Can Public Works Increase Equilibrium Wages? Evidence from India's National Rural Employment Guarantee[1]

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Abstract. We estimate the impact of the world's largest public works program, India's National Rural Employment Guarantee (NREG), on agricultural wages. NREG was rolled out across India in three distinct phases, and this phased introduction is used to identify difference-in-difference estimates of the program effect. Using monthly data on wage rates from the period 2000–2011 in 209 districts across 18 Indian states, we include district-specific fixed effects and trends to control for differences in wage rates in the absence of the program. We find that, on average, the program boosted the growth rate of real daily agricultural wages by 4.3% per year. The effect of the scheme is more consistent with an increase in the growth rate of wages than with a discontinuous jump in wages. The effect was concentrated in some states and in the agricultural peak season, appears to have been gender-neutral and was biased towards unskilled labor. Since many of the world's poorest depend on casual agricultural labor for their livelihood, while at the same time minimum-wage legislation is unlikely to be effective in many developing countries, we argue that rural public employment programs constitute a potentially important anti-poverty policy tool.

JEL classification: H53, J31, J38.

Key words: public works; wages; NREGA; National Rural Employment Guarantee; Asia; India

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1. Introduction

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

Implementing minimum wage legislation is unrealistic in most developing countries, but public employment programs can be a useful tool for increasing labor demand. It has long been hypothesized that this increase in demand for labor 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 program—the Indian government's National Rural Employment Guarantee (NREG)—and analyses its impact on agricultural wages. Using a decade's worth (2000–2011) of monthly data on agricultural wages for a panel of over 200 Indian districts, we find that, on average, NREG boosted the real daily agricultural wage rate in India by 4.3% per year. A key contribution of the paper is the use of monthly observations of wages, allowing us to undertake analysis that would not be possible with less frequent observations. The key findings suggest that the introduction of NREG in an average district caused an increase in the growth rate of wages in the district, as opposed to a one-off step increase — a gradual increase rather than a jump. This is consistent with the notion that the program gradually increased in intensity once launched. While the impact on the growth rate of agricultural wages is unlikely to continue indefinitely, we do not find evidence that it had diminished by the end of our time series, three years after its introduction in all of India's rural districts.

The idea of tying welfare benefits to work requirements goes back at least as far as

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[3] A large literature confirms the negative association between agricultural wages and poverty rates (Deaton and Drèze 2002, Eswaran et al. 2009, Kijima and Lanjouw 2005, Lanjouw and Murgai 2009).

[4] However, this would not be a Pareto improvement: increases in wage rates correspond to higher input prices for employers. But given that agricultural employers are almost invariably better off than the landless, and also fewer in number, this may be an acceptable trade-off.

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. In Germany, public works were used in the 1930s to alleviate unemployment and build inter-city highways—many of which are still in use today. The term 'workfare' was coined in the United States in the late 1960s to describe the idea of social benefits tied to work requirements. Public works have diminished in importance in rich countries, but they are still widespread in the developing world. Subbarao (2003) provides an overview of contemporary public employment programs in Asia and Africa.

Despite the long history of public employment programs, 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 *labor market effect* related to the shift in labor demand; and a *productivity effect* related to the investment in public goods produced under the scheme. The labor market effect would include, but need not be limited to, an increase in wages.[6] 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 labor productivity.

Note that with our data, we are unable to distinguish empirically between the *labor market* and *productivity* mechanisms, but our general impression of the reality of NREG aligns 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.'[7]

India and NREG provide a good context in which to study the impact of public employment programs on wages. First, NREG is an enormous program by any standard and is therefore of considerable interest in itself. In the financial year 2010–11, it generated 2.6

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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 characterised by distortions such as monopsony power of the employers (Basu et al. 2009) or labour-tying (Basu 2013).

[7] However, Ranaware et al (2015) find in a survey that 90% of users of works produced under NREG consider them useful.

billion person-days of employment. Evaluations of pilot schemes are often criticized on the basis that the observed effects may not be scalable. That critique certainly does not apply here, and any lessons learnt should be of broad interest. Second, empirical studies of the wage effects of public works programs 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 of studying the Indian context. Third, the scheme was introduced in 2006 and extended to all of India (except disputed and wholly urban areas) in 2008 in three distinct phases. The phased rollout allows us to use difference-in-difference 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 attributed 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 two key contributions. First, it estimates the effect of a large employment program on wages in a developing country. There are several theoretical studies analyzing these issues, but empirical tests of these theories in a developing country context are still rare. Second, our dataset allows us to test for heterogeneous effects across regions, seasons, skill category and gender, while allowing for district-specific trends in wage rates to address endogeneity concerns.

Our main findings are as follows: NREG increased the growth rate of agricultural wages by 4.3% per year in the average district in the first few years after its introduction. The effect does not appear to have diminished by mid-2011. The effect is strongest in states that are traditionally strong in the implementation of social programs and in states that have previously been identified as 'star performers' in NREG implementation. The effect appears to be concentrated in the main agricultural season in India, that is, in the second half of the calendar year, when agricultural labor is relatively scarce. The scheme mainly affects wages for unskilled as opposed to skilled labor. There is no discernible difference in the effect on men's and women's wages. The effect is positive and significant across districts in all three phases of implementation.

Our paper relates to the 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.[8] Besley and Coate (1992) emphasize the screening benefits of workfare in both developed and developing countries.[9] They argue that work requirements make it easier for the government to screen 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. Besley and Coate present the optimal workfare program for screening purposes and derive a sufficient condition for this to be cheaper than cash benefits. More recently, Basu, Chau and Kanbur (2009) and Basu (2013) show theoretically that workfare programs can benefit the poor through efficiency gains in an agricultural labor market characterized by distortions such as labor tying and employer monopsony.

The empirical literature on public works and agricultural wages is growing. Ravallion, Datt and Chaudhuri (1993) and Gaiha (1997) study the Maharashtra Employment Guarantee (MEG) which was operational in the Indian state of Maharashtra from 1970 until it was superseded by NREG. They authors study the impact of an official wage increase in 1988 on MEG employment and conclude that there is 'little sign in these data of anything more than a slight impact of changes in the [MEG] wage on agricultural wages in either the short run or the long run.' Gaiha (1997) examines the impact of MEG on agricultural wages using monthly data. He finds a positive effect of MEG on agricultural wages in Maharashtra. Our study differs from these in that we look at a nationwide program and use monthly and district level data from 18 states. Our identification strategy also differs in that we exploit the phased rollout of NREG to compute a difference-in-difference type estimator.

There are now a number of studies that relate specifically to NREG. Mann and Pande (2012) provide an overview of published and unpublished work up to 2012. Drèze and Oldiges (2011) examine the implementation of NREG using administrative data. Using household data from three Indian states, Jha, Bhattacharyya and Gaiha (2011) analyze the nutritional impact of NREG wage, non-NREG income, and Public Distribution System (PDS) participation. Dutta et al. (2012) use 2009–10 NSS data to show that there is much unmet

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[8] 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.

[9] They also emphasise the deterrent argument, whereby workfare might encourage poverty-reducing investment by the poor.

demand for NREG work in all states. Niehaus and Sukhtankar (2013a, 2013b) study the effect on corruption of a statutory increase in NREG wages. Klonner and Oldiges (2014) assess the impact of NREG on poverty and welfare. Muralidharan et al (2016) find that NREG was delivered more efficiently after the introduction of a smartcard system in Andhra Pradesh. Afridi et al (2016) study the effect of parental participation in NREG on children's educational attainment. While some of these studies empirically analyze poverty and nutritional impact of NREG at the household level, none of them deal with the impact of NREG on agricultural wages.

We are aware of three papers, concurrent with ours, that estimate the impact of NREG on earnings. Azam (2012), Imbert and Papp (2015) and Zimmermann (2012) all use household-level cross-sectional survey data from the National Sample Survey (NSS), while we employ a long-running series of district-wise monthly wage rates called Agricultural Wages in India (AWI). Like us, Azam as well as Imbert and Papp use the phased roll-out of the scheme to identify difference-in-differences estimates. They construct their data using several NSS 'thick rounds', 'thin rounds', and 'sub-rounds'.[10] While Imbert and Papp find that NREG increases casual earnings, and more so for men than for women, Azam also reports an overall positive earnings effect but a larger effect for women. Their findings of earnings increases align well with ours. However, we do not find a statistically significant gender difference in the effect of NREG, and we argue that the difference may arise because we study wage rates rather than earnings. Zimmermann (2012) also draws on NSS data, but uses a regression discontinuity design rather than computing differences in differences, and finds zero or marginal earnings effects for both men and women. Thus, while drawing on the same data, these three papers report qualitatively different findings.

Relative to these three studies, our paper's contribution is in part to use a completely unrelated source of data to investigate whether NREG has had a general-equilibrium effect on wages. While NSS data are collected relatively infrequently, our data source provides monthly observations by district. As well as providing an independent estimate of the effect

[10] 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 (National Sample Survey Organisation 2007). In the sub-rounds, the district-level sample sizes can be very small.

of NREG on wages, this allows us to undertake analysis which can only with difficulty be done with NSS. Rather than just comparing wages averaged over some period before and some period after the introduction of NREG, we are able to conclude that the effect was gradual. While the AWI offers superior data frequency, its district coverage is limited in practice and covers only 42% of the Indian population in our main specification. This could potentially explain why some of our findings differ from those in related work.

Unlike the NSS-based studies, we control for pre-existing district-level *trends* in wages, in addition to district fixed effects, to address concerns of bias arising from the fact that the allocation of districts to the three phases was based on an indicator of 'backwardness' rather than random. We study heterogeneity of the wage effect by month of the year (seasonality). Unlike the analyses based on NSS data, which club together Phases I and II and study them as a single intervention, we can and do study the effect of NREG across the three phases separately. We have enough pre-program data to construct placebo tests as further robustness checks. Finally, because our data provide wages by type of agricultural labor, we can contrast the wage effect for unskilled labor with those for several categories of skilled labor.

2. The National Rural Employment Guarantee

NREG is the latest in a succession of rural employment programs in India. The primary objective is to provide wage employment to un- and under-employed agricultural laborers, but it also aims produce and restore public goods, typically in the form of local infrastructure, and to promote decentralized governance. Much of the responsibility for implementing the program is assigned to the Gram Panchayats (GPs), the lowest tier of elected government.

Despite the precursors, the passing of the National Rural Employment Guarantee Act (NREGA) in 2005 was widely seen as a significant event.[11] For the first time, the Indian government enacted a commitment to provide employment. In addition to providing '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

[11] Drèze (2010) describes the political process leading up to the enactment.

manual work' (Ministry of Law and Justice 2005), the act also created other entitlements: Work is to be provided within 15 days of receipt of a written or oral job application. Applicants are entitled to an unemployment allowance if the local government fails to provide work, and to a transport allowance if the site is more than 5km from their home. The act stipulates equal wages for men and women, and it sets down some minimum standards for the worksites, including access to drinking water, shade and childcare. The operational guidelines supplementing the act (Ministry of Rural Development 2008) provide more detail.

The program was rolled out across all of India's districts (except disputed areas and wholly urban districts) in three distinct phases. Two hundred districts, considered by the government's Planning Commission to be the most 'backward', received the program in February 2006. A further 130 districts received the program in April 2007, and the remaining districts followed in April 2008. This phased roll-out serves as the basis for our identification strategy. However, as the allocation of districts to the three phases was based on 'backwardness',[12] it will be important to take account of possible pre-existing differences between districts when estimating the program's effect on wages.

Table 1 presents a summary of NREG operations in the period 2006–2016, based on official records. Annual expenditure on NREG increased rapidly in the roll-out period (2006–2008), and has averaged $7 billion or 0.4% of India's GDP since 2008. Given that government spending as a share of GDP in India is approximately 10%, NREG represents about 4% of India's government spending. The number of households who obtain work under NREG fluctuates around 50 million per year. This corresponds to about a fifth of all Indian households, and more than a quarter of rural households. NREG is, by an order of magnitude, the world's largest public works program in terms of the number of beneficiaries (World Bank 2015). For an account of the program in the period we analyze, based on official data, see Drèze and Oldiges (2011).

Discrepancies between lawmakers' intentions and ground realities have been noted by several analysts. A government audit in 2008 found many irregularities in the way the scheme was implemented at the state, district and local levels (Comptroller and Auditor General of India 2008). Imbert and Papp (2011) compare reported labor participation in the

[12] Zimmermann (2012) and Klonner and Oldiges (2014) provide more detail on the 'backwardness index' that served as the basis for the allocation of districts to phases.

scheme with household data from the National Sample Survey, and find that only 46–52% of reported person-days of employment can be confirmed. However, such measurement error is unlikely to affect our estimates, as we use a binary treatment variable and exposure to NREG in months as our key explanatory variables. A clear advantage of these is that they do not rely on official reports of labor participation or scheme activity.

NREG activity is highly seasonal (Planning Commission 2013), and activity tends to peak in the dry season, March–June, when demand for agricultural labor is low. However, it has been observed that NREG activity can drop sharply between March (the last month of the financial year) and April, suggesting that activity was at least partly supply-led in the early years (e.g., Dey and Bedi, 2010).

3. Data

The main data used here are 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. The AWI series is published by the Indian Ministry of Agriculture and provides daily wage rates for a number of categories of rural labor. For most categories, there are separate series for men and women. AWI covers three main categories of unskilled labor: 'field labor', 'other agricultural labor' and 'herding'. The most important category, field labor, is for several states further disaggregated into four tasks: ploughing, sowing, weeding and reaping. In contrast, 'other agricultural labor' is not further disaggregated, but includes tasks like digging and carrying. The third category of unskilled labor is 'herding', the data quality for which is patchy and where the unit of measurement is sometimes unclear. The focus of the analysis will be on wages for field labor, as this is by far the most important category, but we also look at wages for 'other agricultural labor'.[13]

Wage rates are also provided for three categories of skilled labor: carpenters, blacksmiths, and cobblers. Female wage data are not provided for these categories, probably

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[13] Based on National Sample Survey (2006), Table 42, the proportion of person-days of manual casual labour in agriculture that was devoted to 'cultivation' was 89%. While it is not clear that 'cultivation' in the National Sample Survey corresponds exactly to 'field labour' in AWI, they are at least closely related: both include the subtasks of ploughing, sowing, weeding and harvesting. It seems reasonable, therefore, to conclude that field labour is by far the most important wage category.

because workers in these categories in India are nearly always men.

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, Datt and Ravallion (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 are collected by local officials in each district. They collect information from a sample of agricultural workers from a nominated center or village in each district, and report the average. For our purposes, the series has several advantages. First, it provides wage rates disaggregated down to the month and district levels. Second, unskilled wages are reported by gender, which allows us to test whether NREG has affected the gender wage gap. Third, it reports agricultural wages in several categories, which allows us to distinguish between the effects of NREG on skilled and unskilled wages.

Not every district is covered in the AWI data, and the reasons for this are not well understood. Our regressions are based on all AWI data available for the period 2000–2011. Based on these, we construct longitudinal data on wages in 18 states that collectively cover 94% of the Indian population (based on the 2011 census). Across the 18 states we use, in our main specification, wage data from 40% of districts, covering 42% of the population. However, there is considerable variation in how many districts are represented from each state (Appendix 3). While it is not known why some districts are missing from AWI, the pattern is stable over time and unlikely to be correlated with the roll-out of NREG. We believe that our results are largely representative for most of India, with the exception of a few major states that are not represented or under-represented in the data—notably Punjab (no districts in the data) and Jharkhand.[14] The heterogeneity in data availability is also an additional reason for undertaking state-by-state analysis (section 5.2).

The AWI series has been criticized for its 'black-box' data collection methodology. Himanshu (2005) argues that the officials in charge of collecting data from each district are

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[14] Maharashtra is represented by only two districts, but this is a special case as there was a large-scale, state-level employment guarantee scheme (the Maharashtra Employment Guarantee) in place in this state for decades before the introduction of NREG, so it is not clear whether we should expect to see any effect of the new program.

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 'center' 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, because any weaknesses associated with the collection methodology are unlikely to be correlated with the phased rollout of NREG. That is, as long as any over- or under-reporting did not get systematically weaker or stronger as NREG was introduced in a district, our estimated effects are still identified. 'White noise' measurement error due to weaknesses in the collection methodology should attenuate the estimated effect and make it harder to detect, so that our estimates would be conservative rather than exaggerated.

In an early paper, Rao (1972) compares data from the AWI series to NSS data. While AWI tend to report higher wages than those in NSS, the two series are highly correlated. He finds that AWI data lack in precision but condones their use for testing 'directional' hypothesis and for comparison of wages across regions. Deaton and Drèze (2002) find that the AWI series are highly negatively correlated with expenditure-based poverty indices. Besides explicitly focusing on wages rather than earnings, using AWI data also has several advantages over NSS data for our purposes, as described in the Introduction.

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

Our main interest is in wages for field labor. In AWI, these are reported by gender and, for most states, by task (ploughing, sowing, weeding and reaping). We construct an overall field labor 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;[15] 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

[15] If missing data are correlated with program roll-out, our estimates could be biased. We check for 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 NREG exposure, along with time and district fixed effects. We find that missing data are not correlated with program rollout or NREG exposure.

wages for field labor 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. We look for differential effects by gender in section 5.5.

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 country, 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 plots agricultural real wages against time, averaged across all districts in each phase. Note that the wages in Phase I districts are the lowest and average wages in Phase III districts are higher than Phases I and II, reflecting the non-random allocation of districts to phases.

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 in a particular month and 0 otherwise. Hence, all district series start with zero and end with one after a single switch determined by the NREG start date of that district's phase.

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 in every period 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 a potential concern that AWI simply report NREG statutory wages rather than market wages after the introduction of the scheme, wages for 'field labor' (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 some districts. Still, these districts are dropped from the sample in order to err on the side of caution. For the sake of data consistency, we also dropped 34 districts in which the location of the AWI data collection center changed over the period, but we have checked that our results are robust to keeping them.

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 recaps the key variables and how they are constructed. Even though some observations drop out of the regressions due to missing values, our preferred specification in Table 3, column 4, uses 15,272 observations from 209 districts in 18 states. As discussed above and in Appendix 3, the effective sample is fairly representative.

4. Empirical strategy

One 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 program allows us to use the districts in which NREG was already present, or not yet present, to take account of wage increases not related to the introduction of the scheme (difference-in-difference estimates).

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. That is, using the AWI monthly data we can substantially relax the standard parallel trends assumption.

The basic specification takes the form

$$y_{it} = \alpha + \theta T_{it} + \varepsilon_{it}, \qquad (1)$$

where y_{it} is the natural logarithm of real daily wages in district *i* in month t, α is a constant, T_{it} is the NREG treatment variable for district i at time t and ε_{it} is an error term which may be serially correlated. Time *t* is measured as the number of months since January 2000. 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*. (See the Data section for information on how the treatment variables are defined.) The coefficient of interest, θ, represents the impact of

NREG on agricultural wages, interpreted either as a one-off discontinuity (jump) in wages at the time of introduction, or as a kink in the wage growth trend. Every regression incorporates robust standard errors, clustered at the district level, to take account of serial correlation in the error term.

The specification with a binary treatment variable tests for a discontinuous 'jump' in wages at the time of introduction of the program. In specifications where T_{it} is, instead, a variable counting the months of exposure to the program district *i* had had at time *t*, the estimated coefficient is to be interpreted as the change in the monthly growth rate of wages caused by the program. In a plot of the wage rate against time, the binary treatment variable would identify a discontinuous jump in wages, while the exposure variable would identify a kink, or change in the slope, in the evolution of wages over time.

The basic specification will be augmented by district, year and month fixed effects as well as by district-specific time trends. Thus, our full specification takes the form

$$y_{it} = \alpha_i + \beta_i t + \theta T_{it} + \varphi_y + \gamma_m + \varepsilon_{it}, \qquad (2)$$

where α_i and $\beta_i t$ capture district fixed effects and district time trends, φ_y and γ_m are fixed effects for year and month of the year, respectively, and the remaining quantities are as in (1). In words, the augmented specification takes account of pre-existing differences in both levels and trends in agricultural wage rates at the district level, as well as year-on-year macroeconomic developments in wages that might affect the whole country, and also any seasonal variation. The way we treat the error terms take account of the fact that wages are likely to be serially correlated over time. We identify any variation captured by the treatment variable, net of these fixed effects and trends, as the causal effect of the program.

Selection is a potential source of bias. As mentioned, districts were not randomly allocated to the three phases of the NREG rollout. In fact, it was an explicit aim of the government 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 parallel trends), 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 (linear) wage growth paths is addressed by introducing district-specific trends as controls. We also run placebo tests as further checks that the estimates are not driven by selection bias.[16]

Most papers analyzing the effects of the introduction of NREG compute, as we do, difference-in-difference estimates (DID). However, Zimmermann (2012) and Klonner and Oldiges (2014) instead rely on regression discontinuity (RD). They use the 'backwardness index' constructed by the Indian government's Planning Commission to divide districts into phases as the running variable, and identify the effect of the program as the jump in the outcome variables at the cut-off that defines the division of districts into Phase II or Phase III. (Hence, they are unable to compare the effect of the program across the different phases.)

RD as a technique for estimating treatment effects has advantages and disadvantages relative to DID. The main advantage is to identify the treatment effect based on a discontinuity in eligibility, thereby in principle avoiding concerns that treatment and control groups are 'otherwise different'. However, basic RD estimates the treatment effect only at a particular value of the running variable, namely at the cut-off. That is, the RD method conceptually estimates the effect of the program as the difference in outcome between a district that is just poor enough to be included in the program and a district that is just rich enough to be excluded. In order to estimate an *average* treatment effect, which is what is usually of the greatest policy interest, one has to make further assumptions on how the treatment effect evolves when moving away from the discontinuity—assumptions that weaken the potentially clean identification that makes RD attractive in the first place.

As mentioned above, we address concerns that districts in different phases may have been on different pre-existing wage growth paths by allowing for district-wise linear trends, and we use placebo tests to confirm that the findings are not driven by non-linear but persistent district-level trends.

[16] The Backward Regions Grant Fund, which was introduced in 2007 and aimed at 250 backward districts, is a possible confounder. However, it was much smaller than NREG, peaking at 8% of NREG expenditure in 2007-2008 (Planning Commission 2014). It was also not intended as an employment scheme or explicitly aimed at agricultural labourers. We therefore believe that this scheme's effect on agricultural wage rates, if any, would have been small.

5. Results

5.1 Main results: The impact of NREG on unskilled field labor wages

Table 3 presents the main results: the effect of NREG on real daily wage rates for unskilled field labor. The dependent variable here and throughout is the logarithm of daily wage rates measured in January 2000 prices. In column 1, field labor wages are regressed on the binary NREG treatment variable (specification 1). The coefficient is significant and indicates that wage rates in NREG-treated districts and months are approximately 7% 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.

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 0.0031 and highly significant. In column 4, district-specific time trends are included in addition. This would take account of any bias arising from differential linear trends, that is, wage growth rates that vary across districts independently of NREG. This does leave some threats to internal validity but they would need to be of a specific type, that is, non-linear trends. To recap, the monthly data allow us to substantially relax the standard difference-in-difference assumption that treatment and control areas were on parallel trends before the introduction of NREG, by controlling for district-wise linear time trends. The coefficient increases to 0.0035 and remains highly significant. This is our preferred specification (2), and the interpretation is that on average, the introduction of NREG in a district increased the growth rate of agricultural wages by 0.35% per month or 4.3% per year.[17] That is, NREG causes a positive and significant increase in the growth rate of agricultural wages of, on average, 4.3% per year on top of any underlying trends.

A gradual rather than immediate effect on wages is consistent with the idea that, once launched in a district, the program caused a gradual increase in the demand for labor, possibly linked to a gradual improvement in scheme efficiency and capacity.

[17] 1.0035^12 - 1 = 0.0428

In column 5, the binary and exposure terms are included simultaneously, and the results confirm that NREG is associated with an increase in wage growth rather than a step change.

This contrasts with Imbert and Papp (2015) 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 need not result from 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. Likewise, a gradual increase in wages like the one we identify should also show up as a binary effect given sufficient post-intervention data. This suggests that the number of data-points in the post-intervention period is insufficient to identify the associated increase in average wage levels.

While it is possible that NREG causes a permanent increase in wage growth, it is more plausible that the effect would diminish over time. In this scenario, the introduction of NREG would be followed by a gradual adjustment to a new labor 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 stabilizes.

To look for a diminishing effect, a squared exposure term is introduced in the regression in column 6. If the effect of NREG diminishes over time, a negative and significant squared-term coefficient would be expected. However, 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 not that the effect of NREG on wage growth is permanent, but rather that we do not observe wages for sufficiently long after the introduction of the scheme to be able to identify the diminishment.

It is of interest to consider how the effect of NREG relates to growth in agricultural wages in India overall. Rough estimation implies that NREG increased agricultural wages in India as a whole by 1.4% per annum between February 2006 and March 2007, by 2.3% per annum between April 2007 and March 2008, and by the full 4.3% per annum between April 2008 and the end of our time series in mid-2011.[18] Using a different data source (Wage Rates

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[18] There were 200 districts in Phase I, 130 in Phase II and 295 in Phase III. Weighting all districts equally and applying the main estimate of 4.3% annual growth throughout the period, the contribution of NREG to overall growth was 4.3% * 200 / (200+130+295) = 1.4% per annum in Phase I, 4.3% * (200+130) / (200+130+295) =

in Rural India), Reddy (2015) estimates that, in the period 2006–2012, wage rates for male workers grew by 4.75% and 5.04% per annum for sowing and ploughing, respectively. This suggests that NREG has been an important contributor to overall growth in agricultural wages in India, in particular in the period 2008–2011 when all districts were included.

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 (MEG) on agricultural wages. MEG started operations in 1978 and is by many considered to be a direct precursor of NREG. Gaiha (1997) reports a long run effect of MEG on wages of 18% and a short run effect of 10%. Ravallion et al. (1993) find little evidence of an increase in MEG statutory wages passing through to agricultural wages either in the long or the short run. Imbert and Papp (2015) look at the impact of a binary NREG treatment variable, but use NSS rather than AWI data. They report that the impact is 4.7% per annum on average, quite close to and well within one standard error of our main estimate of 4.3% per annum. However, it should be kept in mind that they estimate the impact on earnings rather than wage rates.

By way of benchmarking our findings against minimum wages, we obtained data on minimum wages for agricultural labor in January 2006, just before NREG was introduced. In India, minimum wages are set at the level of the state. We then computed the ratio of (nominal) field labor wage rate to the minimum wage for all districts in our sample. In January 2006, this ratio was 0.963 in the average district. The implication is that, using our key estimate of 4.3% annual growth in wages, it would take just over a year of exposure to NREG for wages in the average district to catch up with the minimum wage.

However, there is considerable variation across districts. It is, therefore, also of interest to look at the proportion of districts where wages met the minimum standard. In January 2006, the minimum wage was met or exceeded in 27% of sample districts. Our results would predict, if minimum wages were not raised in the meantime, and ignoring inflation, that this would rise to 33% of districts after one year of India-wide NREG exposure.

2.3% per annum in Phase II and 4.3% per annum in Phase III.

5.2 State-wise estimates

There is variation in the vigor 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 (Drèze and Khera 2009). In order to study regional heterogeneity, the effect of NREG on wages is estimated state by state. The specification used corresponds to the one in Table 3, column 4: the logarithm of real field labor wages by district and month are regressed on exposure to NREG in months, and fixed effects for year, month and district as well as district-wise time trends are included.

Figure 3 presents the results. There are positive and significant effects for Rajasthan and Andhra Pradesh, two states that were highlighted as top performers in Drèze and Khera (2009). The effects are also positive and significant for Kerala and West Bengal. These are states that tend to do well on many social metrics. In addition, there are positive and marginally significant (at the 10% level) effects for Bihar and Haryana. For all other states in the data set, the effects are not individually statistically significant. All but four point estimates are positive, and no state is associated with a statistically significant negative effect.

5.3 Seasonal variation

In order to study whether the effect of NREG is uniform throughout the year, wage rates were 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 4. The dots represent point estimates and the bars, robust 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 monthly estimates are not statistically significantly different from each other, and that all point estimates are positive.

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, to the extent seasonal variation can be identified, it appears that the demand shift in labor due to NREG exposure pushes up wages only when agricultural workers are in relatively high demand. This seems reasonable: as with any commodity, we would expect the price of labor to increase when it is scarce.

These results may appear to contrast with the findings of Imbert and Papp (2015), who find that the earnings effect of NREG are concentrated in the *first* half of the year, that is, the agricultural slack season. The difference between their findings and ours may arise partly from the fact that we study wages while they look at earnings. Earnings equal wage rates only for the fully employed. For the un- and under-employed, earnings will typically be below wage rates. While it is reasonable that the program should increase equilibrium *wages* more when competition for labor is high, it is also plausible that the effect on *earnings* should be stronger in the slack season, when there are few employment options for agricultural workers.

The difference in results could also be due to the smaller number of districts in the AWI data relative to NSS.

5.4 Impact on skilled labor wages

Table 4 presents the results of regressions of wages in the skilled labor 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 influence the wages paid for any of these categories of labor.

The intended beneficiaries of NREG are unskilled agricultural laborers. To the extent that skilled and unskilled workers operate in separate labor markets, it is not surprising that the wages or carpenters, cobblers and blacksmiths are insulated from the effect of the program. For example, if carpenters are typically more fully employed and generally receive higher wage rates than those offered by NREG, as seems plausible, then we would not expect NREG to attract many qualified carpenters, and hence we should not expect to see an equilibrium effect on carpenters' wages.

How correlated are these wages to the field labor wages? In Table 5, we examine the correlation and report the correlation matrix. The numbers suggest only a moderate correlation between field labor wages and the wages of the skilled categories. This further corroborates our hypothesis that skilled and unskilled workers operate in separate labor 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 demand for female labor from NREG projects might be expected to 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 the genders. In Table 6, we study the impact of NREG intensity on the gender gap in agricultural wages. Here, men's and women's wages are considered separately but in a pooled regression; 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 an interaction term Treated × female in column 1 and Exposure × 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 is negative and significant, reflecting the gender wage gap in Indian agriculture. The coefficient implies that the wages of female field laborers are typically only 79% of their male counterparts' wages.[19] The binary treatment coefficient is small and not significant, as is the interaction between treatment and female. The implication is that, consistent with the main results above, the introduction of NREG is not associated with a step change in wages for either men or women.

In column 2 we look at exposure to NREG in months rather than the binary treatment variable. The un-interacted female variable hardly changes. The un-interacted exposure variable (0.32% per month) is positive, significant and similar in magnitude to what we found above (Table 3, column 4), while the interaction between female and exposure is small and not significantly different from zero. The interpretation is that, 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 gap in equilibrium unskilled agricultural wages.

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

Separate regressions of men's and women's wages on exposure yield somewhat higher effects for women than for men: the point estimates are 0.30% per month for men and 0.37% per month for women (not reported).

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. But the distinction between wages and earnings is again relevant. Azam's estimated effect on female earnings is consistent with earlier work noting that the program disproportionately employs women (Drèze and Khera 2009). Since women in rural India are much less likely to be in wage employment than men, NREG might be expected to make a bigger difference to the earnings of women than men. But the equilibrium price of labor (the wage rate) will only reflect an increase in demand to the extent that the good (labor) becomes scarce. That is, residual slack in the female labor market may explain why the scheme's upward pressure on female-specific wages is muted.

5.6 Phase-wise effects and placebo test

Hitherto it has been implicitly assumed that the program had an identical effect for districts across all three phases. However, the effect could, in principle, differ by phase. Table 7 tests for differences in effect size across the three phases. As before, district, year and month-of-the-year (seasonal) fixed effects are included, as are district-specific time trends, and the standard errors are robust and clustered at the district level.

Columns 1–3 examine the impact of the binary treatment variable. Column 1 pools the three phases. No significant effect is found for Phase I districts, captured by the coefficient on the un-interacted "Treated" variable, and there is also no significant difference in effect between Phase I districts and Phase II or Phase III districts, captured by the interaction variables. In column 2, Phase III districts are dropped to obtain a cleaner comparison between Phase II and Phase I, and in column 3, Phase I districts are dropped, so that Phase II districts are now the reference category ("Treated") for Phase III districts. None of the coefficients are statistically significant.

In columns 4–6, the exercise is repeated for the "Exposure" variable. Column 4 pools all three phases, column 5 drops Phase III districts and column 6 drops Phase I districts. In all three columns, the un-interacted Exposure variable is positive and statistically significant, while the interactions between Exposure and individual Phase indicators are not. This confirms that exposure to the program had a positive effect on wage across all three phases. Moreover, while the point estimate varies, it is not possible to reject the hypothesis that the effect of program exposure was the same for all three phases.

Overall, the phase-wise analysis confirms the finding that NREG had a gradual effect on wages, and suggests that NREG had a similar effect in districts across all three phases.

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.[20] Hence Phase I districts 'received' the placebo 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.

We also ran a second placebo check, with the 'treatment' shifted back by five years rather than three. Again, observations associated with the real treatment (from 2006 onwards) were deleted, and the specification was otherwise the same. The results were, qualitatively,

[20] 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 are roughly balanced for Phase I districts.

the same as for the first placebo test (not reported).

Finally, following the suggestion of an anonymous referee, we ran a third placebo test (not reported but available on request). Here we study the period February 2005 to March 2007 only. We drop Phase I districts from the analysis, and look for a (placebo) effect in Phase II districts starting in February 2006. This is when the scheme was introduced in Phase I districts, but we would not expect an effect on wages in Phase II districts. For this analysis, Phase III districts serve as the comparison group for Phase II's placebo treatment. The specification is otherwise as in the placebo tests reported above, and once again we do not see any effect of the placebo treatment.

5.7 'Other agricultural labor'

Field labor represents the bulk of unskilled agricultural labor in India (see footnote 11), and wages for field labor is the main outcome variable considered in this paper. However, in the unskilled category, AWI also reports wages for 'other agricultural labor'. According to the AWI definitions, while field labor covers key agricultural tasks like ploughing, sowing, weeding, transplanting and reaping, 'other agricultural labor' include 'coolies employed for watering the fields, load carriers coolies [sic], well diggers, laborers for cleaning silt from waterways and embankments, etc.'

While quantitatively less important than field labor, and somewhat nebulously defined, 'other' labor is of interest in part because a significant proportion of employment offered under NREG would probably fall under this category. Unskilled rural workers should flow freely between field and 'other' labor, hence we might expect to see an effect of the program on wages for both these categories.[21] The wage series for field labor and other agricultural labor are also highly correlated (Table 5).

Table 9 mirrors the regressions presented in Table 3, except that the dependent variable is now wages for 'other agricultural labor'. The effect is significant in columns 1 and 3. However, while the magnitude remains positive at 0.22% per month, the coefficient is no longer statistically significant at conventional levels (p=0.13) once the full set of fixed effects and trends are included in column 4.

[21] As argued in section 5.4, it is reasonable that no effect is detected on wages for skilled labour.

Note that the number of state observations has dropped from 18 in Table 3 to 16 in Table 9. It turns out that AWI does not provide wage data for 'other' labor in Rajasthan. As Rajasthan is one of the previously identified 'star' states in terms of NREG implementation and associated with one of the clearest state-level effects in our analysis (Figure 3), we believe that the lack of wage data from this state may explain why the coefficient for 'other' labor is not significant overall. (The other dropped state is Uttarakhand.)

This explanation is corroborated by state-level estimates for 'other' labor, in Figure 5. While Rajasthan drops from the analysis, the results are otherwise qualitatively similar to those for field labor wages (Figure 3). West Bengal and Andhra Pradesh are associated with positive effects that are individually statistically significant. The effect is marginally significant (at the 10% level) for Bihar. Like for field labor wages, a majority of the state-wise estimates are positive, and none are negative and significant.

In Table 10, the effect of NREG on 'other' labor wages is allowed to vary by phase. As for field labor (Table 7), there is evidence of a kink in the wage series at the time of the introduction of the scheme. The estimated effect is 0.33% per month and highly statistically significant (p<0.01), and there is no evidence that it varies across the phases. Apart from a marginally significant effect for Phase III, there is no evidence of a step change effect.

We do not find that the effect on 'other' labor wages varies by gender or season (not reported).

The evidence presented in this section on the effect of NREG on wages for 'other agricultural labor' are consistent with the findings for field labor wages. The estimated effect in the main specification (Table 9, column 4) is 0.22% per month with a p-value of 0.13. We argue that the lack of significance in the main specification may be that AWI does not provide wage data for 'other agricultural labor' in Rajasthan, one of the states with the clearest identified effects for field labor wages. State-level estimates are qualitatively similar to what was found for field labor wages, and phase-wise specification confirms that the most likely effect of the program is a 'kink' (positive and statistically significant) in the plot of wages over time, rather than a step change. Furthermore, we cannot reject that the effect of the program is the same for districts across all three implementation phases.

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 boosted the growth rate of real daily agricultural wages by 4.3% per year. The effect of NREG on equilibrium wages appears to be gender neutral and concentrated in the main agricultural season. It also appears to be well targeted, as it affects wages for unskilled but not for skilled labor. State-by-state estimates suggest significant and positive effects in Andhra Pradesh, Rajasthan, West Bengal and Kerala—the former two having previously been identified as 'star performers' in terms of NREG implementation, and the latter two being states that tend to do well on many social indicators. The validity of our identification strategy is corroborated with a placebo test.

It should be noted that, while we control for district-level linear time trends, this does not rule out the possibility that our results are driven by non-linear trends that differ between districts in the different phases. However, to the extent that these are persistent over time, such non-linear trends should have been picked up by the placebo tests. The absence of significant coefficients in the placebo tests is reassuring. For our results to be driven by underlying differences in district-level wage growth rates, these would have to be non-linear, non-persistent and coinciding with the phases of the NREG roll-out.

It is difficult, if not impossible, for developing country governments to enforce statutory minimum wages. Our findings suggest that public employment programs may provide governments with a workable, if costly, mechanism with which to influence wage rates in the rural unskilled labor 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 with the potential to reach far beyond those who actually participate in the program.

However, considerations of the total welfare effect would also need to take account of the effect of the wage increase on the labor supply of the poor, and on employers' costs, as well as the welfare effects of the public goods constructed under the scheme.

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Table 1. NREG Headline Statistics 2006–2015

	2006–07	2007–08	2008–09	2009–10	2010–11	2011–12	2012–13	2013–14	2014–15	2015–16
Number of districts covered	200	330	All*	All*	All*	All*	All*	All*	All*	All*
Total expenditure in rupees (billions)	88	159	273	379	394	375	397	386	360	440
Total expenditure in USD (billions)	1.9	4.0	5.9	7.9	8.5	7.6	7.6	6.2	5.8	6.7
Expenditure as a proportion of India's GDP	0.19%	0.33%	0.48%	0.56%	0.50%	0.42%	0.41%	0.32%	0.29%	0.32%
Person-days of employment generated (billions)	0.9	1.4	2.2	2.8	2.6	2.1	2.3	2.2	1.7	2.4
Number of households obtaining wage work (millions)	21	34	45	53	55	50	50	48	41	48
Average person-days of employment per household that obtained NREG work	43	42	48	54	47	42	46	46	40	49
Person-days of employment availed by low-caste (SC/ST) persons, of total	62%	.	55%	51%	51%	41%	40%	40%	39%	40%
Person-days of employment availed by women, of total	40%	.	48%	48%	48%	46%	47%	48%	55%	55%

Note: All data except GDP figures and exchange rates are based on official NREG records. Expenditures were converted to USD using historical exchange rates as per early October each year (the mid-point of the Indian April-March financial year). Annual GDP figures were obtained from the World Development Indicators and interpolated to correspond to the Indian April-March financial year, except for 2015-2016 which is based entirely on 2015 GDP data as 2016 data were not available at the time of writing.
* From 2008-09 onwards, the program covered all districts except wholly urban districts and those affected by conflict.

Table 2. Summary Statistics.

	Mean	SDev	Min	Max	Obs
Unskilled labor wages, average of men's and women's rates					
Field labor	58.36	28.73	17	230	15272
Other agricultural labor	58.20	31.07	15	239	14467
Unskilled labor, wage rates by gender					
Field labor (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 labor (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
Other agricultural labor (m)	64.42	36.59	10	279	15754
Other agricultural labor (f)	51.50	25.95	14	230	14483
Skilled labor 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 and deflated to January 2000 prices using state-wise monthly consumer price indices for agricultural laborers. The main category of unskilled labor is field labor. Field labor 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 labor (aggregate) are reported. The male and female field labor wages analyzed here are constructed as follows. Where provided, the general field labor 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 labor wages when both are available and missing otherwise.

Table 3. Main Results: The Effect of NREG on Unskilled Field Labor Wages

	(1) Field labor wages	(2) Field labor wages	(3) Field labor wages	(4) Field labor wages	(5) Field labor wages	(6) Field labor wages
Treated	0.073***	-0.0031			0.00030	
	(0.021)	(0.016)			(0.015)	
Exposure			0.0031***	0.0035***	0.0035***	0.0026***
			(0.0012)	(0.0013)	(0.0013)	(0.0016)
$[\text{Exposure}]^2$						2.2×10^{-5}
						(2.7×10^{-5})
District, year and month fixed effects	No	Yes	Yes	Yes	Yes	Yes
District trends	No	No	No	Yes	Yes	Yes
Observations	15272	15272	15272	15272	15272	15272
Districts	209	209	209	209	209	209
States	18	18	18	18	18	18

Notes: The dependent variable is 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 district *i* at time *t* and 0 otherwise. 'Exposure' is a non-negative integer counting the number of 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 Labor Categories

	(1) Carpenters' wages	(2) Blacksmiths' wages	(3) Cobblers' wages
Exposure	0.00059	0.0011	0.0087
	(0.0014)	(0.0017)	(0.0019)
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' is a non-negative integer counting the number of 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 Labor Categories?

	Field labor	Other agricultural labor	Carpenter	Blacksmith	Cobbler
Field labor	1				
Other agricultural labor	.94	1			
Carpenter	.78	.79	1		
Blacksmith	.61	.60	.91	1	
Cobbler	.27	.32	.71	.79	1

Table 6. NREG and the Gender Wage Gap

	(1) Field labor wages	(2) Field labor wages
Female	-0.23***	-0.23***
	(0.013)	(0.012)
Treated	0.00024	
	(0.015)	
Treated x female	-0.0038	
	(0.011)	
Exposure		0.0032**
		(0.0013)
Exposure x female		-0.000076
		(0.00036)
District, year and month fixed effects	Yes	Yes
District trends	Yes	Yes
Observations	32450	32450
Districts	209	209
States	18	18

Notes: The dependent variable is 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 district *i* at time *t* and 0 otherwise. 'Exposure' is a non-negative integer counting the number of 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 Field Labor Wages: Testing for Differential Effects by Phase

	(1) Field labor wages	(2) Field labor wages	(3) Field labor wages	(4) Field labor wages	(5) Field labor wages	(6) Field labor wages
Treated	0.013	0.017	0.012			
	(0.021)	(0.021)	(0.027)			
Treated x Phase II	-0.0082	-0.013				
	(0.037)	(0.040)				
Treated x Phase III	-0.024		-0.020			
	(0.038)		(0.042)			
Exposure				0.0031**	0.0057***	0.0064***
				(0.0013)	(0.0016)	(0.0021)
Exposure x Phase II				0.0025	0.0028	
				(0.0018)	(0.0018)	
Exposure x Phase III				-0.00037		-0.0025
				(0.0016)		(0.0021)
District, year and month (season) fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15272	9551	8656	15272	9551	8656
Districts	209	125	127	209	125	127
States	18	17	18	18	17	18

Notes: The dependent variable is 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 district *i* at time *t* and 0 otherwise. 'Exposure' is a non-negative integer counting the number of months NREG has been active in district *i* at time *t*. Standard errors, in parentheses, are robust and clustered at the district level. Phase 3 districts are deleted from columns 2 and 5, and Phase 1 districts are deleted from columns 3 and 6. * p<0.10, ** p<0.05, *** p<0.01

Table 8. Placebo Test

	(1) Field labor wages	(2) Field labor wages	(3) Field labor wages	(4) Field labor wages
Treated (placebo)	0.012			
	(0.011)			
Exposure (placebo)		0.00010	-0.0013	-0.0014
		(0.0010)	(0.0013)	(0.0016)
[Exposure (placebo)]^2				5.1×10^{-6}
				(3.7×10^{-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 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

Table 9. The Effect of NREG on Unskilled 'Other Agricultural Labor' Wages

	(1) 'Other' labor' wages	(2) 'Other' labor wages	(3) 'Other' labor wages	(4) 'Other' labor wages	(5) 'Other' labor wages	(6) 'Other' labor wages
Treated	0.075***	0.0014			0.0044	
	(0.023)	(0.018)			(0.017)	
Exposure			0.0034**	0.0022	0.0022	0.0015
			(0.0015)	(0.0015)	(0.0015)	(0.0018)
$[\text{Exposure}]^2$						1.7×10^{-5}
						(3.1×10^{-5})
District, year and month fixed effects	No	Yes	Yes	Yes	Yes	Yes
District trends	No	No	No	Yes	Yes	Yes
Observations	14467	14467	14467	14467	14467	14467
Districts	198	198	198	198	198	198
States	16	16	16	16	16	16

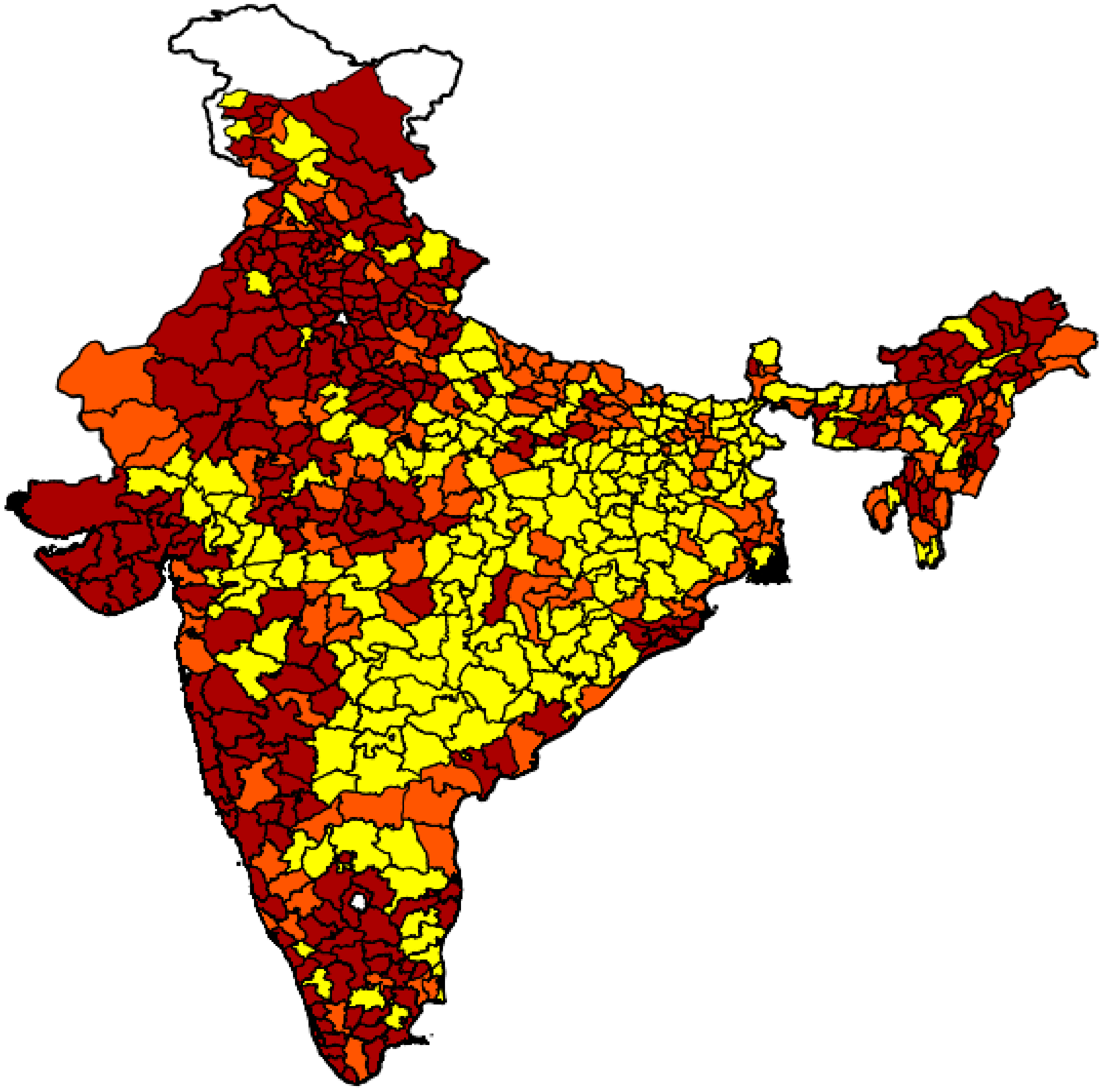
Notes: The dependent variable is log daily wages, in fixed January 2000 prices, observed between April 2000 and June 2011. 'Treated' is a binary variable equal to 1 if NREG was active in district *i* at time *t* and 0 otherwise. 'Exposure' is a non-negative integer counting the number of months NREG 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 10. The Effect of NREG on 'Other Agricultural Labor' Wages: Testing for Differential Effects by Phase

	'Other' labor wages
Treated	-0.029
	(0.022)
Treated x Phase II	0.033
	(0.041)
Treated x Phase III	0.057*
	(0.034)
Exposure	0.0033***
	(0.0012)
Exposure x Phase II	0.019
	(0.017)
Exposure x Phase III	0.0083
	(0.020)
District, year and month (season) fixed effects	Yes
District trends	Yes
Observations	14467
Districts	198
States	16

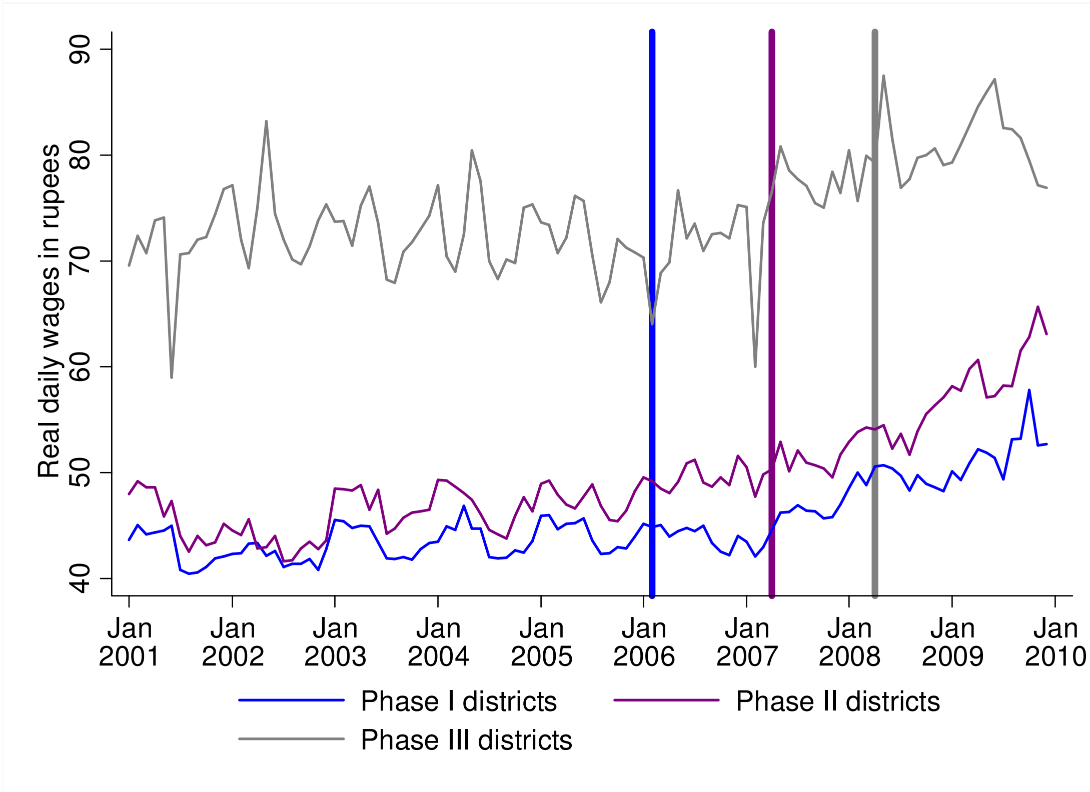
Notes: The dependent variable is log daily wages, in fixed January 2000 prices, observed between April 2000 and June 2011. 'Treated' is a binary variable equal to 1 if NREG was active in district *i* at time *t* and 0 otherwise. 'Exposure' is a non-negative integer counting the number of months NREG 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

Figure 1. The three phases of the NREG rollout.



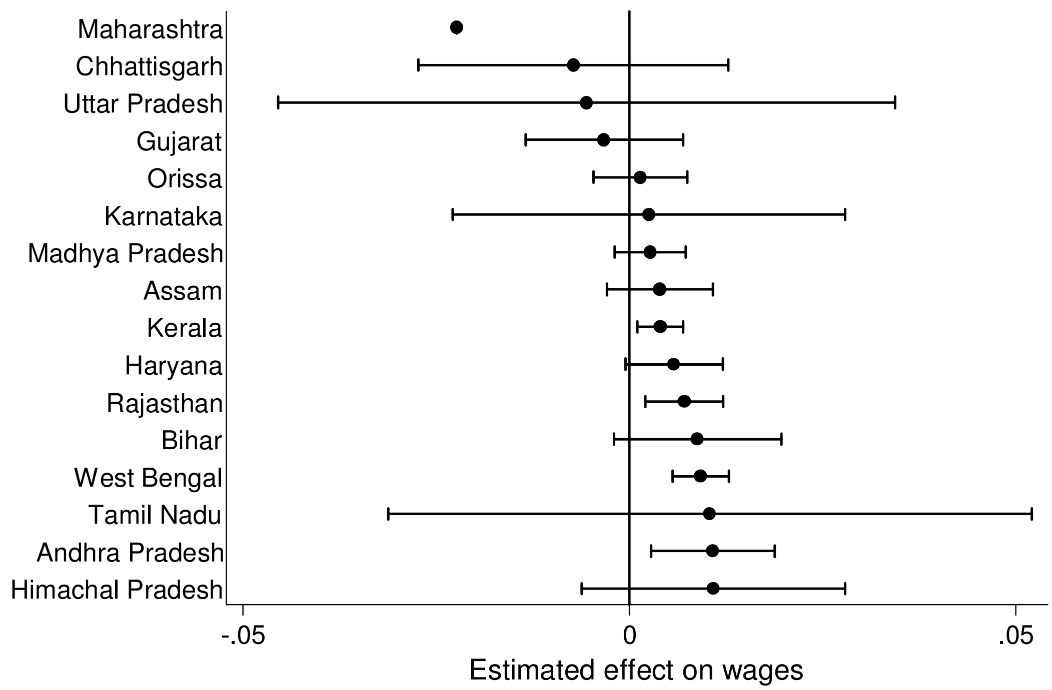
Notes: The map shows all rural districts of mainland India, colour-coded according to NREG implementation phase. Phase I districts are shown in yellow, Phase II districts in orange and Phase III districts in brown.

Figure 2. Real Agricultural Wages, by phase.



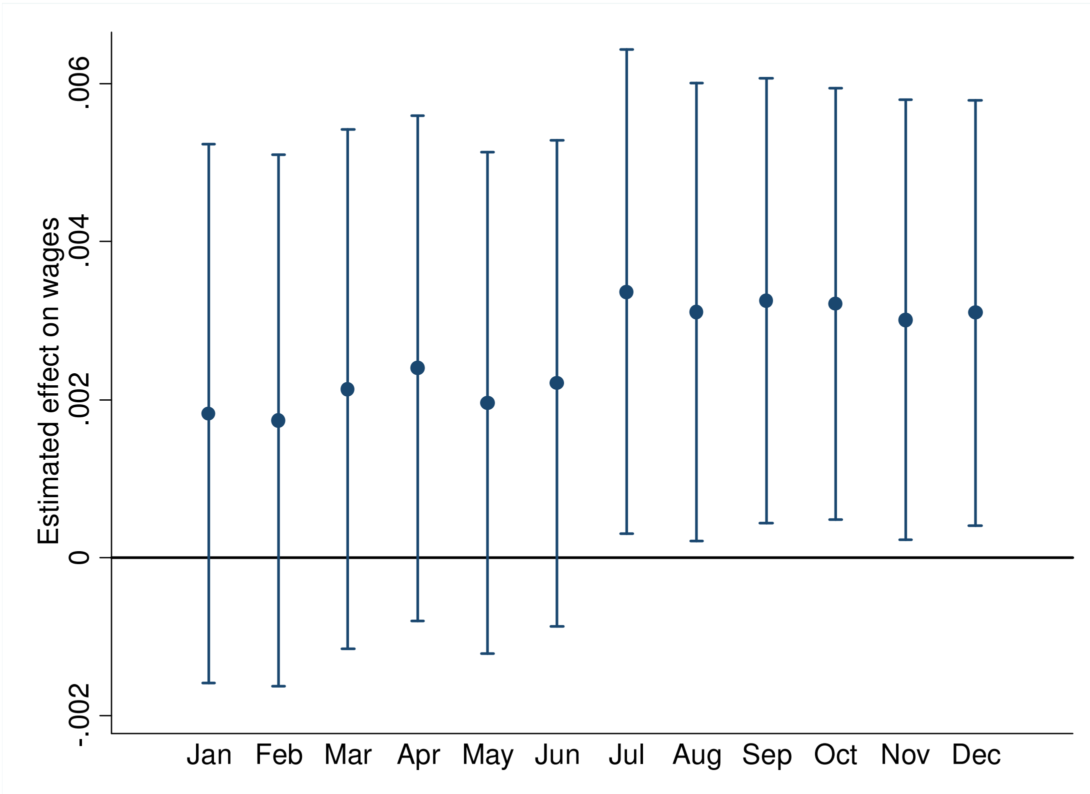
Notes: The figure shows real daily field labor wages in January 2000 prices. For each month, year and phase, the rates are averaged across the field labor sub-categories, gender and districts. The vertical lines show the time of introduction of NREG in each phase.

Figure 3. Effects of NREG on Field Labor Wages by State.



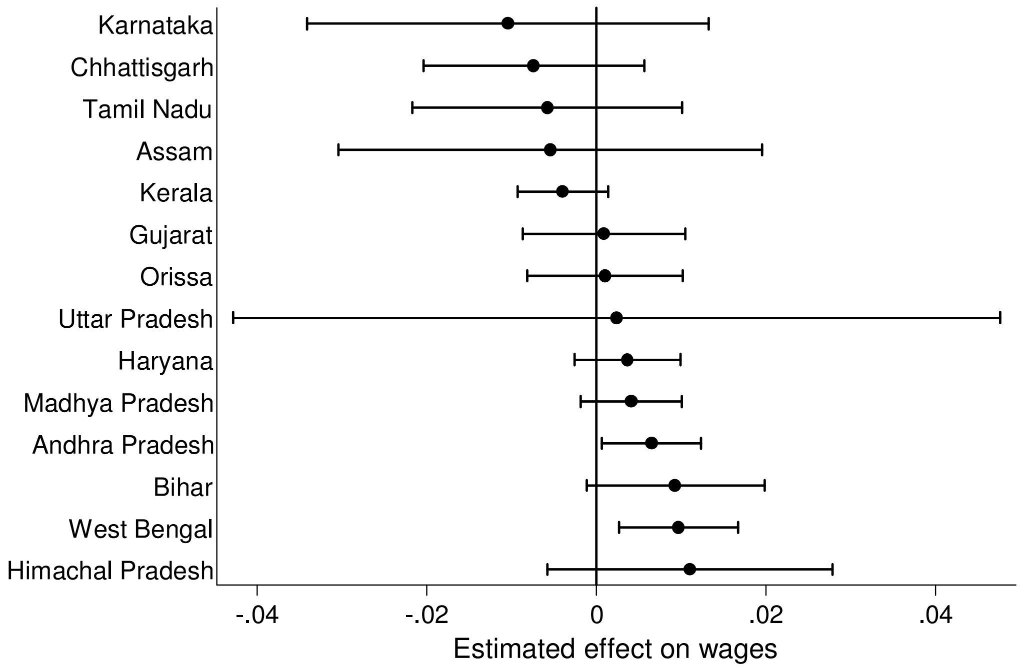
Notes: The figure shows the coefficients from state-by-state regressions of log real field labor wages on exposure to NREG in months. The regression includes district, year and month fixed effects and district-wise time trends, and standard errors are clustered at the district level. The dots show the point estimates, and the bars indicate 95% robust confidence intervals. Some states dropped out of the regression due to insufficient observations, and the confidence interval for Maharashtra is not shown because it was an order of magnitude wider than for the other states (-.48, .44). The effects for Kerala, Rajasthan, West Bengal and Andhra Pradesh are positive and individually statistically significant at the 5% level. The effects for Haryana and Bihar are positive and significant at the 10% level.

Figure 4. Effects of NREG on Wages by Month.



Notes: The figure shows the coefficients from a regression of log real field labor wages on 12 monthly binary variables, each interacted with exposure to NREG in months. The dots show the point estimates, and the bars indicate 95% robust confidence intervals. The regression includes district, year and month fixed effects and district-wise time trends, and standard errors are clustered at the district level. While the point estimates are positive for every month, the effects are individually statistically significant only in the second half of the year — the agricultural peak season.

Figure 5. Effects of NREG on 'Other Agricultural Labor' Wages by State.



Notes: The figure shows the coefficients from state-by-state regressions of log real 'other' unskilled agricultural labor wages on exposure to NREG in months. The regression includes district, year and month fixed effects and district-wise time trends, and standard errors are clustered at the district level. The dots show the point estimates, and the bars indicate 95% robust confidence intervals. Some states dropped out of the regression due to insufficient observations. The effects for Andhra Pradesh and West Bengal are positive and individually statistically significant at the 5% level. The effect for Bihar is positive and significant at the 10% level.

7. Appendices

1. Data

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 laborers, and provided by district, month, labor category and sex. More details in the Data section. *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
Andhra Pradesh	
	Adilabad, 1
	Anantapur, 1
	Chittoor, 1
	East Godavari, 2
	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

State	District, NREG phase
Gujarat	
	Ahmedabad, 3
	Amreli, 3
	Banaskantha, 1
	Bhavanagar, 3
	Dohad, 1
	Gandhi Nagar, 3
	Jamnagar, 3
	Junagadh, 3
	Kachchh, 3
	Kaira, 3
	Parbudar, 3
	Patan, 3
	Rajkot, 3
	Sabarkantha, 1
	Surendranananagar, 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

State	District, NREG phase
Madhya Pradesh	
	Ashoknagar, 2
	Balghat, 1
	Barwani, 1
	Betul, 1
	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

State	District, NREG phase
Punjab	
	Amritsar, 2
	Bhatinda, 3
	Faridkot, 3
	Fatehgarh Sahib, 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

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
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

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

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

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

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

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
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

Koriya, 1
Mahasamund, 2
Raigarh, 1
Raipur, 2
Rajnandgaon, 1
Sarguja, 1

Idukki, 2
Kannoor, 3
Kasargod, 2
Kollam, 3
Kottayam, 3
Kozikode, 3
Malapuram, 3
Palakkad, 1
Pathamamithitta, 3
Thrissur, 3
Trivandrum, 3

Phulbani, 1
Puri, 3
Rayagada, 1
Sambalpur, 1
Sonepur, 1
Sundargarh, 1
kendrapara, 3

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

3. AWI district coverage. The table shows, for each state, the number of districts used in the main specification, the overall number of districts in the state, and the proportion of state population covered by these. Population figures are from the 2011 census.

State	AWI districts	Census districts	Population in AWI districts	Population in all census districts	Proportion of population covered
Andhra Pradesh	22	23	80,655,295	84,665,533	95%
Assam	3	27	2,987,253	31,169,272	10%
Bihar	8	38	24,015,993	103,804,637	23%
Chhattisgarh	15	18	24,379,022	25,540,196	95%
Gujarat	10	26	22,255,208	60,383,628	37%
Haryana	14	21	22,092,263	25,353,081	87%
Himachal Pradesh	8	12	5,254,916	6,856,509	77%
Jharkhand	1	24	1,491,879	32,966,238	5%
Karnataka	5	30	16,000,580	61,130,704	26%
Kerala	13	14	32,571,119	33,387,677	98%
Madhya Pradesh	40	50	70,952,517	72,597,565	98%
Maharashtra	2	35	7,547,005	112,372,972	7%
Orissa	26	30	34,807,103	41,947,358	83%
Rajasthan	9	33	16,328,017	68,621,012	24%
Tamil Nadu	7	32	11,702,452	72,138,958	16%
Uttar Pradesh	7	71	24,099,691	199,581,477	12%
Uttarakhand	1	13	1,927,029	10,116,752	19%
West Bengal	18	20	78,476,104	91,347,736	86%
Total for 18 states	209	517	477,543,446	1,133,981,305	42%