Does Providing Public Works Increase Workers' Wage Bargaining Power in Private Sectors?

— Evidence from National Rural Employment Guarantee Scheme in India

Yanan Li [*][1] and Yanyan Liu [†][2]

[1]Department of Applied Economics & Management, Cornell University

[2]International Food Policy Research Institute

PRELIMINARY RESULTS. PLEASE DO NOT CITE.

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[*]yl2294@cornell.edu

[†]Y.Liu@cgiar.org

Abstract

This paper answers the question that, does having some household members working in public work program increase other household members' wage bargaining power in private sectors? we use DID matching method to estimate NREGS's effect on participating households' labor market outcomes. Results show that non-participants from participating households (i.e. households with at least one person participating in the program) receive a 5% wage increase compared to individuals from non-participating households. This result is consistent with a unitary household utility model and wage bargaining story. Intuitively, when a household participates in the program, the benefit obtained from this program may transmit from participants to household non-participants, hence leading to a higher reservation wage for the latter. This wage effect only exists in Karif season, an agricultural busy season.

Key Words: Wage Bargaining; NREGS; Wage effect; Rural labor market

1 Introduction

Previous studies have documented a positive wage effect of National Rural Employment Guarantee Scheme (hereafter, NREGS program). 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 (e.g. Basu et al., 2009; Berg et al., 2014; Imbert and Papp, 2015). Most current empirical studies use district level variation of NREGS rolling out, estimating average treatment effect (ATE) of the program at district level. ATE is relevant in that it says, for two identical individuals who are not working in NREGS, one from NREGS district but the other not, then the first individual tends to receive a higher wage in private sector than the latter.

However, ATE measurement is silent on differential wage effects for program participants and non-participants within the same district. Intuitively, in a district with access to NREGS program, it's likely that NREGS participants enjoy a higher positive wage effect than non-NREGS participants. In the same vein, it's also likely that non-participants from an NREGS-participating-household enjoy a higher positive wage effect than individuals from a non-NREGS-participating-household.[1] To say something about such differential effects, we need to estimate Average Treatment Effect for the treated (ATT). For the ease of empirical analysis, the current paper focuses on the second comparison, by restricting the sample to non-NREGS-participants.

There could be multiple channels leading to such differential effects. One is through bargaining story. When NREGS program provides a household with extra employment opportunities (and usually with a higher wage), assuming a unitary household utility model where household members share benefits from NREGS participation, such employment opportunities help to secure household subsistence needs. As an indirect result, it may be followed by a higher reservation wage of non-NREGS-participants in the same household as well as that of program participants.

Thus, our hypothesis is, non-participants from NREGS-participating households tend to receive a higher private sector wage than individuals from non-NREGS-participating households. For this story to hold, we need the following two assumptions — 1) a unitary household utility model where household members share benefits from NREGS participation and 2) more job offers to transmit higher reservation wage to a higher real wage.

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[1]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 "NREGS-participating households" where at least one person participates in NREGS program, and the second type "non-NREGS-participating households" where nobody participates in the program. We are going to stick to these terms throughout the paper.

In one word, our research question is, does the fact that some household members receiving public work opportunities increase other household members' wage bargaining power in private sectors (mostly as agricultural casual labor)? This paper provides an implicit test of the bargaining story by empirically estimating the Average Treatment Effect on private sector wages for nonparticipants from NREGS-participating households (ATT). This measurement is also important in evaluating welfare effect for program participants.

Empirically, we use dif-in-dif matching method to pin down this effect. Treated households are defined as households with at least one member participating in the program, and control households are defined as households with nobody participating in the program *ever*.

We find non-participants from participating households receive about 5% higher wage compared to individuals from non-participating households. This wage effect only exists in Karif season which is an agricultural busy season. The rational is that NREGS work brings competition for labor against private sector, when there is already a relatively large labor demand in private sectors in Karif season. In contract, in Rabi and Summer season, when labor demand is originally low, NREGS work does not result in competition with private market.

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 Literature Review

This paper is related to the empirical 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 program in agricultural labor market (e.g. Berg et al., 2014; Imbert and Papp, 2015), although some other studies find zero or marginal earnings effect (e.g. Zimmermann, 2012). The most cited one is by Imbert and Papp (2015). They focus on the effect of NREGS program on labor market equilibrium in terms of earnings and employment.

Our paper is related to this wage effect, but essentially asks a different question. We want to examine the role of wage bargaining between employers and wage labor in deciding final wages. In order to do that, we need to tease out any equilibrium effect in labor markets. Put in another way, equilibrium effect mainly arises from NREGS participants shifting from private to

public works program, while bargaining effect arises from non-NREGS-participants bargaining in private labor markets.

The second aspect of difference lies in the data. Most above mentioned studies use repeated cross-sectional NSSO employment data. Sample years are 2004-05, 2007-08. We use household survey panel in 2005-06 and 2007-08, which allow us to control for individual level time-invariant unobservables. As Imbert and Papp (2015) assert, in their paper, the relevant level of analysis is at district level, and the reason they use individual level wages is to tease out the effect of population composition change. Therefore, not controlling for individual fixed effect probably does no harm. However, the limitation of repeated cross-sectional data makes it difficult to study intra-household interactions, which none of existing studies did. Our paper adds to the literature how intra-household interactions in making work decision affect wage bargaining and hence wage levels.

Thirdly, a 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.

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 terms of identification strategy, Ravi and Engler (2015) nicely points out potential selection issue between program participant and nonparticipants, and uses propensity score matching plus dif-in-dif to address this issue. Our paper uses similar methodology.

3   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.

3.1 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 proving 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.

3.2 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 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

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[2]Authors' calculation based on our sample
[3]Authors' calculation based on our sample

Note: Agricultural seasons are indicated between vertical lines.
Kharif season=June-Oct; Rabi season=Nov-Feb; Summer season= March-May
The author calculates total number of worker-days by month since the introduction of NREGS.
Data is downloaded from muster records in NREGS website.
The sample is restricted to 3 districts that our survey covers in AP rather than all all districts.
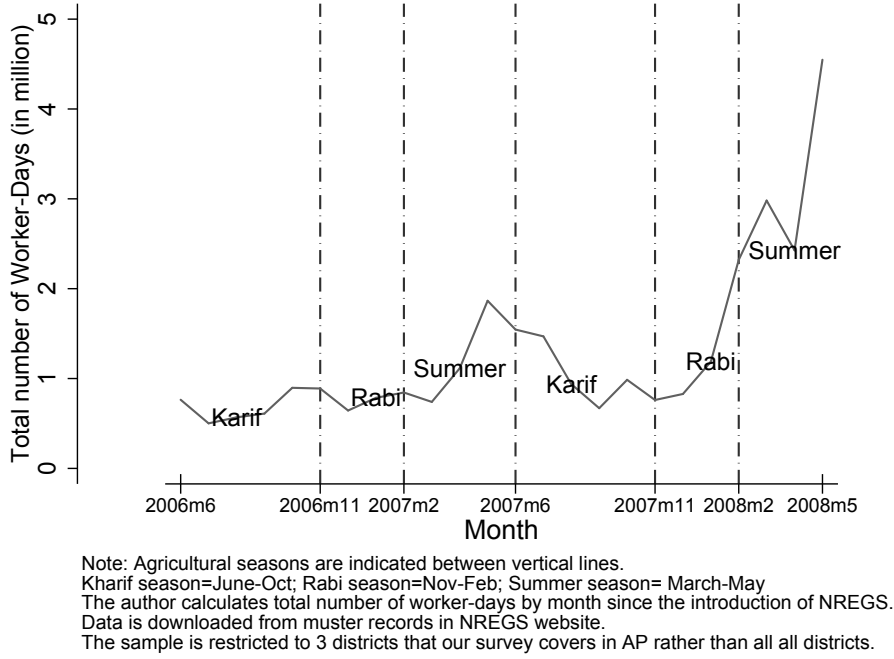
Figure 1: Seasonality of NREGS works, 2006.6-2008.5

and less in agricultural busy season. This pattern in our data is consistent with existing studies (e.g. Maiorano, 2014; Imbert and Papp, 2015).

4   Modeling and Hypothesis

Assuming 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]

5   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.

5.1 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. Karif 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 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

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	Karif	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	Karif	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					

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 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.

5.2 Descriptives

[To be added]

6 Empirical Model and Identification

We use matching method to estimate the effect of having at least one person participating in NREGS on other members' wage and employment effect, as in Ravi and Engler (2015). DID matching estimator entails a comparison of the change in labor market outcome of non-participants from participating households to that of workers from non-participating households. This comparison is conditional on household and individual characteristics $X=\{$caste, household poverty type, relighion, age, sex, marriage, reading and writing ability$\}$.

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 at least one person participating in NREGS program, and the second type "non-participating households" of the public program where nobody participating in the program. Define a treatment indicator D_i as follows: $D_i = 1$ if individual i comes from a participating household and individual i itself

is not working in the program; and $D_i = 0$ if individual i comes from a household that has *never* had anyone participating in NREGS program. Sample is restricted to non-NREGS participants who have worked positive days in the season in question. Both treatment and control households are from NREGS-available villages to get rid of general equilibrium effects.

We are interested in the change of non-participants' agricultural casual labor wage following some members participating in the program in season s, i.e.

$$Y_t(1) - Y_t(0) \tag{1}$$

Our analysis uses a DID matching estimator that requires the following identifying assumption:

$$E[Y_t(0) - Y_0(0)|P(X), D = 1] = E[Y_t(0) - Y_0(0)|P(X), D = 0] \ \text{ for } \ t \geq 1, \tag{2}$$

where $P(X)$ denotes the propensity score, i.e., $P(X) = Pr(D = 1|X)$. Given (2), and further assuming $0 < P(X) < 1$, the following estimator can be obtained:

$$ATT_{DID-Matched} = \frac{1}{N_T} \sum_{\substack{i=1 \\ [D_i=1]}}^{N} \left(\Delta Y_{i,t}(1) - \widehat{E}\Delta[Y_{i,t}(0) \, | P(X_i), D_i = 0] \right), \tag{3}$$

where $\Delta Q_t \equiv Q_t - Q_0$. We estimate the matched outcome using the average of the outcomes of the x "nearest neighbours". Mathematically:

$$\widehat{E}[\Delta Y_{i,t}(0) \, | P(X_i), D_i = 0] = \frac{1}{x} \sum_{j \in A_x} \Delta Y_{j,t}(0), \tag{4}$$

where A_x is the set of control observations with the lowest values of $|P(X_i) - P(X_j)|$. Our implementation uses $x = 20$.[4]

In robust analysis, because NREGS participation at household level also varies by total number of days of work (out of the maximum 100), we utilize this variation by replacing binary treatment variable D_{it} with a continuous treatment.

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

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[4]Any ties are broken randomly.

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 two periods of data prior to treatment, we can examine the pre-treatment common trend assumption by doing a placebo test.

7 Results

7.1 Main results

This section provides estimates of Average Treatment Effect on the Treated households (ATT) of NREGS program. Using 2006 and 2008 survey data, we estimate the effects that NREGS program has on participating households.

Assuming 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. This section empirically tests this effect and estimates its magnitude.

To get rid of general equilibrium effects, we restrict both treatment and control households to be from NREGS-available villages. The sample is restricted to non-NREGS participants who have worked positive days in the season in question.

In table 2, Column 1 and 2 report the estimated effect of NREGS participation on agricultural casual wage for workers from participating households compared to those from nonparticipating households. It shows a 5% wage increase for both male and female agricultural casual labor in private labor market. The effect only appears in Karif season, but not in Rabi or Summer season. The reasons for that is two folds. First, figure 1 shows that NREGS work is more concentrated in June-October, or Karif season. Second, Karif season is a busy ag season in itself, with relatively more demand for agricultural casual labor than the other two seasons. Therefore, the introduction of public program induces a competition for labor in that season, hence raising agricultural casual labor wage.

Employment effect is not statistically significant. The outcome variable in Column 2 and 4 is agricultural labor working days for those receiving a positive wage in both 2006 and 2008. Column 3 and 6 is agricultural labor working days for the whole sample, i.e. including extensive as well as intensive margin. We don't get any employment effect.

Table 2: Effects on Agricultural wage and employment for participating households, ATT

	Male			Female		
	Ag wage	Ag days	Ag days (all)	Ag wage	Ag days	Ag days (all)
Karif season						
ATT	0.060	-1.717	-0.365	0.060	1.011	-3.558
s.e.	0.033	5.260	2.851	0.034	4.366	3.270
p-val	0.069	0.744	0.898	0.078	0.817	0.277
N treated	77	77	319	93	93	222
N untreated	603	603	2159	965	965	2305
Rabi season						
ATT	-0.012	-0.968	-0.500	0.002	0.526	-1.896
s.e.	0.039	4.370	2.474	0.039	3.671	2.320
p-val	0.765	0.825	0.840	0.964	0.886	0.414
N treated	72	72	260	69	69	172
N untreated	638	638	2341	1062	1062	2493
Summer season						
ATT	0.017	-3.285	-1.763	0.004	-4.258	-2.000
s.e.	0.049	3.133	1.718	0.043	4.432	1.648
p-val	0.731	0.295	0.305	0.925	0.337	0.225
N treated	67	67	345	56	56	246
N untreated	511	511	2499	705	705	2762

Notes: Estimates are derived using propensity score matching and dif-in-dif method. Sample is restricted to non-NREGS workers. Treatment individuals are from participating households and control individuals are from non-participating households. Both of these two groups are from NREGS-available villages. The outcome variable in Column 2 and 4 is agricultural labor working days for those receiving a positive wage in both 2006 and 2008. Column 3 and 6 is agricultural labor working days for the whole sample, i.e. including extensive as well as intensive margin.

7.2 Definition of treated households

We test if the estimates presented in Table 2 rely on the definition of treated households. In the main definition, as long as one household member participates in the program for a positive number of days and receive a positive amount of money, then their households are counted as treated households. However, in our context of wage bargaining story, a tiny amount of monetary benefit from the program may not be helpful enough to raise reservation wage. Therefore, I redefine treated households as, having at least one household member work in the program and receive money greater than 300 (or 200) Rupees. We still obtain similar estimations as in table 2, in terms of the direction and magnitude of the effect. Results are available upon request.

7.3 Placebo test

Our identification of ATT effect relies on the common trend assumption, i.e. nonparticipants from NREGS-participating households and individuals from a non-NREGS-participating households have the same wage growth pattern. To test this, I use 2004 and 2006 data to do placebo tests. Estimates are obtained using the same specifications and same sample, except that assuming

Table 3: Placebo test – Effects on Ag wage and working days, ATT

	Male			Female		
	Ag wage	Ag days	Ag days (all)	Ag wage	Ag days	Ag days (all)
Karif season						
ATT	0.017	4.693	-1.031	0.023	0.303	1.594
s.e.	0.035	5.388	2.755	0.032	5.261	2.991
p-val	0.625	0.384	0.708	0.484	0.954	0.594
N treated	80	80	301	87	87	186
N untreated	560	560	2024	868	868	2130
Rabi season						
ATT	-0.050	-4.924	-1.010	-0.054	-4.402	-3.773
s.e.	0.047	5.537	2.946	0.034	5.973	3.960
p-val	0.291	0.374	0.732	0.110	0.461	0.341
N treated	64	64	240	66	66	137
N untreated	632	632	2198	965	965	2294
Summer season						
ATT	0.014	0.265	0.452	0.031	-3.182	-2.447
s.e.	0.044	3.875	1.611	0.039	4.554	2.066
p-val	0.741	0.946	0.779	0.429	0.485	0.236
N treated	71	71	316	57	57	193
N untreated	579	579	2305	780	780	2490

Notes: Estimates are derived using propensity score matching and dif-in-dif method. Sample is restricted to non-NREGS workers. Treatment individuals are from participating households and control individuals are from non-participating households. Both of these two groups are from NREGS-available villages. The outcome variable in Column 2 and 4 is agricultural labor working days for those receiving a positive wage in both 2004 and 2006. Column 3 and 6 is agricultural labor working days for the whole sample, i.e. including extensive as well as intensive margin.

treatment was between 2004 and 2006 agricultural year. Results are provided in Table 3. The previously obtained positive wage effect in main results goes away. None of the estimates is statistically significant. Thus, we can not reject the common trend assumption.

8   Conclusion and discussion

With rich individual participation information of NREGS program, this paper estimates labor market effects of NREGS participation for participating households. Our research question is, does having some household members working in public work program increase other household members' wage bargaining power in private sectors (mostly as agricultural labor)?

To answer this question, we use DID matching method to estimate NREGS's effect on participating households' labor market outcomes, i.e. average treatment effect on the treated households. Results show that non-participants from participating households (i.e. households with at least one person participating in the program) receive about 5% higher wage compared to individuals from non-participating households. This result is consistent with a unitary house-

hold utility model and wage bargaining story. Intuitively, when a household participates in the program, the benefit obtained from this program may transmit from participants to household non-participants, hence leading to a higher reservation wage for those the latter. This wage effect only exists in Karif season, an agricultural busy season.

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.

In addition, by defining the start of NREGS program in a village by whether anyone has really worked on it, we acknowdege that we ignored announcement effect.

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