin down the magnitude of the effect.

Ideally, we want to answer this question by estimating the equilibrium parameter in a Nash Bargaining game between workers and private sector employers. However, it requires matched employer and employee data to do so, and such data is usually not available in developing countries. Alternatively, the indirect approach of looking at this issue is to estimate wage effects of public works program. Public works, serving as the role of unemployment insurance, may pose an upward pressure on private sector wages via higher reservation wages. A positive wage effect indicates a *potentially* higher bargaining power although we don't directly observe the bargaining parameter. This paper employs the indirect approach of estimating wage effects of participating in public works program.

To differentiate our work from existing literature, two concepts need to be distinguished — Average Treatment Effect (ATE) and Average Treatment Effect on the Treated (ATT). The former averages treatment effect for both compliers (or participants) and noncompliers (or non-participants in program available areas), compared to NREGS-non-available areas. In the context of public works program (which usually has spillover effect), ATE tells two things. First, it provides a lower bound of wage effects for the real participants (or ATT). Second, in the case of large spillover effect, ATE is similar to general equilibrium wage effect, which tells wage effects regardless of participation status. In other words, even for those who do not participate in the

program, the presence of public program still has an option value of increasing reservation wages. While most existing studies have estimated Average Treatment Effect on private wages of public works program (e.g. Imbert and Papp, 2015; Zimmermann, 2012), this paper estimates Average Treatment Effect on actual participants to shed light upon wage bargaining effect.

In one word, our research question is, does participating in public work opportunities increase own and their spouses' wage bargaining power in private sectors (mostly as agricultural casual labor)? Specifically, we estimate the following two things — individual own response of agricultural wages to its participation in public works program; spouses' response to their partners' participation in the program. By doing this, this paper provides an implicit test of the bargaining story by empirically estimating the Average Treatment Effect on private sector wages for participants and their spouses. The estimate is important in evaluating welfare effect for program participants.

Our analysis is based on the context of India's Mahatma Gandhi National Rural Employment Guarantee Scheme (hereafter, NREGS program), the world's largest public programs so far according to World Bank report in 2015. It covers at least 15% of Indian population, providing at least 100 days of guaranteed wage employment in a financial year to each household whose adult members volunteer to do unskilled manual work at the minimum wage level. It focuses on works such as water conservation, drought proofing, irrigation works and land development. Starting from February 2006, the program gradually expanded throughout India by mid-2008. Like other public works program, NREGS is designed to help the poor stabilize income and smooth consumption in agricultural off-peak season.

Due to the self-targeting goal of NREGS, program participation is a result of self-selection by design. This selection issue undoubtedly poses challenges to identifying wage effects. For instance, if poorer people are more likely to work in the program and if they have different wage and employment paths from richer people, then the common trend assumption underlying dif-in-dif model may not hold. However, several facts help mitigate this concern to some extent. First, during our study period, low take-up rate (around 10%) eases the concern on households' self-selection into the program. Only a small fraction of lucky households participate in NREGS in the beginning, because workers are unaware of their entitlement to employment. This situation improves only when local volunteer organizations help them to learn to apply for a job card, demand work and open a bank account, tracking the payment of their wages and filing complaints ¹. Second, the accuracy of targeting is in general insufficient, as large numbers of

¹For instance, in Jharkhand state, local volunteers operate the program NREGA Sahayata Kendras to help

needy households are in the queue for job cards (Jha et al., 2008). This means, not only rural poor, but rural non-poor also participate in NREGS, although participation rate among the poor is slightly higher (Dutta et al., 2012). Inefficient targeting to some extent mitigates our concern on workers' self-selection into the program by poverty status or individual capability of finding a job in private sector. Third, we utilize the empirical approach used in the well known analysis of job displacement by Jacobson et al. (1993). This methodology allows to simultaneously estimate all pre-treatment trends of outcomes in addition to main treatment effect in current periods. If participants and nonparticipants present similar wage growth paths prior to the introduction of NREGS, then our estimation is less likely to be driven by self-selection.

This paper is related to the literature on the impact of workfare schemes in labor markets low-income countries (see Devereux and Solomon, 2006). Several studies have documented a positive earnings (or wage) effect of NREGS in agricultural labor market (e.g. Basu et al., 2009; Berg et al., 2014; Imbert and Papp, 2015; Azam, 2011). They find government hiring via public works programs may crowd out private sector work and therefore leads to a rise in equilibrium private sector wages. However, some other studies find zero or marginal earnings effect (e.g. Zimmermann, 2012). The most cited one is by Imbert and Papp (2015). All these studies focus on the effect of NREGS program on labor market equilibrium in terms of earnings and employment, and the current paper evaluates average treatment effect for the participants.

This paper is also different from existing literature in the data sources. Most above mentioned studies use repeated cross-sectional NSSO employment data in 2004-05 and 2007-08. We use household survey panel in 2005-06 and 2007-08, which allow us to control for individual level time-invariant unobservables. Moreover, with seasonal variations of labor market participation, we can get estimates for pre-treatment trends. In addition to not being able to control for individual unobservables, the limitation of repeated cross-sectional data makes it difficult to study intra-household interactions, which none of existing studies did. By studying how spouses respond to the partners' participation in public works program, our paper speaks to the literature of Added Worker Effect that's mostly based on developed countries.

Thirdly, our paper is analogous to the literature of unemployment insurance in developed countries. It's a long debate whether unemployment insurance reduces labor supply and increases reservation wage. Using censored regression model and Heckman two-stage estimation method, previous studies find that reservation wages of the unemployed decline 0.6 percent over time, and drops 15% when benefits are exhausted (e.g. Kiefer and Neumann, 1979; Fische, 1982). Our

workers secure work entitlements

paper finds similar results. Participation in NREGS increases men's agricultural wage, reduces wives' labor supply and increase wives' agricultural wage.

The current paper also talks to a small literature on welfare effects of NREGS (e.g. Basu and Sen, 2015; Ravi and Engler, 2015; Imbert and Papp, 2015). Ravi and Engler (2015) looks at poverty reduction effect of NREGS. Imbert and Papp (2015) find a welfare redistribution from rural labor employers to workers.

In addition, the potential flaw of the study by Imbert and Papp (2015) is the assumption of competitive market. Our paper assumes the opposite, i.e. employers having market power in hiring casual workers.

We find that if husbands participate in NREGS in agricultural main season, they tend to gain a 6 percent wage increase in agricultural labor market. At the same time, their wives who are not working in public works tend to reduce labor supply by about 6-10 agricultural working days, and gain 7% higher agricultural daily wage. This result is consistent with a unitary household utility model and wage bargaining story. Intuitively, when husbands participate in public works program, the benefit obtained from this program may transmit to their wives as well, hence leading to a higher reservation wage for the latter.

Two interesting findings include heterogeneous effect by season, and by participation intensity. Specifically, men's own wage effect and spousal wage effect only exist in agricultural main season, not in off season. The rational is that, in Karif/Rabi season there is already a relatively large labor demand in private sectors, thus the introduction of NREGS program brings competition for labor against private sector. In contract, in Summer season, labor demand is originally low, so NREGS work does not result in competition with private market. Another interesting pattern is as husbands work more days and receive more payment from NREGS work, wives' labor supply show a stronger negative effect. This pattern may indicate income effect underlying these wage effects.

On the other hand, women's own response is in similar magnitude but only appears in agricultural off season. And husbands do not respond to wives' participation.

The rest of paper is organized as below. Section 2, a brief literature review. Section 3 provides background information of NREGS program implementation. Section 4 builds a theoretical framework for this analysis. Section 5, data. Section 6, empirical model. Section 7, results. Section 8, conclusion.

2 Program Background

Here are some relevant facts about this program. NREGS is a three-phase rollout program, with 199 districts in Phase 1 (Feb 2006), 128 districts in Phase 2 (April 2007) and the remaining 261 districts in Phase 3 (April 2008).

This program issues a unique job card two weeks after they apply for NREGS works and get approved. Job cards are then used to keep track of days worked and payments received by each participant. A job card identification number also contains the information where the household resides in, such as state, district and village. Job card information is publicly available in NREGS official website to protect labors against corruption and fraud.

Several households may apply for a project and then work on it together, such as irrigation, road pavement etc. Within a household, more than one member can work in the project at the same time.

2.1 Wage and Rationing of NREGS work

The average daily wage on NREGS work is 81 Rupees, as opposed to about 55 Rupees/day for women and 86 Rupees for men working as agricultural casual labor (mostly casual labor hired by landlords).² Thus, NREGS work is usually seen more attractive than working as agricultural casual labor in private sector, especially for women. This is consistent with the initial aim of this program – to empower women by providing them employment opportunities.

Although the program asserts providing 100 days working opportunity for each household per year, there is actually an unmet demand of work. The average working days is roughly 35 days for all members of the household during that year.³ The rationing of demand for NREGS work is a reason that across Indian states the number of NREGS days provided is only weakly correlated with poverty (Dutta et al., 2012).

In terms of workers' time allocation, most of those (above 50% based on our survey data) who participate in NREGS work as agricultural or non-agricultural casual labor in private sector, with only a small fraction of them work in salary jobs.

2.2 Seasonality of NREGS works

There are three main agricultural seasons in India, i.e. Karif (June-Oct), Rabi (Nov to Feb) and Summer season (March to May). Karif season is concurrent with monsoon season, hence

²Authors' calculation based on our sample

³Authors' calculation based on our sample

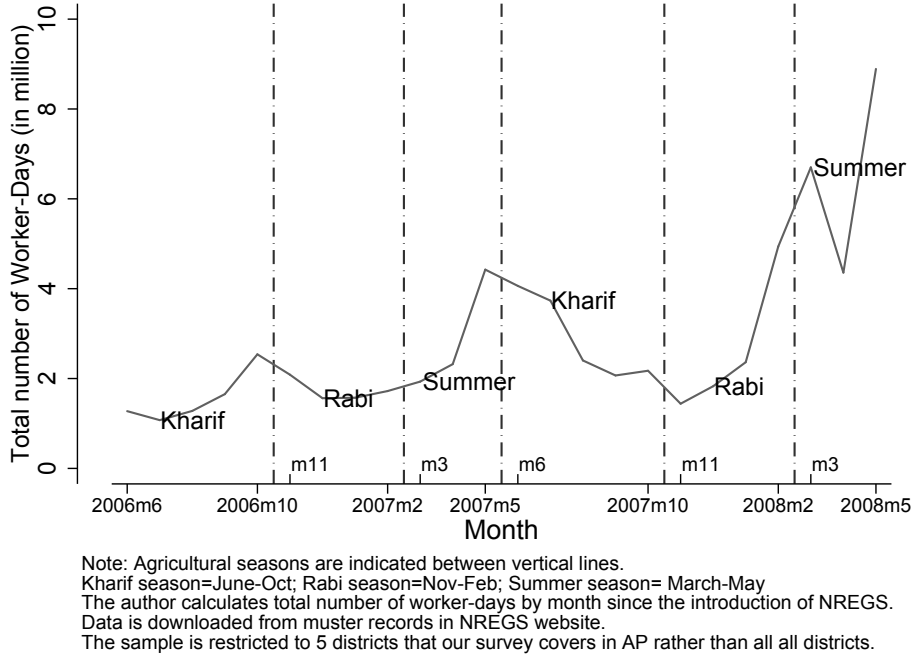


Figure 1: Seasonality of NREGS works, 2006.6-2008.5

agricultural busy season, and has a relatively large casual labor demand by landlords. The competition of private sector and public sector for rural labor makes it possible for a positive wage effect of this program. Rabi season is winter season with less labor demand in private agricultural sector. Summer season is very dry and hence agriculture lean season with little labor demand by landlords. The introduction of NREGS program helps to stabilize labor demand in lean seasons.

Figure 1 presents the seasonality of NREGS works in our survey districts in Andhra Pradesh state. The number of worker-days varies by season and month. To avoid competition with private sector labor demands, NREGS program provides more works in off-agricultural season and less in agricultural busy season. This pattern in our data is consistent with existing studies (e.g. Maiorano, 2014; Imbert and Papp, 2015).

3 Modeling and Hypothesis

We use the framework of McCall.

Use w_r to represent reservation wage, and w actual job offer, b is the income one can get if not working in private sector

$$w_r - b = \int_0^\infty (w - w_r) dF(w) \quad (1)$$

To rearrange it,

$$b = w_r - \int_0^\infty (w - w_r) dF(w) \quad (2)$$

In the context of a public program that guarantees some employment or cash-on-hand, the utility (in terms of income) that one can get from opting out of private sector increases if one participates in NREGS program. Note also that the RHS of equation 2 is a monotonically increasing function of w_r . As a result, Participating in NREGS increases reservation wage.

While looking at spousal response to partners' participation in public works program, we need to assume a unitary household model and intra-household sharing mechanism — the benefit from NREGS program may transmit from participants to non-participant members in the same household. Compared to individuals from non-participating households, these non-participants from treated households have better fallback options, hence more likely to have a higher bargaining power in negotiating wages with landlords in private labor markets. [To be added later]

4 Data

Our sample includes 471 villages in 5 districts in Andhra Pradesh, i.e. Visakhapatnam, Nellore, Kadapa, Warangal and Nalgonda. Our data comes from three sources. First, Rural Poverty Reduction Project survey data in 2004, 2006 and 2008 agricultural year; second, NREGS administrative data from the official website; third, Indian population census data.

The survey data contains NREGS job card identification number and detailed information of household members' labor market participation (other than in NREGS programs), such as demographic backgrounds and salary or wage in each work by season. 2004 survey was the first wave survey data, mostly conducted during March-August 2004. The interview asks the subject to recall information during June 2003-May 2004. Then, 2006 survey was conducted intensively during August and October 2006; subjects were asked to recall information during June 2005-May 2006. Similarly, 2008 survey was conducted during September-December 2008, and subjects recalled information between June 2007-May 2008. Our survey data almost two waves of survey data prior to the introduction of the program, and one wave after.

The administrative data (muster rolls) is downloaded from nregs official website. It contains job card identification number, information on NREGS participation for each participant, such as the start and end date of working at a specific project in NREGS program, total payment during each recorded working period. Because our survey data is at person-season level, we need to aggregate NREGS participation information into season level as well.

Population census data contains village information such as rainfall and other village characteristics.

Since both survey and administrative data has job card information and individual names, we use these to merge survey households and NREGS-participating households from administrative data. The final data is in the form of household-member-season. For each member in the household, we have labor market participation information in each season.

4.1 Program roll out and take-up

Table 1 documents how NREGS program rolled out in our sampling villages and the variation of program take-up. Our survey divides the year into three agricultural seasons based on rainfall amount, i.e. Kharif season =June-October, Rabi season = November-Feb, Summer season=March-May.

The start of NREGS program in a village is defined by the first day that any household starts to work in this public program. In other words, suppose NREGS program is already available in a village and households can apply for it, but none of them really do, hence no NREGS work is going on in the village, then this village is still viewed as a non-NREGS village. In this way, we find the rolling out process of this program at village level. Our sample contains 471 villages in 5 districts. Table 1 shows at the end of the survey window, only 45 villages still didn't have access to NREGS.

Table 1: Program phased roll-out at village and individual level

Survey year	season	Villages			Individuals		
		Starting NREGS	With NREGS	Without NREGS	# of nonpart.	# of participants	participation rate
2006	Kharif	0	0	471	8509		
2006	Rabi	2	2	469	8494		0
2006	Summer	219	221	250	8342	68	1.90%
2007		75	296	175			
2008	Kharif	42	338	133	8156	779	12.50%
2008	Rabi	11	349	122	8254	664	9.81%
2008	Summer	77	426	45	7663	1,165	15.97%
post survey		45					
total # of villages		471					

Table 1 also suggests NREGS takes a long time to take off, when we compare village roll out and households take up rate. Although half of the villages already had access to NREGS in May 2006 (phase 1), only 2% individuals actually worked in it. Phase 2 districts started in April 2007. Our data does not cover this period. Starting in June 2007, take up rate increased

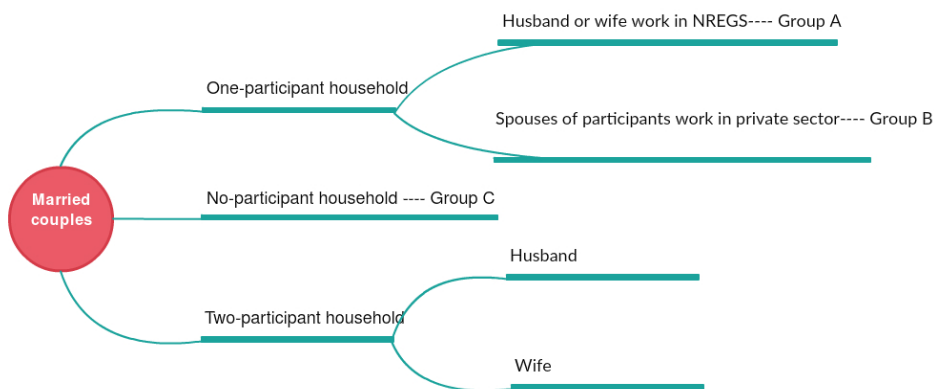


Figure 2: Grouping of the sample

to around 12.5% in our sampling villages.

We exploit the fact that this program was taken up gradually at individually level, treating three seasons in 2006 survey year as pre-treatment periods, and the corresponding seasons in 2008 as post periods.

4.2 Descriptives

Table 2 presents a comparison for three groups of individuals based on their own participation status in NREGS program and their spouses' participation status. Karif season and Rabi season are aggregated as agricultural main season. First I divide all households in the sample into three types depending on couples' participation status in NREGS program, see Figure 2. Type 1 households, or "one-participant households", have either wife or husband participate in NREGS program; Type 2 households, or "no-participant households", have neither of the spouse participate in NREGS; Type 3 households, or "two-participant households", have both of the spouse participate in it. Second, I further divide individual workers from "partially-participating households" into two groups – participants and non-participants, as shown in block 1 and block 2 in Table 2.

We will estimate the spillover effect of participating NREGS by comparing non-participating spouse from partially-participating households and workers from non-participating households. The last two blocks in Table 2 presents the comparison for these two groups. Panel 2 presents average number of days an individual works if he/she does that type of work. Panel 3 is informative in terms of potential wage effect. For instance, in agricultural season, a female non-participant from treated households on average earns 48.7 Rupees/day, as opposed to 48 Rupees/day for a female worker from control households. In addition, the former works on average 57.7 days as agricultural wage labor, as opposed to 58.4 days in the latter group. The fact that non-

Table 2: Descriptive statistics – three group comparison

	Part. from partial-part. Households				Non-Part. from partial-part. households				Individuals from non-part households				Part. from all-part. Households			
	Main season		Summer season		Main season		Summer season		Main season		Summer season		Main season		Summer season	
	Wife	Husband	Wife	Husband	Wife	Husband	Wife	Husband	Wife	Husband	Wife	Husband	Wife	Husband	Wife	Husband
Total # of observations	356	200	201	116	200	356	116	201	3457	3457	1955	1955	327	326	291	290
<i>NREGS work and payment</i>																
working days/season	13.5	10.3	14.0	9.5												
payment (Rupee/season)	981.4	755.3	977.7	644.0												
unit payment (Rupee/day)	71.5	71.1	67.7	66.4												
<i>Working days by job type</i>																
Ag casual labor (days)	65.2	53.7	30.9	31.9	57.7	57.3	27.5	32.3	58.4	55.3	32.9	34.2	61.3	54.1	28.2	27.0
# of workers	280	105	139	61	128	163	65	68	2062	1424	902	695	273	217	204	147
non-Ag casual labor (days)	16.2	20.8	22.1	17.6	30.9	45.0	22.4	37.5	27.6	44.4	23.6	33.1	16.4	19.5	21.7	22.3
# of workers	356	200	201	116	49	105	51	89	596	782	580	638	327	326	291	290
day_salary (days)		138.0		85.2	112.0	125.6	90.5	87.7	114.5	128.2	84.9	88.2	91.5	126.1	66.4	76.4
# of workers	0	11	0	5	3	43	2	22	74	312	39	180	2	16	5	17
day_selfemp (days)	42.5	85.0	24.2	53.5	88.8	81.9	60.0	59.9	82.7	85.6	60.6	63.0	67.5	75.4	40.9	50.3
# of workers	8	15	11	10	12	35	7	28	294	504	161	268	15	30	9	20
<i>Wage by job type (Rupee/day)</i>																
Ag casual labor	50.0	79.3	48.6	67.4	48.7	78.6	43.9	74.1	48.0	76.7	46.3	74.7	49.3	76.5	46.6	69.5
non-Ag casual labor	71.0	73.0	67.0	68.9	66.9	86.8	62.5	85.6	68.1	89.7	64.8	79.3	69.4	75.3	67.3	71.3
salary wage	2034.8			1900.0	1466.7	3285.8	2850.0	3371.3	1937.1	3149.9	1635.4	3507.9	3250.0	4675.0	1650.0	2281.3
self emp wage	50.0	113.3	53.2	98.9	217.5	134.0	71.4	153.0	123.4	214.2	127.8	241.5	62.1	175.5	275.0	214.2

The sample is divided into three types of households, based on individual participation status in NREGS program and their spouses' participation status. Type 1 households, or "one-participant households", have either wife or husband participate in NREGS program; Type 2 households, or "no-participant households", have neither of the spouse participate in NREGS; Type 3 households, or "two-participant households", have both spouses participate in it. Then type 1 individual workers from "partially-participating households" into two groups – participants and non-participants, as shown in block 1 and block 2

participating spouses from treated households receive a higher wage and work fewer days than individuals from control households is consistent with our empirical results.

The first block about participants from partially-participating households in Table 2 answers the following questions. First, it shows how many days and how much do they earn in each season. Females participate in NREGS for more days than males, have equal daily payment, and on average earning more than males. Such results are consistent with the initial goal of empowering women. Second, it answers what else these participants do other than NREGS works. Quite surprisingly, Panel 2 shows that they work for no fewer days than people not participating in NREGS, consistent with an earlier finding of unmet demand of nregs works. Panel 3 shows their wages in private sector are a bit higher than non-participants.

According to National Sample Surveys (NSS), in 2006-07, the average monthly per capita expenditure (MPCE) for rural households is 695 Rupees or about \$14. About 52 percent of this MPCE was spent on food⁴. NSSO survey on situation assessment of farmers (2003) estimate that a farmer household, on the average, has a total monthly income of 2115 Rupees from all sources (Bahala, 2008).

5 Empirical Model and Identification

In a village with NREGS program, some households apply for and finally get work opportunities from this program, whereas other households may either not apply or finally do not pass final review process. We call the first type of households "participating households" where either husband or wife (but not both) participates in NREGS program, and the second type "non-participating households" if neither husband nor wife participates in the program. We have dropped households where both husband and wife work in NREGS program, because in those families, it's unclear whether individual i 's wage change is a reaction to its own participation to the program, or a reaction to its spouse's participation. For ease of comparison, we dropped such families, about 1/4 of treated households.

NREGS gradually rolls out to 426 out of 471 villages in our sample areas during 2006-2008. To estimate ATT on wages, theoretically there are two different comparisons we could make. One is comparing participating households in NREGS-available villages to households in NREGS-non-available villages, and the other is comparing participating households to non-participating households in NREGS-available villages. While the former comparison is the conventional way

⁴<http://www.prb.org/Publications/Articles/2008/howindianslive.aspx>

of estimating ATT, instrumental variable methodology needs the assumption that the rolling out process needs to be random across villages. Alternatively, the variation of participation status in the second comparison purely comes from individual self selection, which poses a threat to identify the wage effect. Considering the fact that random assignment of NREGS at village level seems too strong an assumption, we use the second comparison and try to identify ATT by mitigating the concerns due to self-selection. Because even non-participants are also faced with the "option value" of the availability of the program, this comparison will yield an underestimated wage effect.

The identification strategy for ATT is based on the assumption that the distribution of NREGS job opportunities is exogenous to households, so that without NREGS job, individual wage growths in Treatment and Control households would have identical trends. However, if some households (e.g. elite class) have manipulation power on the distribution of job opportunities, then this assumption will be violated. For instance, if households with high-skill non-participants are more likely to obtain NREGS work opportunities, then the effect of receiving public works on non-participants' private sector wages will be confounded by non-participants' skill/ability.

Fortunately, with seasonal data in our sample, we can use the model in the well cited job displacement study by Jacobson et al. (1993) to identify self response and spousal response of labor market outcomes to participating in NREGS for the participating households. It's essentially a dif-in-dif framework but allows to simultaneously estimate all pre-treatment trends of outcomes in addition to main treatment effect in current periods. If participants and nonparticipants present similar wage growth path prior to the introduction of NREGS, then our estimation is less likely to be driven by self-selection.

Empirical specifications for self-response analysis are as follows. In estimating individual response to its own participation in the program, we define the treatment indicator D_{it} as follows: $D_{it} = 1$ if individual i works in NREGS program at time t , and otherwise $D_{it} = 0$. I estimate the same model separately for wives and husbands. In the parallel analysis of spousal response to the partner's participation, treatment indicator is defined in the same way, but left hand side variables in the regression model are the spouse's outcomes rather than individual i 's own outcomes.

We have two years of data, 2006 and 2008, each with three seasons. In 2006, no one gets treated. In 2008, participants move in and out of NREGS program during the three seasons. The ideal comparison is comparing labor market outcomes for participants and non-participants in each season in 2008, with three seasons in 2006 as three pre-treatment trends. For instance, I

extract participants and non-participants for Karif season in 2008, and the same sample in three seasons in 2006. Then I estimate the following model —

$$y_{it} = \alpha_i + \sum_{s \in S} \lambda_t^s + X_{it}\beta + \left(\sum_{s=-2,-1} D_{it}^s \gamma_s + D_{it}^0 \gamma_0 \right) + \varepsilon_{it} \quad (3)$$

where α_i captures individual fixed effect in each season, X_{it} includes age squares and reading ability for self and spouses, caste and dependency ratio interacted with time. λ_t^s captures time trends by season, $S = \{-2, -1, 0\}$, with -2 standing for the second to last season before the current Karif season (i.e. Rabi season in 2006), and -1 standing for the last season before current treatment season (i.e. Summer season in 2006), and 0 for current season (i.e. Karif season in 2008). The first season in 2006 is used as baseline season, hence omitted in the regression. Accordingly, γ_{-2} and γ_{-1} give pre-treatment effects in, respectively, Karif season in 2006 and Rabi season in 2006, and γ_0 gives the treatment effect of interest. If estimates for γ_{-2} and γ_{-1} are close to zero both in the sense of statistical significance and economic magnitude, but γ_0 is not, then it eases our concern on identification issue.

Similarly, we would like to estimate the treatment effect for participants who work in NREGS in Rabi or Summer season in 2008 using the same model. However, the drawback of doing these three estimations separately by season is that it's hard to get statistical significance, because of the low take-up rate in the program. Therefore, I append these three sample together, and estimate equation 4, assuming that treatment effect in Karif and Rabi seasons are the same. This makes sense because both of them are agricultural main seasons with substantial agricultural labor demand. Therefore, our heterogeneous effect by season is split into agricultural main and off season. All the results given in the paper are based on this model. I have checked that they are similar to results obtained from Equation 3.

$$y_{it} = \alpha_i + \sum_{s \in S} \lambda_t^k + X_{it}\beta + \left(\sum_{s=-2,-1} D_{it}^k \gamma_s + D_{it}^{0m} \gamma_{0m} + D_{it}^{0s} \gamma_{0s} \right) + \varepsilon_{it} \quad (4)$$

The second identification threat is husbands and wives may make joint decision on who works in NREGS and who works in private labor market. In that case, ε_{it} doesn't satisfy i.i.d. assumption which is necessary to get consistent estimates.

6 Results

For all the results reported below, it seems that statistical significance is not quite high. This is because of the small number of treated individuals. Having dropped families where both spouses participate in NREGS, the final sample contains on average 90 men or 150 women participants in each season. The following sections present individual own response and spousal response, and then a pattern of these effects.

6.1 Self response to participating in NREGS

Combining men's and women's own response to participation in NREGS, we reach the following results.

The first thing we can observe from Table 3 and Table 4 is about pre-treatment effect. For both men and women, wage paths of participants do not differ from that of non-participants in terms of statistical significance and economic magnitude. However, women's employment paths do differ between these two groups. It seems to say, participant wives would have worked less if without this program.

Second, look at wage effect and employment effect. Table 3 shows that participant men gain a positive wage effect as agricultural labor, in agricultural main season. This effect probably indicates that the introduction of NREGS has led to competition for labor between private sector and public works.

Different from men's effect, women's effect concentrates in agricultural off season. Table 4 shows women's participation as casual wage labor in private sector has increased in lean season. This result indicates that NREGS has helped to generate more employment for women, especially in agricultural off season. At the same time, due to more attractive employment opportunities in NREGS, participant women work less in private sector as agricultural daily worker, and they earn a higher wage if they do. These results probably suggest a crowding out effect for women in off season.

The fact that treatment effects are concentrated in off season is also consistent with that in Imbert and Papp (2015), although they do not distinguish men and women. As they argue, NREGS work is mainly offered in off season. This reason may also apply in our data, as Figure 1 shows more work is going on in Summer season. However, we still have to explain why wife's own response mainly occurs in agricultural off season, and men's in main season.

Table 3: Men's Self Response, using NREGS payment 300 Rupees as cut off

	Casual labor			Ag casual labor		
	(1) Wage	(2) Days	(3) Work Y/N	(4) Wage	(5) Days	(6) Work Y/N
Treatment * Rabi season, 2006	-0.01 (0.01)	-0.02 (1.79)	-0.01 (0.01)	-0.00 (0.01)	-1.29 (1.66)	-0.00 (0.01)
Treatment * Summer season, 2006	0.01 (0.01)	2.97 (2.29)	-0.00 (0.02)	0.01 (0.01)	0.01 (2.28)	-0.01 (0.02)
Treatment * Main season	0.02 (0.04)	3.63 (4.83)	0.05 (0.04)	0.07* (0.04)	-5.20 (4.66)	0.02 (0.05)
Treatment * Off season	0.01 (0.04)	4.17 (4.37)	0.09 (0.06)	0.00 (0.05)	2.83 (4.70)	0.01 (0.06)
Observations	14790	14790	29532	12275	12275	29532
R^2	0.816	0.715	0.725	0.810	0.706	0.697

All models include a full set of individual and season fixed effect, and observable covariates. Standard errors are clustered at household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Women's Self Response, using NREGS payment 300 Rupees as cut off

	Casual labor			Ag casual labor		
	(1) Wage	(2) Days	(3) Work Y/N	(4) Wage	(5) Days	(6) Work Y/N
Treatment * Rabi season, 2006	0.00 (0.01)	-0.86 (1.26)	0.01 (0.01)	0.01 (0.01)	-0.97 (1.19)	0.01 (0.01)
Treatment * Summer season, 2006	0.01 (0.01)	-5.97*** (2.14)	-0.03 (0.02)	0.00 (0.01)	-6.67*** (2.17)	-0.04* (0.02)
Treatment * Main season	0.04* (0.02)	3.50 (2.94)	-0.04 (0.03)	0.03 (0.02)	2.24 (2.84)	-0.05 (0.03)
Treatment * Off season	0.08*** (0.02)	4.94* (2.91)	0.04 (0.03)	0.08*** (0.03)	-5.45* (3.16)	0.01 (0.04)
Observations	16731	16731	29532	15911	15911	29532
R^2	0.780	0.665	0.713	0.792	0.677	0.694

Note: Casual labor includes both ag and nonagricultural casual labor who earns daily wage. Column 1, 2 and 4, 5 restrict the sample to individuals who work a positive number of days as agricultural wage labor, whereas column 3 and 6 also include individuals who don't work as agricultural wage labor.

All models include a full set of individual and season fixed effect, and observable covariates. Standard errors are clustered at household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.2 Results of spousal response

Table 6 and Table 5 estimate spouses' response to the partners' participation in NREGS, for participating households. Our findings are as follows.

First, if husbands work in NREGS, their wives tend to gain a positive wage effect and also work less in agricultural labor market. This is consistent with our story of wage bargaining and unitary household model. On the other hand, however, when wives work in NREGS, mostly husbands almost do not show any reaction in either ag labor market or casual labor market as a whole

Second, combining results of self response, we find an interesting phenomenon — own and

spousal response go side by side. For instance, in main season, when participant husbands gain a positive wage effect, hence a possible income effect, their wives then respond by a positive wage and negative labor supply effect. Another example is in lean season. Participant wife gains a positive wage effect, and at the same time, husbands presents a negative labor supply effect, although not statistically significant.

Table 5: Spousal response, wife to husband, NREGS payment 300 Rupees above

	Casual labor			Ag casual labor		
	(1) Wage	(2) Days	(3) Work Y/N	(4) Wage	(5) Days	(6) Work Y/N
Treatment * Rabi season, 2006	-0.01 (0.01)	-1.48 (1.75)	-0.02 (0.01)	-0.01 (0.01)	-1.54 (1.77)	-0.01 (0.01)
Treatment * Summer season, 2006	0.01 (0.01)	3.57 (2.72)	-0.02 (0.03)	0.02 (0.01)	3.20 (2.83)	-0.01 (0.03)
Treatment * Main season	0.05 (0.04)	-5.97 (4.97)	-0.07* (0.04)	0.06* (0.03)	-6.86 (4.69)	-0.10** (0.05)
Treatment * Off season	0.02 (0.04)	4.68 (3.99)	0.02 (0.05)	0.04 (0.04)	0.56 (4.28)	0.02 (0.06)
Observations	16731	16731	29532	15911	15911	29532
R^2	0.780	0.664	0.713	0.791	0.676	0.694

Notes: Casual labor includes both ag and nonagricultural casual labor who earns daily wage. Column 1, 2 and 4, 5 restrict the sample to individuals who work a positive number of days as agricultural wage labor, whereas column 3 and 6 also include individuals who don't work as agricultural wage labor.

All models include a full set of individual and season fixed effect, and observable covariates. Standard errors are clustered at household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: spousal response, Husband to wife, NREGS payment 300 Rupees above

	Casual labor			Ag casual labor		
	(1) Wage	(2) Days	(3) Work Y/N	(4) Wage	(5) Days	(6) Work Y/N
Treatment * Rabi season, 2006	0.00 (0.01)	-0.37 (1.41)	0.01 (0.01)	-0.01 (0.01)	-1.42 (1.47)	0.00 (0.01)
Treatment * Summer season, 2006	0.01 (0.01)	-2.45 (1.98)	0.01 (0.02)	0.00 (0.01)	-3.75* (2.14)	0.02 (0.02)
Treatment * Main season	-0.01 (0.04)	-4.54 (3.61)	0.00 (0.03)	0.02 (0.03)	-3.19 (4.23)	0.02 (0.04)
Treatment * Off season	-0.03 (0.04)	-0.92 (3.44)	-0.01 (0.04)	-0.03 (0.04)	-6.01 (4.16)	-0.03 (0.04)
Observations	14790	14790	29532	12275	12275	29532
R^2	0.816	0.715	0.725	0.809	0.706	0.697

All models include a full set of individual and season fixed effect, and observable covariates. Standard errors are clustered at household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.3 Pattern of treatment effects

We test if our earlier estimates rely on the definition of treated households. In the context of wage bargaining story, a tiny amount of monetary benefit from the program may not be helpful enough to raise reservation wage. In the main results given above, as long as husband/wife participates

in the program and receives more than 300 Rupees, then their households are counted as treated households. In robustness checks, I redefine treated households as, having the spouse work in the program and receive money greater than a certain amount of Rupees. I tried several thresholds, i.e. 100, 200, ..., 800.

An interesting pattern is, as husbands or wives work more days and receive more payment from NREGS work, the according effects are getting stronger. This probably indicates the role of income effect underlying wage and employment response.

7 Conclusion and Discussion

This paper estimates labor market effects of public works for participating households. Our research question has two folds. First, does working in public work program increase individual own wage bargaining power in private sectors (mostly as agricultural labor)? Second, does husband's (or wife's) participation increase spouses' wage bargaining power in private sectors? we use Dif-in-Dif method to estimate NREGS's effect on participating households' labor market outcomes.

Results show that men tend to receive a 10% higher wage and work less in private market if they participate in NREGS program in agricultural main season; and at the same time, their wives who are not working in public works tend to reduce labor supply by about 6-10 agricultural working days, and gain 7% higher agricultural daily wage. This result is consistent with a unitary household utility model and wage bargaining story. Intuitively, when husbands participate in public works program, the benefit obtained from this program may transmit to their wives as well, hence leading to a higher reservation wage for the latter.

Two interesting findings include heterogeneous effect by season, and by participation intensity. Specifically, men's own wage effect and spousal wage effect only exist in agricultural main season, not in off season, which means NREGS works may bring competition for labor in agricultural main season. Another interesting pattern is as husbands work more days and receive more payment from NREGS work, wives' labor supply show a stronger negative effect. This pattern may indicate income effect underlying these wage effects.

The identification of our estimates relies on the assumption that, conditional on observables included in our model, the distribution of NREGS job opportunities is exogenous to households. In other words, without NREGS job, individual wage growths in Treatment and Control households would have identical trends. By using the methodology used in JLS's job displacement

analysis, we show that the wage effects we get are not driven by unobserved pre-treatment trends. Still, the second identification threat is joint decision making by couples.

8 Appendix

Data organization. [to be written]

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