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# Can Public Employment Schemes Increase Equilibrium Wages?
# Evidence from a Natural Experiment in India

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**Abstract**

We estimate the impact of the Indian government's major rural public works programme, the National Rural Employment Guarantee (NREG), on agricultural wages. The rollout of NREG in three phases is used to identify difference-in-difference estimates of the programme effect. Using monthly wage data from the period 2000-2011 for a panel of 209 districts across 18 Indian states, we find that on average NREG boosts the growth rate of real daily agricultural wages 4.8 per cent per year. The effect is concentrated in some states and in the agricultural season. The effect appears to be gender-neutral and biased towards unskilled labour. We argue that rural public employment programmes constitute a potentially important anti-poverty policy tool.

**Key words**    public works; workfare; agricultural wages; NREG; India

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# 1. Introduction

Seventy per cent of the world's 1.4 billion extremely poor live in rural areas (IFAD 2011). Some of them till their own land, but at the bottom of the pyramid are landless labourers who subsist on casual agricultural wage labour (ILO 1996). Direct transfers aside, policies that can put upward pressure on agricultural wages are therefore likely to be one of the most effective ways of improving welfare of the poorest people on the planet.[3]

Enforcing minimum wage legislation is unrealistic in most developing countries, but public employment programmes constitute a positive shift in the labour demand curve. It has long been hypothesised that this demand shift may, if large enough, push wages up through a general equilibrium effect. If so, the welfare effects of public employment schemes could reach well beyond the people who are directly employed by them.[4]

This paper looks at a large-scale public employment programme—the Indian government's National Rural Employment Guarantee (NREG)—and analyses its impact on agricultural wages. Using a decade's worth of monthly data on agricultural wages for a panel of over 200 Indian districts, we find that, on average, NREG has boosted the real daily agricultural wage rate in India by 4.8 per cent per year. While the impact on the growth of agricultural wages may not continue indefinitely, we do not find evidence that it has diminished by the end of our time series.

The idea of tying welfare benefits to work requirements goes back at least to pre-revolutionary France, where the poor could receive alms in return for work at 'charity workshops'. The English Poor Law of 1834 required the poor to live in 'workhouses' in order to receive welfare (Himmelfarb 1984). British colonial administrators in India frequently used public works to deliver famine relief (Drèze 1990). Katz (1996) discusses work requirements to access welfare in the United States in the 19th and early 20th century. Adolf Hitler used public works to alleviate inter-war unemployment and build inter-city highways in Germany, many of which are still in use. The term

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[3] A large literature confirms the negative association between agricultural wages and poverty rates, particularly in India (see Kijima and Lanjouw, 2005 and Eswaran *et al*., 2009). Eswaran *et al*. (2009) also links agricultural wages and poverty to sectoral labour flows. Lanjouw and Murgai (2009) find that poverty estimates correlate well with changes in agricultural wage rates in India.

[4] However, this is not a Pareto improvement: an increase in wage would have a negative welfare effect on employers. But given that agricultural employers are almost invariably better off than the landless, and also far fewer in number, this may be an acceptable trade-off.

'workfare' was coined in the United States in the late 1960s to describe the idea of social benefits tied to work requirements. Subbarao (2003) provides an overview of contemporary public employment programmes in Asia and Africa.

In spite of the long history of public employment programmes, academics and policymakers continue to debate their effect on the poor and on wider society. Theory suggests[5] that public works have three potential effects on welfare: a *direct effect* on those employed in the works; a *labour market effect* related to the shift in labour demand; and a *productivity effect* related to the investment in public goods produced under the scheme. The labour market effect would include, but need not be limited to, an increase in wages.[6] Moreover, the effect on wages could be muted if the employment programme is operational mainly during off seasons when the overall demand for labour is low, or if the scheme mainly employs workers who would otherwise be unemployed. It is also conceivable that the public goods created under the scheme could have a direct effect on welfare in addition to the possible increase in labour productivity.

Note that a public works programme can have a positive impact on wages through the productivity channel—if the improved public goods make unskilled labour more productive—as well as through the labour market channel. With our data we are unable to distinguish empirically between the labour market and productivity mechanisms, but our general impression of the reality of NREG is aligned with that of the World Bank (2011), who write that 'the objective of asset creation runs a very distant second to the primary objective of employment generation.'

For several reasons, India and NREG provide a good context in which to study the impact of public employment programmes on wages. First, NREG is an enormous programme by any standard and is therefore of considerable interest in itself. In the financial year 2010–11, it generated 2.57 *billion* person-days of employment. Evaluations of small pilot schemes are often criticised on the basis that the observed effects may not be scalable. That critique certainly does not apply here, and any lessons learned should be of broad interest. Second, empirical studies of the wage effects of public works programmes are rare in part because of a scarcity of reliable wage data. The availability of good wage data at a disaggregated regional and temporal level is a great advantage

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[5] See Ravallion (1990) for a theoretical discussion on the effects of public works on welfare.

[6] NREG can benefit the poor through efficiency gains in an agricultural labour market characterized by distortions such as monopsony power of the employers (Basu *et al*., 2009) or labour-tying (Basu, 2013).

of studying the Indian context. Third, the scheme was introduced in 2006 and extended to all of India (except urban and disputed areas) in 2008 in three distinct phases. The phased rollout allows us to use difference-in-differences estimation as our identification strategy. In other words, the districts in which NREG was already present, or not yet present, provide information on contemporaneous non-NREG wage increases, so that the estimated effect due to NREG is net of other trends. Fourth, India is a large and diverse country. The federal structure provides ample empirical variation, while also making internal validity easier to defend than for cross-country studies.

The paper makes the following contributions. First, it estimates the effect on an employment programme on wages in a developing-country context. There are several theoretical studies analysing these issues, but empirical tests of these theories in a developing country context are rare. Second, our dataset allows us to test for heterogeneous effects across regions, seasons, skill category and gender.

Our main findings are as follows: NREG has increased the growth rate of agricultural wages by 4.8 per cent per year on average since its introduction. The effect does not appear to have diminished by mid-2011. The effect is concentrated in states that are traditionally strong in the implementation of social programmes and in states that have previously been identified as 'star performers' in NREG implementation. The effect is concentrated in the main agricultural season, i.e. in the second half of the calendar year. The scheme mainly affects unskilled wages as opposed to skilled wages. There is no differential effect on men's and women's wages. The effect is positive and significant across districts in all three phases of implementation. A placebo test which shifts the implementation of the scheme back by three years does not find any impact.

Our paper is related to a large theoretical literature on rural public works, targeting and workfare. The theoretical contributions of Akerlof (1978) and Nichols and Zeckhauser (1982) highlight the targeting benefits of attaching work requirements to welfare.[7] Besley and Coate (1992), in contrast, emphasize the screening benefits of workfare in both developed and developing countries[8]. They argue that work requirements make it easier for the government to screen

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[7] van de Walle (1998) presents a review of the literature on targeting. Besley and Kanbur (1993) also discuss the merits of targeting in welfare projects.

[8] Besley and Coate (1992) also emphasize the deterrent argument whereby workfare might encourage poverty reducing investments by poor individuals.

individuals and assess their circumstances on a case-by-case basis. The situation is, however, more complex in developing countries as it is often difficult to judge the earning potential of welfare applicants. They present the optimal workfare programme for screening purposes and derive a sufficient condition for this to be cheaper than cash benefits. More recently, Basu *et al*. (2009) and Basu (2013) theoretically demonstrate that workfare programmes can benefit the poor through efficiency gains in an agricultural labour market characterized by distortions such as monopsony power of the employers or labour-tying.

The empirical literature on public works and agricultural wages is rather small. Ravallion *et al*. (1993) and Gaiha (1997) study the Maharashtra Employment Guarantee Scheme (EGS) which has been operational in the Indian state of Maharashtra since 1970. Ravallion *et al*. (1993) study the impact of an official wage increase in 1988 on EGS employment and conclude that there is 'little sign in these data of anything more than a slight impact of changes in the EGS wage on agricultural wages in either the short run or the long run.' Gaiha (1997) examines the impact of EGS on agricultural wages using monthly data. He finds a positive effect of EGS on agricultural wages in Maharashtra. Our study differs from these in that we look at a nationwide programme and use monthly and district level data from 19 states. Our identification strategy also differs in that we exploit the phased rollout of NREG to identify a difference-in-difference-type estimator.

Concurrently with this paper, Imbert and Papp (2012) and Azam (2012) estimate the impact of NREG on earnings and employment using NSS (National Sample Survey) employment and unemployment cross-sectional survey data. Both studies construct their data using several NSS 'thick rounds', 'thin rounds', and 'sub-rounds'.[9] Imbert and Papp find that NREG increases both employment and casual earnings, whereas Azam reports an overall positive earnings effect but also larger effect for women. Their finding of casual wage increases aligns well with our results. However, unlike Azam (2012) we do not find a statistically significant difference in the wage effect of NREG by gender. We argue that the difference may arise because we study wages, not earnings. In addition to a larger sample and more frequent (monthly) observations, our study design allows

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[9] Note that the sample size in NSS varies significantly across rounds. The quinquennial surveys (also known as 'thick' rounds) typically have large sample sizes and are more reliable. In between the thick rounds, NSS conducts 'thin rounds' with much smaller sample sizes. In between 'thin rounds' NSS conducts very small 'sub-rounds' where the sample is not stratified and potentially unreliable (NSSO, 2007). In the sub-rounds, district level sample size can be very small.

us to look for a differential effect across genders. We are also able to study the wage effect by profession, the effect across all three programme implementation phases, and heterogeneity of the wage effect by months of the year. Zimmermann (2012) also uses NSS data to look at the impact of the scheme on wages, but employs regression discontinuity rather than differences-in-difference methodology, and finds that wages increase for women but not for men.

Our study is also related to the rapidly expanding empirical literature on NREG more generally. Dutta *et al*. (2012) use 2009/10 NSS data to show that there is much unmet demand for NREG work in all states. Niehaus and Sukhtankar (2013) study the effect on corruption of a statutory increase in NREG wages. Jha *et al*. (2009) use household data from 900 households to examine the extent of elite capture in NREG in Andhra Pradesh and Rajasthan. They find that area of land owned is a negative predictor of NREG participation in Rajasthan, but the situation is reversed in Andhra Pradesh, indicating poorer targeting. They conclude that programme capture could be a factor in Andhra Pradesh. In a related paper, using household data from three Indian states (Andhra Pradesh, Maharashtra, and Rajasthan), Jha *et al*. (2011) analyse the nutritional impact of NREG wage, non-NREG income, and Public Distribution System (PDS) participation. They find that NREG affects nutritional status of households with respect to two macronutrients (calories and protein) as well as various micronutrients. Even though some of these studies empirically analyse poverty and nutritional impact of NREG at the household level, none of them deal with the impact of NREG on agricultural wages. Household data from specific states also makes the studies mentioned above difficult to generalize. In contrast, our results are nationally representative.

The remainder of the paper is organised as follows: Section 2 provides background information on NREG. Section 3 presents the data and descriptive statistics. Section 4 outlines the identification strategy, Section 5 presents our results and Section 6 concludes.

## 2. The National Rural Employment Guarantee

The National Rural Employment Guarantee (NREG) is the latest in a line of rural employment programmes implemented in India. Its objectives are as follows. First, NREG aims to provide wage employment to un- and under-employed landless agricultural labourers locally (within 5km radius of a worker's village) during the agricultural slack season. Second, it aims to produce or restore public goods. Third, it aims to promote decentralised governance, as the responsibility of

implementing the programme is assigned to the Gram Panchayats (GPs), the lowest tier of elected government.

In spite of the precursors, the passing of the National Rural Employment Guarantee Act in 2005 was widely perceived as a significant event. For the first time, the Indian government enacted a commitment to provide employment. In addition to the main objective of 'to provide enhancement of livelihood security of the households in rural areas of the country by providing at least 100 days of guaranteed wage employment to every household in unskilled manual work' (Ministry of Law and Justice, 2005), the act also created other entitlements. Applicants are entitled to an unemployment allowance if the local government fails to provide work within 15 days of the receipt of a written or oral job application. This allowance should be at least a quarter of the wage rate[10] for the first 30 days during the financial year and at least half of the wage rate after that. The government guidelines to supplement the act explain the workings of the programme in detail.[11]

Table 1 presents a brief summary of NREG operations in period 2008 to 2011, based on official statistics. Total expenditure on NREG increased significantly during this period, reaching $7.88 billion in 2011, which is approximately 0.5 per cent of the Indian GDP for that year (Ministry of Rural Development, 2011). The number of households obtaining work under NREG also increased over this period. In 2010–11 it increased to 54.95 million which is 34 per cent of all rural households in India. The number of works taken up under NREG has almost doubled over this period (see Table 1, row 7). On average, every village in India undertook eight NREG projects in 2010–11.

However, discrepancies between lawmakers' intentions and reality on the ground have been noted in several analyses. A government audit in 2008 found many discrepancies between the legal requirements of the act and the way the scheme has been implemented at the state, district and local levels (CAG 2008). Imbert and Papp (2011) compare reported labour participation in the scheme with household data, and find that only between 46 and 52 per cent of the reported employment can be confirmed using National Sample Survey data. Such measurement error is unlikely to affect our

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[10]Note that NREG wages are either a piece rate or a daily wage rate and not less than the minimum wages stipulated by the state government. The act stipulates that wages for men and women should be equal.

[11]For example, see NREG operational guidelines 2008 on the website http://nrega.nic.in/Nrega_guidelinesEng.pdf.

estimates as we are using 'binary treatment' and 'NREG exposure in months' as our key measures which are independent of actual or reported labour participation.

## 3. Data

The main data used here are taken from the Agricultural Wages in India (AWI) series. The series was initiated in 1951, but only data for the period 2000–2011 are used here. AWI is published by the Ministry of Agriculture and provides daily wage rates for a number of categories of rural labour. For most categories there are separate series for men and women. AWI covers three main categories of unskilled labour: 'field labour', 'other labour' and 'herding'. The most important category, field labour, is for several states further disaggregated into four tasks: ploughing, sowing, weeding and reaping. In contrast, 'other labour' is not further disaggregated. The third category of unskilled labour as mentioned above is 'herding', the data quality for which is patchy with the units of measurement often unclear. We believe that a large proportion of agricultural wage labour undertaken in India would fall under the field labour category which we use here.

Wage rates are also provided for three categories of skilled labour: carpenters, blacksmiths, and cobblers. As workers in these categories in India are nearly always men, no female wage data are provided for skilled labour.

The AWI series is the most widely used source for time series analysis of agricultural wages in India (Himanshu, 2005). The World Bank's state-level data set, developed by Özler *et al.* (1996), draws on the AWI district-level data and aggregates them using National Sample Survey (NSS) weights to arrive at state level numbers. Ravallion *et al.* (1993) also use AWI data.

The AWI data is collected by local officials in each district. They collect information from a sample of agricultural workers from a nominated centre or village in each district and report the average. For our purposes it has several advantages. First, it is unique in providing wage rates by month and district. Second, wages are reported by gender, which allows us to test the effect of NREG on the gender wage gap. Third, it reports agricultural wages in several categories which allows us to distinguish between the effects of NREG on wages of skilled versus unskilled labour.

The AWI series has been criticised for its 'black-box' data collection methodology. Himanshu (2005) argues that the officials in charge of collecting data from each district are not given detailed instructions on how to do so, and in most cases the reported wages are not based on

representative survey data. The data are also collected from one specific 'centre' in each district, which may or may not be representative of the district as a whole. Still, we believe that for our purposes the AWI is a good source of data, primarily because any weaknesses associated with the collection methodology is unlikely to be correlated with the rollout of NREG across India.

Moreover, the AWI series has several advantages over the alternative NSS (National Sample Survey) data. First, NSS employment surveys report earnings from casual labour, but not agricultural wages which we study here. Second, NSS does not provide monthly data by district. Third, NSS do not report earnings by activity and therefore cannot be used to analyse the differential impact of NREG on skilled and unskilled wages. Fourth, the AWI data does not suffer from the data quality issues associated with creating a wage series using NSS thin and thick rounds which we described in footnote 9.

All wage data are deflated to constant January 2000 prices using the Consumer Price Index for Rural Labourers published by the Indian Bureau of Labour. The results are therefore to be interpreted as effects on real wages. The Consumer Price Index for Rural Labourers is available by month and state.

Our main interest is in wages for field labour. In AWI, these are reported by gender and in most cases by task (ploughing, sowing, weeding and reaping). We construct an overall field labour wage series as follows. First, for each district and month, and separately for men and women, we compute a simple average across the tasks. Missing values are ignored;[12] that is, for each district and month, we take the average of wages for those tasks that are reported. Three states (Andhra Pradesh, Karnataka, and Maharashtra) only report wages for field labour in general as opposed to by task.

Second, we take the simple average of the resulting series for men and women. Where only men's or only women's wages are reported, we set the average to be missing.

A binary NREG treatment variable is constructed based on the phased rollout of NREG. The variable takes the value 1 if NREG is active in a particular district at a particular month and 0 otherwise. Hence, all district series start with zero and end with one after a single switch determined

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[12] If missing variables are correlated with programme roll-out then our estimates would be biased. We check this by constructing a binary variable equal to 1 if the constructed main field labour variable is missing for a district and month. This variable is regressed on the treatment binary variable as well as on the NREG exposure, along with time and district fixed effects. We find that missing variables are not correlated with programme rollout or NREG exposure.

by the NREG start date in the district's phase.

Figure 1 illustrates the phase-wise rollout of NREG in India. Phase I districts are shown in yellow, phase II districts in orange and phase III districts in brown. Phase III districts appear to dominate in the northern, western and southern parts of the countries, while phase I districts are more commonly found in central and eastern parts. Nevertheless, each region of India and all the major states have some districts in each phase.

Figure 2 also plots the average agricultural reals wages by phase. We notice that the average wage in phase I districts are the lowest and average wage in phase III districts are significantly higher relative to phases I and II.

We also construct an exposure variable which is a simple count of the number of months that NREG has been active in a district. Hence, for a given district, exposure is zero until the scheme is introduced. Exposure is set to one in the month NREG was introduced, and increases by one for each month thereafter.

In order to address concerns that AWI simply report NREG statutory wages rather than market wages after the introduction of the scheme, wages for 'field labour' (our main measure of wages) are compared to NREG statutory wages in January–March 2009, an arbitrarily chosen time after the introduction in the scheme in all districts. Only seven districts reported market wages equal to NREG statutory wages. These are not necessarily misreported, since it is not unlikely that the market wage matched the NREG statutory wage in a small number of districts. Still, these districts are dropped from the sample in order to err on the side of caution. Districts in which the location of the AWI data collection centre changed over the period are also dropped.

Table 2 presents descriptive statistics for the remaining 295 districts in 20 states over the period May 2000 to June 2011. The names of these states and districts are provided in Appendix 2. Appendix 1 presents the data appendix with variable names and definitions. The sample is fairly representative as it covers all major states and some smaller states. Even though the numbers of observations in our regressions vary due to missing values, our preferred specification in Table 3, column 4, uses 15,272 observations from 209 districts in 18 states. This is by far the largest dataset that has been used to analyse the impact of public works programmes on wages in a developing country.

# 4. Empirical Strategy

A simple way to estimate the effect of NREG on wages would be to compare wages before and after the rollout in each district (simple differences). However, if the rollout happened simultaneously in all districts, it would not be possible to disentangle the effect of NREG from an unrelated, simultaneous increase in wage rates across all districts. The phased rollout of the programme allows us to use the districts in which NREG was already present, or not yet present, as a control for wage increases not related to the introduction of the scheme (difference-in-difference estimates).

In order to take account of permanent differences in wage levels across districts, district fixed effects are included in most specifications. Year fixed effects control for national macro-trends and shocks affecting all districts. Twelve monthly fixed effects are used to take account of seasonal patterns in agricultural wages. Furthermore, district-wise linear time trends allow for a different underlying wage growth rate in each district.

The basic specification takes the form:

$$y_{it} = \alpha + \theta T_{it} + \varepsilon_{it} \tag{1}$$

where $y_{it}$ is the natural logarithm of real daily wages in district $i$ in month $t$, $\alpha$ and $\theta$ are constants, $T_{it}$ is the NREG treatment variable for district $i$ in month $t$ and $\varepsilon_{it}$ is an error term which may be serially correlated. The variable $T_{it}$ will be either a binary variable indicating whether or not NREG had been introduced in district $i$ by time $t$, or a non-negative integer counting how many months NREG had been in operation in district $i$ by time $t$. The coefficient of interest, $\theta$, represents the impact of NREG on agricultural wages, interpreted either as a one-off jump in wages at the time of introduction or as a break in the wage growth trend.

The basic specification will be augmented by district, year and month fixed effects as well as by district-specific time trends. Every regression incorporates robust standard errors, clustered at the district level, in order to take account of serial correlation in the error term.

Selection is a potential source of bias. Districts were not randomly allocated to the three phases of the NREG rollout. In fact, it was an explicit aim that poorer districts should receive the scheme first. But if the difference in wages across districts in the three phases could, in the absence of NREG, be wholly accounted for by an additive constant term (i.e., a different starting point but an identical development path thereafter), then such differences would be captured by the district

fixed effects. The possibility that districts did not just have different starting points but were also on different wage growth paths is addressed by introducing district-specific trends as controls. We also run a placebo test to check that the estimates are not driven by selection bias.

# 5. Results

## 5.1 Main results: The impact of NREG on wages for unskilled field labour

Table 3 presents our main results: the effect of NREG on real daily wage rates for unskilled field labour. Our dependent variable here and throughout is the logarithm of daily wage rates measured in January 2000 prices. In column 1, field labour wages are regressed on the binary NREG treatment variable. The coefficient is significant and indicates that wage rates in NREG-treated districts and months are approximately 7 per cent higher than in districts and months for which NREG was not present. However, this regression does not include fixed effects for district, year and month. When these are included in column 2, the effect of NREG is no longer significantly different from zero. This indicates that the introduction of NREG was not associated with an immediate jump in wages.[13]

In column 3, wages are regressed on exposure to NREG in months. Fixed effects for district, year and month are still included. The coefficient of interest is positive at 0.003 and highly significant. In column 4, district-specific time trends are included in addition. This would take account of any bias relating to wage growth rates that vary across districts independently of NREG. The coefficient increases to 0.004 and remains highly significant. This is our preferred specification, and the interpretation is that on average, the introduction of NREG in a district increases the growth rate of agricultural wages by 0.4 per cent per month. Therefore this can be interpreted as an average $0.4 \times 12 = 4.8$ per cent annual increase. That is, NREG causes a positive and significant increase in the growth rate of agricultural wages of, on average, 4.8 per cent per year on top of any underlying trends.

While it is possible that NREG causes a permanent increase in wage growth, it is perhaps more plausible that the effect diminishes over time. This would be the case if the introduction of

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[13] This is in contrast to Imbert and Papp (2012) and Azam (2012) who report a positive and significant effect of binary NREG treatment on wages. However, both these papers use data with a lower frequency of observation than we do, so their results cannot be interpreted as an immediate effect. Hence both their results and ours are consistent with an effect of NREG on wages that is gradual or delayed rather than immediate.

NREG represented an immediate and permanent shock which leads to the gradual adjustment to a new labour market equilibrium. 'Teething problems' or a gradual ramping up of the scheme in each district could also explain an initial growth spurt which slows down as the scheme stabilises.

To look for a diminishing effect, a squared exposure term is introduced in the regression in column 5. If the effect of NREG diminishes over time, a negative and significant squared-term coefficient would be expected. However, our findings are contrary to expectations. While the linear exposure term remains positive and significant, the coefficient on the squared term is close to zero and not statistically significant. Our interpretation of this is that either the effect of NREG on wage growth is permanent or, perhaps more plausibly, we do not have wage data for sufficiently long after the introduction of the scheme to be able to identify the diminution.

How does the magnitude of our estimate compare to those of other studies? Ravallion *et al.* (1993) and Gaiha (1997) studied the impact of the Maharashtra Employment Guarantee Scheme (EGS) on agricultural wages. The Maharashtra EGS has been in operation since 1978 and is by many considered to be a direct precursor of NREG. Gaiha (1997) reports a long run effect of EGS on wages of 18 per cent and a short run effect of 10 per cent. Ravallion *et al.* (1993) find little evidence of an increase in EGS statutory wages passing through to agricultural wages either in the long or the short run. Imbert and Papp (2012) look at the impact of binary NREG treatment on casual wages, but use NSS rather than AWI data. They report that the impact is 4.5 per cent on average. However, it should be noted that they report impact on wage income whereas we look at wage rates.

## 5.2 State-wise estimates

There is variation in the vigour with which NREG was implemented. Some states have been highlighted as 'star performers', while others have drawn criticism for the way the scheme has been handled. In order to study regional heterogeneity, the main regression in column 4, table 3 is also estimated state by state. In all these regressions, the dependent variable is field labour wages, and fixed effects for year, month and district as well as district-wise time trends are included.

When wages are regressed on the binary NREG variable, the coefficient is not significant at the 5% level for any state (not reported). This corresponds with our finding in Table 3, column 2 that the binary treatment variable is not associated with an overall effect.

When wages are regressed on the exposure variable, the results are as reported in Table 3. There are positive and significant effects for Rajasthan and Andhra Pradesh, two states that have

been highlighted among the top performers in Drèze and Khera (2009). The effects are also positive and significant for Kerala and West Bengal, two states that tend to do well on many social metrics. In addition, there are positive and marginally significant (at the 10% level) effects in Bihar and Haryana. For all other 13 states in the data set, the effect is not statistically significant. Figure 3, plots the coefficient estimates with their 95 per cent confidence intervals for the 15 major states.

## 5.3 Seasonal variation

In order to study whether the effect of NREG is uniform throughout the year, wage rates are regressed on the exposure variable interacted with 12 monthly fixed effects. As before, the regression includes fixed effects for district, year and month along with district-wise time trends. The standard errors are robust and clustered at the district level.

The results are presented in Figure 2. The dots represent the point estimates and the bars, 95% confidence intervals. None of the coefficients associated with the first half of the year (January–June) are significantly different from zero at the 5% level, while all the coefficients for the second half of the year (July–December) are positive and significant. Note, however, that the estimates for the different months are not statistically significantly different from each other.

These findings align well with the fact that July–December corresponds to the main agricultural season in India, while January–June is a relatively 'slack' season. Hence, it appears that NREG exposure pushes up wages only when labour is relatively scarce, which seems reasonable.

However, our results may appear to contrast with the findings of Imbert and Papp (2012), who find that the earnings effect of NREG are concentrated in the first half of the year. They argue that their finding is reasonable if most NREG project work is concentrated in the agricultural slack season, but they do not consider the labour scarcity argument. A possible explanation for the difference between the findings is that we study wages while they look at earnings. While it is reasonable that the programme should increase wages more when demand for labour is high, it is also plausible that the effect on *earnings* is stronger in the slack season, if workers would otherwise be un- or under-employed.

## 5.4 Impact on Skilled Labour

Table 4 presents the results of regressions of wages in skilled labour categories: carpenters, blacksmiths and cobblers on NREG exposure. As before, fixed effects for district, year and month

are included, as are district-wise time trends. NREG exposure does not appear to have an effect on the wages paid for any of these categories of labour.

The intended beneficiaries of NREG are unskilled agricultural labourers. To the extent that skilled and unskilled workers operate in separate labour markets, it is not surprising that the wages or carpenters, cobblers and blacksmiths are insulated from the effect of the programme.

How correlated are these wages to the field labour wages? In table 5, we examine the correlation and report the correlation matrix. We do not find any evidence of strong correlation between field labour wages and the wages of the skilled categories. This further corroborates our hypothesis that skilled and unskilled workers operate in separate labour markets

## 5.5 Impact on the Gender Wage Gap

NREG requires at least a third of beneficiaries to be women, and also stipulates that women's and men's wage rates should be equal. The relatively larger increase in the demand for female labour from NREG projects might result in a larger impact on women's wages than on those of men. Since women are normally paid less than men, equal wages under NREG might also suggest a larger upward pressure on women's wages. It is therefore of interest to compare the effect of NREG on wages across gender. In Table 6, we study the impact of NREG intensity on the agricultural gender wage gap. We consider men's and women's wages separately; hence the larger sample size. We create a binary variable which takes the value 1 for female wages and 0 for male wages. NREG's impact on the gender wage gap is tested by adding to our preferred specification an interaction term Exposure × female in column 1 and Treated × female in column 2, along with the un-interacted female binary variable. If the coefficient on the interaction term is positive and significant then arguably NREG is reducing the agricultural gender wage gap.

In column 1, the un-interacted 'female' coefficient on NREG intensity is negative and significant highlighting the gender wage gap in Indian agriculture. Female field labourers are on average paid 79 per cent of their male counterparts.[14] The exposure coefficient is positive, significant and similar in magnitude to what we reported earlier. The differential effect of NREG exposure on women's wages relative to those of men captured by the coefficient on Exposure ×

[14] If $w_f$ is female field labour wages and $w_m$ is male field labour wages then our estimates predict $\ln w_m - \ln w_f = 0.23$. Therefore, $w_f = 0.79 w_m$.

15

female variable is very small and not significant. Therefore, in percentage terms, exposure to NREG has the same positive effect on the wages of both men and women. In other words, NREG exposure neither diminishes nor enlarges the gender wage gap.

In order to verify that the lack of a differential gender effect is not driven by the use of the exposure variable, the analysis is repeated with the binary treatment variable in column 2. If the overall effect of NREG on wages is a change in trend whereas the effect on women's wages is more immediate, then the previous specification may not have picked it up. In column 2, the results mirror those found earlier and therefore no discrete jump in wages is identified.

These results may appear to contradict those of Azam (2012), who finds that the effect on women's earnings is greater than the effect on men's earnings. However, this seemingly contradictory result could easily be reconciled if one focuses on the difference between wage rates and earnings. In other words, it is reasonable that women may make a relatively larger earnings gain than men within the programme, but the differences between the male and female labour markets mean that this does not translate into a larger effect on equilibrium wages for women than for men.[15]

## 5.6 Phase-wise effects and Placebo Test

Table 7 presents the results of a phase-wise analysis. We estimate our preferred model (table 3, column 4) for all three phase independently. Columns 1-3 reports results for phase I-III respectively. The effect is positive and strongly significant for all three phase. The magnitude of the effect is greatest in phase II districts followed by phase I and III.

Finally, Table 8 presents the results of a placebo test. Recall that the first phase of NREG implementation started in February 2006, the second in April 2007, and the third in April 2008. For the placebo test, all observations from 2006 onwards are deleted, so that all remaining district/time observations are untreated. A placebo treatment indicator was defined by shifting the NREG introduction date back by three years in all districts.[16] Hence Phase I districts 'received' the placebo

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[15]To illustrate this point further, note that several descriptive studies have noted that the programme disproportionately employs women (Dreze and Khera, 2009). Therefore it is natural that women's earnings increased more than men's. However, equilibrium wage rates would only be expected to increase more for women than for men if the programme led to a more severe scarcity of female labour than of male labour. This is by no means certain as in the absence of the programme women are more likely to be un- or underemployed than men. Therefore, the upward pressure on female-specific wages would be muted.

[16] Given that the data series start in the year 2000, setting the placebo NREG-introduction dates to be three years before the actual introduction dates means that the periods of observation before and after the placebo treatment

treatment in February 2003, Phase II districts in April 2004 and Phase III districts in April 2005.

In column 1, wage rates are regressed on the placebo treatment with the district, year and month fixed effects as controls. The estimated effect of the placebo treatment is not significant. Column 2 introduces exposure rather than the binary treatment as the main independent variable, while still controlling for district, year and month fixed effects. The coefficient of interest is close to zero and insignificant. The introduction of district time trends in column 3 and a squared placebo exposure term in column 4 does not change the overall finding of no effect of the placebo treatment. The placebo test therefore corroborates our earlier results and corroborates the interpretation that the effects estimated above are indeed caused by NREG.

# 6. Concluding Remarks

This paper estimates the effect of NREG on agricultural wages in India using monthly data from 209 districts spread across 18 Indian states in the period May 2000 to June 2011. The phased rollout of NREG is exploited to obtain difference-in-difference estimates. Controlling for district and time fixed effects and district-wise time trends, the results indicate that on average NREG boosts the growth rate of real daily agricultural wages by 4.8 per cent per year. The effect of NREG appears to be gender neutral and concentrated in the main agricultural season. It also appears to be targeted well as it mainly affects unskilled wages and not skilled wages. The effect is mainly concentrated in the better performing states of Andhra Pradesh, Bihar, Haryana, Kerala, Rajasthan, and West Bengal – the so called star states. The validity of our identification strategy is corroborated with a placebo test.

It is difficult if not impossible for developing country governments to enforce statutory minimum wages. Though we have not assessed the cost-effectiveness of the Indian programme, public employment programmes provide governments with an additional mechanism with which to influence wage rates in the rural unskilled labour market. Since the link between agricultural wages and poverty rates are well established, if public works can influence agricultural wages then they constitute an attractive anti-poverty policy instrument. However, the net welfare effects are ambiguous as we do not take into account the effect of wage increase on employers and the effect

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are roughly balanced for Phase 1 districts.

of workfare on the labour supply of the poor.

# References

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# Appendices

**1. Data appendix**

*Daily Wage Rates.* All wages are real daily rates, reported here in rupees deflated to January 2000 prices using state-wise consumer price indices for rural labourers, and provided by district, month, labour category and sex. More details in section 3. *Source:* Agricultural Wages in India (AWI), Ministry of Agriculture, Government of India.

*Treated.* NREG treatment binary is equal to 1 if NREG was active in the district in that month, and

0 otherwise. *Source:* Government of India.

*Exposure.* This measures the number of months NREG has been active in a particular district. It is a non-negative integer counting how many months NREG has been active in district $i$ at month $t$. *Source:* Government of India.

Consumer Price Index. Monthly Consumer Price Index for Rural Labourers. This is used to deflate the daily wage rates. *Source:* Indian Labour Bureau.

## 2. Sample Districts and Phases

| State | District, NREG phase | State | District, NREG phase | State | District, NREG phase | State | District, NREG phase |
|---|---|---|---|---|---|---|---|
| **Andhra Pradesh** | | **Gujarat** | | **Madhya Pradesh** | | **Punjab** | |
| | Adilabad, 1 | | Ahmedabad, 3 | | Ashoknagar, 2 | | Amritsar, 2 |
| | Anantapur, 1 | | Amreli, 3 | | Balghat, 1 | | Bhatinda, 3 |
| | Chittoor, 1 | | Banaskantha, 1 | | Barwani, 1 | | Faridkot, 3 |
| | East Godavari, 2 | | Bhavanagar, 3 | | Betul, 1 | | Fatehgarh Sahib, 3 |

21

Guntur, 2

KarimNagar, 1

Khamam, 1

Krishna, 3

Kurnool, 2

Mahaboob Nagar, 1

Medak, 1

Nalgonda, 1

Nellore, 2

Nizamabad, 1

Prakasam, 2

Rangareddy, 1

Srikakulam, 2

Vijianagaram, 1

Visakhapatnam, 3

Warrangal, 1

West Godawari, 3

YSR, 1

**Assam**

Barpeta, 2

Cachar, 2

Darrang, 2

Dhubpri, 3

Dibrugarh, 3

Goalpara, 1

Golaghat, 3

Halkandi, 2

Jorhat, 3

K. Anglong, 1

Kamrup, 3

Karimganj, 3

Kokrajhar, 1

North Lakhimpur, 1

Nowgaon, 3

Sibsagar, 3

Tejpur, 3

**Bihar**

Aurangabad, 1

Banka, 2

Begusarai, 2

Bhagalpur, 2

Bojpur, 1

Dohad, 1

Gandhi Nagar, 3

Jamnagar, 3

Junagadh, 3

Kachchh, 3

Kaira, 3

Parbudar, 3

Patan, 3

Rajkot, 3

Sabarkantha, 1

Surendrananagar, 3

Surrat, 3

**Haryana**

Ambala, 2

Bhiwani, 3

Faridabad, 3

Fatehabad, 3

Gurgaon, 3

Hissar, 3

Jind, 3

Kaithal, 3

Karnal, 3

Kurkshetra, 3

Mahendragarh, 1

Panchkula, 3

Panipat, 3

Rewari, 3

Rohtak, 3

Sonipat, 3

Yamunanagar, 3

**Himachal Pradesh**

Bilaspur, 3

Chamba, 1

Hamirpur, 3

Kangra, 2

Kinnaur, 3

Kullu, 3

Mandi, 2

Shimala, 3

Sirmaur, 1

Solan, 3

**Jharkhand**

Bhind, 3

Bhopal, 3

Burhanpur, 2

Chhatarpur, 1

Chhindwara, 2

Damoh, 2

Datiya, 2

Dewas, 2

Dhar, 1

Dindhori, 1

Guna, 2

Gwalior, 3

Harda, 2

Hoshangabad, 3

Indore, 3

Jabalpur, 3

Jhabua, 1

Katni, 2

Mandla, 1

Mandsaur, 3

Morena, 3

Narsingpur, 3

Neemach, 3

Nimar Khargaon, 1

Nimarkhandva, 1

Panna, 2

Raisen, 3

Rajgarh, 2

Ratlam, 3

Sagar, 3

Satna, 1

Sehore, 3

Seoni, 1

Shahdol, 1

Shahjapur, 3

Sheopur, 1

Shivpuri, 1

Sidhi, 1

Tikamgarh, 1

Ujjain, 3

Umariya, 1

Vidisha, 3

Ferozepur, 3

Gurdaspur, 3

Hoshiarpur, 1

Jalandhar, 2

Kapurthala, 3

Ludhiana, 3

Mansa, 3

Mogha, 3

Muktasar, 3

Nawanshehar, 2

Patiala, 3

Roopnagar, 3

Sangrur, 3

**Rajasthan**

Alwar, 3

Banswara, 1

Baran, 3

Barmer, 2

Bikaner, 3

Bundi, 3

Churu, 3

Dausa, 3

Dholpur, 3

Jhalawar, 1

Jhunjhunu, 3

Jodhpur, 3

Karauli, 1

Kota, 3

Madhopur, 2

Nagaur, 3

Pali, 3

Rajasamand, 3

Sikar, 3

Sirohi, 1

Tonk, 2

**Tamil Nadu**

Cuddalore, 1

Kanniyakumari, 3

Krishnagiri, 3

Nilgiris, 3

Pudukkottai, 3

Theni, 3

Darbhanga, 1

East Champarn, 2

Gaya, 1

Jahanabad, 1

Jamui, 1

Madhubani, 1

Monghyr, 1

Muzaffarpur, 1

Nalanda, 1

Navada, 1

Purnia, 1

Rohtas, 1

Shekhpura, 2

Sheohar, 1

Sitamari, 2

Sivan, 2

Soopale, 1

Vaishali, 1

West Champaran, 2

**Chattisgarh**

Bastar, 1

Bilaspur, 1

Dantewara, 1

Dhamtari, 1

Durg, 3

Janjgirchaupa, 2

Jashpur, 1

Kabirdham, 1

Kanker, 1

Korba, 2

Koriya, 1

Mahasamund, 2

Raigarh, 1

Raipur, 2

Rajnandgaon, 1

Sarguja, 1

Bokaro, 1

Chatra, 1

Deoghar, 2

Dumka, 1

East Singhbhum, 2

Garhwa, 1

Giridih, 1

Gumla, 1

Hazaribagh, 1

Jamtara, 1

Lohardaga, 1

Pakur, 1

Palamu, 1

Ranchi, 1

Sahibganj, 1

Saraikela (Kharsanwa), 1

Simdega, 1

West Singhbhum, 1

**Karnataka**

Bangalore, 3

Bellary, 2

Hassan, 2

Kolar, 3

Mandya, 3

Mysore, 3

Shimoga, 2

Tumkur, 3

**Kerala**

Alappujha, 3

Ernakulam, 3

Idukki, 2

Kannoor, 3

Kasargod, 2

Kollam, 3

Kottayam, 3

Kozikode, 3

Malapuram, 3

Palakkad, 1

Pathamamithitta, 3

Thrissur, 3

Trivandrum, 3

**Maharashtra**

Ahmednagar, 1

Chandra Pur, 1

Jalgaon, 3

Jalna, 3

Satara, 3

Thana, 2

Wardha, 2

Yeutmal, 1

**Orissa**

Angul, 2

Baragarh, 2

Bhadrak, 2

Bolangir, 1

Boudh, 1

Cuttack, 3

Deogarh, 1

Dhenkanal, 1

Gajapati, 1

Ganjam, 1

Jaipur, 2

Jharsuguda, 1

Kalahandi, 1

Keonjhar, 1

Koraput, 1

Malkangiri, 1

Mayurbhanj, 1

Nawapara, 1

Nawarangpur, 1

Nayagarh, 3

Phulbani, 1

Puri, 3

Rayagada, 1

Sambalpur, 1

Sonepur, 1

Sundargarh, 1

kendrapara, 3

Thoothukudi, 3

Tiruchirapalli, 3

**Tripura**

Agartala, 2

**Uttar Pradesh**

Allahabad, 3

Chandauli, 1

Faizabad, 3

Jhansi, 2

Lucknow, 3

Meerut, 3

Varanasi, 3

**Uttrakhand**

Almora, 3

Bageshwar, 3

Chamoli, 1

Champawat, 1

Dehradun, 3

Haridwar, 2

Nainital, 3

Pauri Garhwal, 3

Pithauragarh, 3

Rudraprayag, 3

Tehri Garhwal, 1

Udham Singh Nagar, 2

**West Bengal**

24 Pargana (South), 1

24 Pargana North, 2

Bankura, 1

Birbhum, 1

Burdhwan, 2

CoochBehar, 2

Darjeeling, 2

Hooghli, 2

Howrah, 3

Jalpaigudi, 1

Maldah, 1

Midnapur (East), 2

Midnapur (West), 1
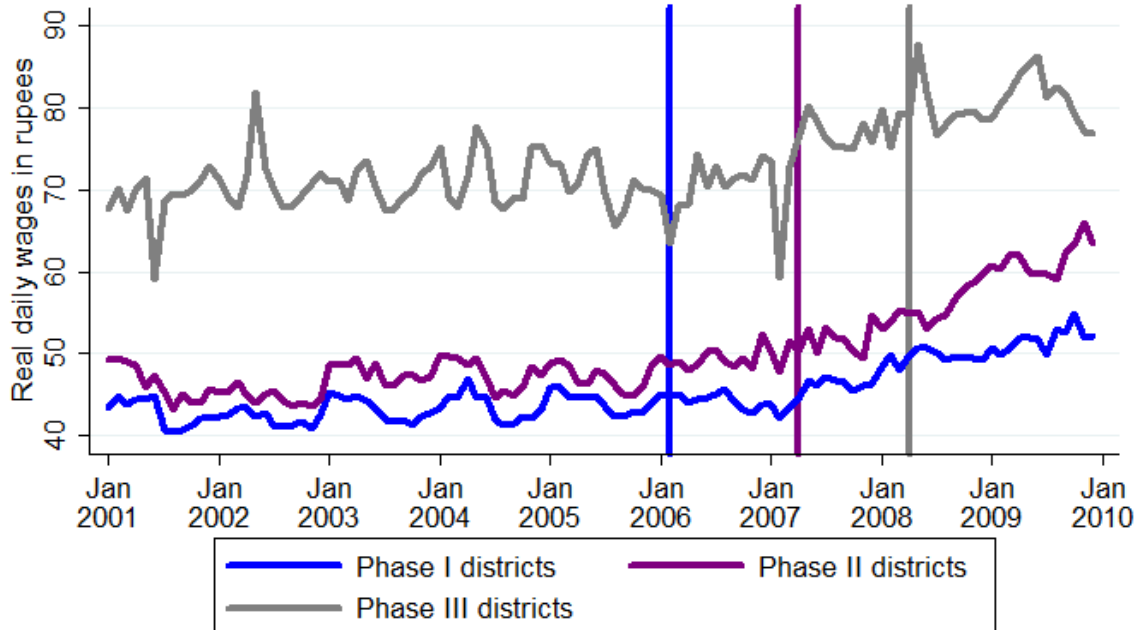
Murshidabad, 1

Nadia, 2

North Dinajpur, 1

Purulia, 1

South Dinajpur, 1

**Figure 1.** District Map of India.
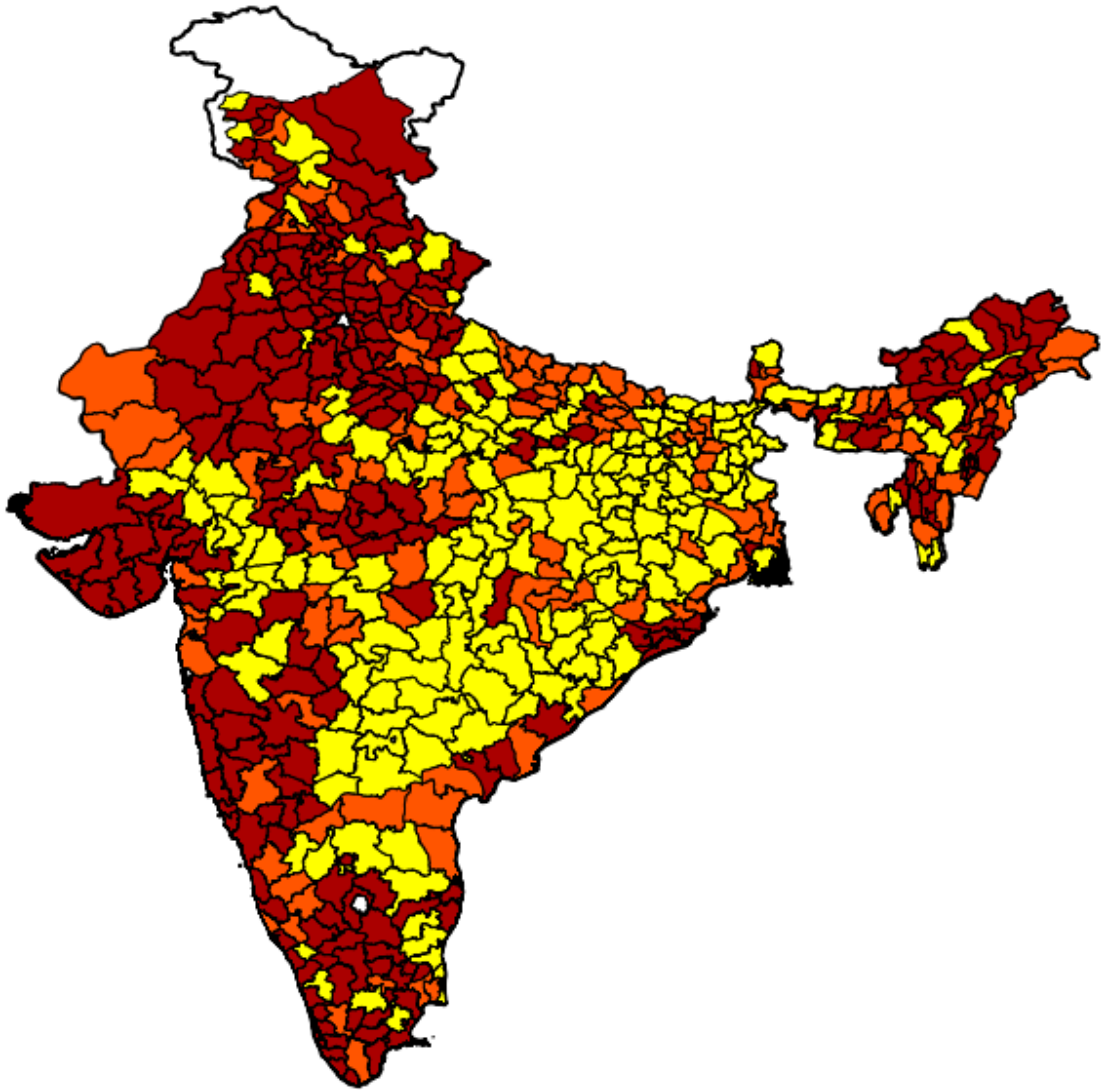
**Notes:** The map shows all rural districts of mainland India, colour-coded according to NREG implementation phase. Phase I districts are shown in yellow, Phase II districts in orange and Phase III districts in brown.

Figure 2. Real agricultural wages

Note: The figure shows real daily field labour wages in January 2000 prices. The rates are averaged across the field labour sub-categories, across men and women and across all districts in each phase. The vertical lines show the time of introduction of NREG in each phase. Monthly observations.

**Figure 3.** Effects of NREG on Wages by State.
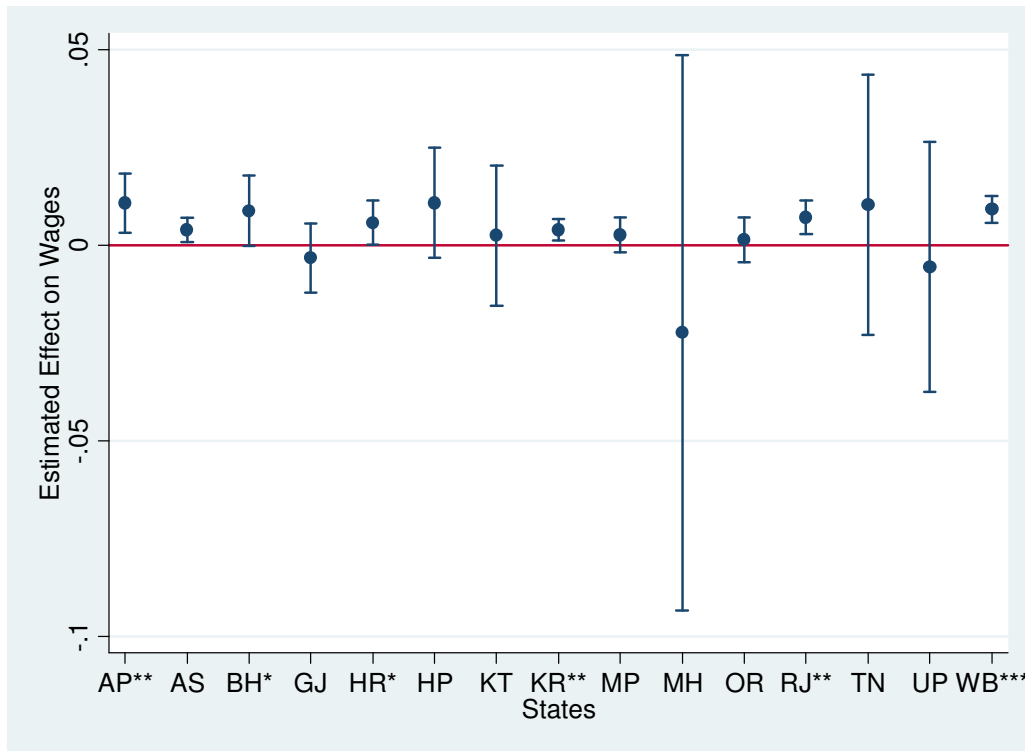


**Notes:** The figure shows the coefficients on the regression reported in table 3, column 4 carried out for different states. The dots show the point estimates, and the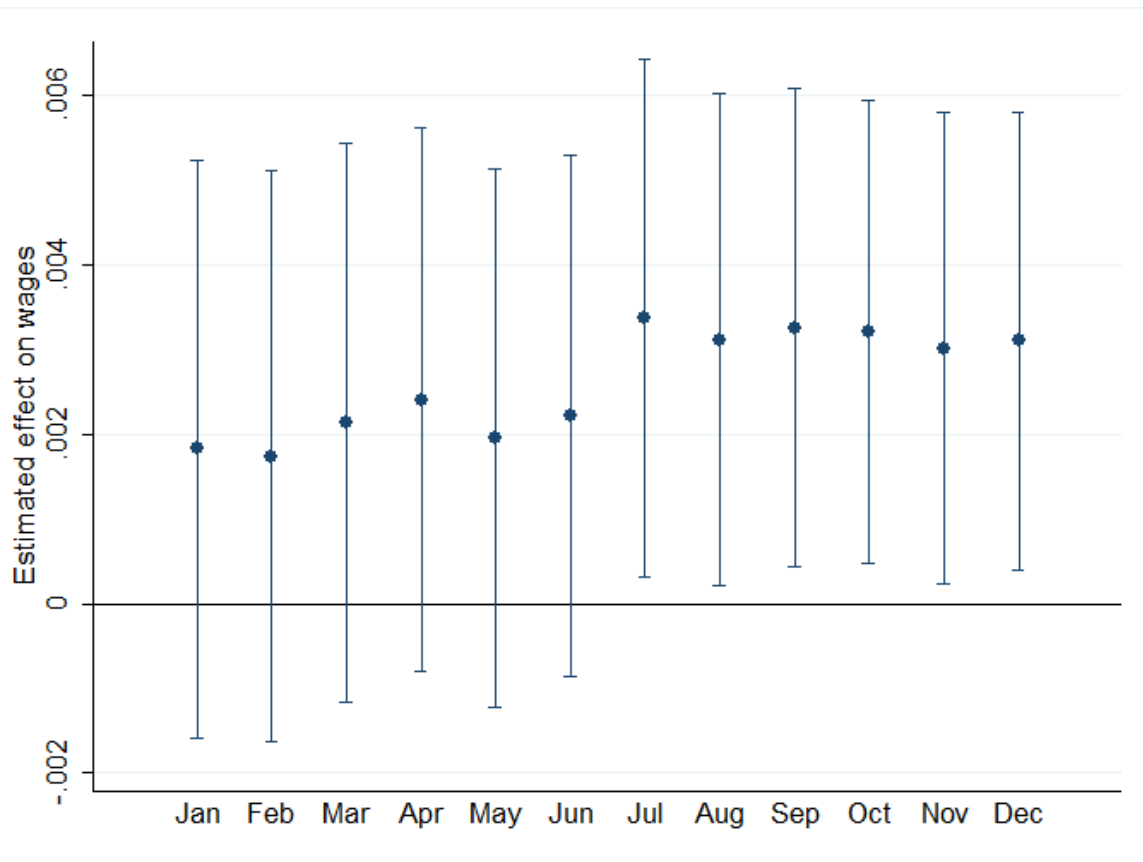 bars indicate 95% confidence intervals. The states included are Andhra Pradesh (AP), Assam (AS), Bihar (BH), Gujarat (GJ), Haryana (HR), Himachal Pradesh (HP), Karnataka (KT), Kerala (KR), Madhya Pradesh (MP), Maharashtra (MH), Orissa (OR), Rajasthan (RJ), Tamil Nadu (TN), Uttar Pradesh (UP), and West Bengal (WB). Standard errors in the regressions are robust and clustered at the district level. The stars on the horizontal axis indicate * $p<0.10$, ** $p<0.05$, *** $p<0.01$ respectively.

**Figure 4.** Effects of NREG on Wages by Month.



**Notes:** The figure shows the coefficients on a regression of field labour wages on 'Exposure' (ie., exposure to NREG in months) interacted with 12 monthly binary variables. The dots show the point estimates, and the bars indicate 95% confidence intervals. The regression controlled for district, year and monthly fixed effects and district-wise time trends, and standard errors were clustered at the district level.

**Table 1.** NREG Headline Statistics for the period 2008-2011

| | Financial year | | |
|---|---|---|---|
| | **2008–09** | **2009–10** | **2010–11** |
| Total expenditure (billions of USD) | 5.45 | 7.58 | 7.88 |
| Number of households obtaining wage work (millions) | 45.11 | 52.53 | 54.95 |
| Person-days of employment generated (billions) | 2.16 | 2.84 | 2.57 |
| Person-days of employment availed by low-caste (SC/ST) persons (%) | 54.72 | 51.2 | 51.48 |
| Person-days of employment availed by women (%) | 47.88 | 48.1 | 47.73 |
| Average person-days of employment per household that obtained NREG work | 47.95 | 53.99 | 46.79 |
| Total NREG works taken up (in million) | 2.77 | 4.62 | 5.10 |
| Estimated number of NREG works taken up per village | 4 | 7 | 8 |

*Source:* The NREG website, http://nrega.nic.in

**Table 2.** Summary Statistics.

|  | Mean | SDev | Min | Max | Obs |
|---|---|---|---|---|---|
| **Unskilled labour wages, average of men's and women's rates** | | | | | |
| Field labour | 58.36 | 28.73 | 17 | 230 | 15272 |
| **Unskilled labour, wage rates by gender** | | | | | |
| Field labour (m) | 64.57 | 33.81 | 18 | 257 | 17122 |
| - ploughing (m) | 73.31 | 48.31 | 19 | 338 | 12421 |
| - sowing (m) | 66.87 | 36.56 | 19 | 290 | 10745 |
| - weeding (m) | 54.73 | 19.89 | 16 | 167 | 8945 |
| - reaping (m) | 64.61 | 33.78 | 20 | 270 | 10348 |
| Field labour (f) | 51.17 | 24.07 | 13 | 203 | 15328 |
| - ploughing (f) | 52.29 | 15.74 | 16 | 98 | 133 |
| - sowing (f) | 45.82 | 13.15 | 16 | 135 | 5575 |
| - weeding (f) | 52.18 | 24.51 | 16 | 200 | 9695 |
| - reaping (f) | 56.90 | 30.87 | 14 | 237 | 9623 |
| **Skilled labour wages** | | | | | |
| Carpenter | 104.88 | 39.89 | 21 | 292 | 16946 |
| Blacksmith | 87.09 | 33.24 | 19 | 247 | 14331 |
| Cobbler | 71.60 | 26.15 | 15 | 224 | 10842 |

**Notes:** All wages are daily rates, reported in rupees deflated to January 2000 prices using state-wise monthly consumer price indices for agricultural labourers. The main category of unskilled labour is field labour. Field labour wages are provided by sub-category (ploughing, sowing, weeding and reaping), except for districts in Andhra Pradesh, Maharashtra and Karnataka, for which only wages for field labour (aggregate) are reported. The male and female field labour wages analysed here are constructed as follows. Where provided, the general field labour category is used. Otherwise, each observation is the simple average of the wage rates for whichever sub-categories are reported for that month. The main outcome variable studied in this paper is the simple average of men's and women's field labour wages when both are available and missing otherwise.

**Table 3.** The Effect of NREG on Real Wages

| | (1) Field labour wages | (2) Field labour wages | (3) Field labour wages | (4) Field labour wages | (5) Field labour wages |
|---|---|---|---|---|---|
| Treated | 0.073*** (0.021) | -0.003 (0.016) | | | |
| Exposure | | | 0.003*** (0.001) | 0.004*** (0.001) | 0.003*** (0.001) |
| $[Exposure]^2$ | | | | | $2 \times 10^{-5}$ $(2.7 \times 10^{-5})$ |
| District, year and month fixed effects | No | Yes | Yes | Yes | Yes |
| District trends | No | No | No | Yes | Yes |
| Observations | 15272 | 15272 | 15272 | 15272 | 15272 |
| Districts | 209 | 209 | 209 | 209 | 209 |
| States | 18 | 18 | 18 | 18 | 18 |

**Notes:** The dependent variable is log daily field labour wages in fixed January 2000 prices, observed between May 2000 and June 2011. 'Treated' is a binary variable equal to 1 if NREG was active in the district at that time, and 0 otherwise. 'Exposure' measures the number of months NREG has been active in a particular district. It is a non-negative integer counting how many months NREG has been active in district $i$ at time $t$. Standard errors, in parentheses, are robust and clustered at the district level.
\* $p<0.10$, \*\* $p<0.05$, \*\*\* $p<0.01$

**Table 4.** The Effect of NREG on Real Wages: Skilled Labour Categories

| | (1) Carpenters' wages | (2) Blacksmiths' wages | (3) Cobblers' wages |
|---|---|---|---|
| Exposure | 0.001 | 0.001 | 0.001 |
| | (0.001) | (0.002) | (0.002) |
| | | | |
| District, year and month (season) fixed effects | Yes | Yes | Yes |
| | | | |
| District trends | Yes | Yes | Yes |
| Observations | 16946 | 14331 | 10842 |
| Districts | 206 | 181 | 156 |
| States | 18 | 15 | 12 |

**Notes:** The dependent variables are log daily wages in fixed January 2000 prices, observed between April 2000 and June 2011. 'Exposure' measures the number of months NREG has been active in a particular district. It is a non-negative integer counting how many months NREG has been active in district $i$ at time $t$. Standard errors, in parentheses, are robust and clustered at the district level.

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

**Table 5.** How Correlated are the Real Wages across Skilled and Unskilled Labour Categories?

|  | Field labour | Carpenter | Blacksmith | Cobbler |
|---|---|---|---|---|
| Field labour | 1.00 |  |  |  |
| Carpenter | 0.31 | 1.00 |  |  |
| Blacksmith | 0.66 | 0.77 | 1.00 |  |
| Cobbler | 0.74 | 0.48 | 0.89 | 1.00 |

**Table 6.** NREG and the Gender Wage Gap

|  | (1) Field labour wages | (2) Field labour wages |
|---|---|---|
| Female | -0.23*** | -0.23*** |
|  | (0.012) | (0.013) |
| Exposure | 0.003*** |  |
|  | (0.001) |  |
| Exposure x female | -0.0001 |  |
|  | (0.0004) |  |
| Treated |  | 0.0002 |
|  |  | (0.015) |
| Treated x female |  | -0.004 |
|  |  | (0.011) |
| District, year and month fixed effects | Yes | Yes |
| District trends | Yes | Yes |
| Observations | 32450 | 32450 |
| Districts | 209 | 209 |
| States | 18 | 18 |

**Notes:** The dependent variables are log daily wages in fixed January 2000 prices, observed between April 2000 and June 2011. 'Treated' is a binary variable equal to 1 if NREG was active in the district at that time, and 0 otherwise. 'Exposure' measures the number of months NREG has been active in a particular district. It is a non-negative integer counting how many months NREG has been active in district $i$ at time $t$. Standard errors, in parentheses, are robust and clustered at the district level.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

**Table 7.** The Effect of NREG on Real Wages: Phase-Wise Estimates

| | (1) Field labour wages | (2) Field labour wages | (3) Field labour wages |
|---|---|---|---|
| | Phase I | Phase II | Phase III |
| Exposure | 0.006*** | 0.013*** | 0.004** |
| | (0.001) | (0.003) | (0.002) |
| District, year and month fixed effects | Yes | Yes | Yes |
| District trends | Yes | Yes | Yes |
| Observations | 6616 | 2935 | 5721 |
| Districts | 82 | 43 | 84 |
| States | 13 | 14 | 15 |

**Notes:** The dependent variables are log daily wages in fixed January 2000 prices, observed between April 2000 and June 2011. 'Exposure' measures the number of months NREG has been active in a particular district. It is a non-negative integer counting how many months NREG has been active in district $i$ at time $t$. Standard errors, in parentheses, are robust and clustered at the district level.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

**Table 8.** Placebo Test

| | (1) Field labour wages | (2) Field labour wages | (3) Field labour wages | (4) Field labour wages |
|---|---|---|---|---|
| Treated (placebo) | 0.012 (0.011) | | | |
| Exposure (placebo) | | 0.0001 (0.001) | -0.001 (0.001) | -0.001 (0.002) |
| [Exposure (placebo)]$^2$ | | | | $5 \times 10^{-6}$ ($3 \times 10^{-5}$) |
| District, year and month fixed effects | Yes | Yes | Yes | Yes |
| District trends | No | No | Yes | Yes |
| Observations | 7535 | 7535 | 7535 | 7535 |
| Districts | 176 | 176 | 176 | 176 |
| States | 16 | 16 | 16 | 16 |

**Notes:** The dependent variable is log daily field labour wages in fixed January 2000 prices, observed between May 2000 and December 2005. 'Treated (placebo)' and 'Exposure (placebo)' were generated by pushing forward the NREG introduction date by three years in each district and deleting all observations from January 2006 onwards. The coefficients reported here are associated with this placebo treatment. Standard errors, in parentheses, are robust and clustered at the district level.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$