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Can Rural Public Works Affect Agricultural Wages? Evidence from India[1]

Erlend Berg, Sambit Bhattacharyya, Rajasekhar Durgam and Manjula Ramachandra[2]

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Abstract. It has long been hypothesised that public works programmes, in addition to the welfare effect on those directly employed, can influence equilibrium wage rates. In this paper we test the impact of the Indian government's major 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 249 districts across 19 Indian states, we find that on average NREG boosts the real daily agricultural wage rates by 5.3 per cent. It takes 6 to 11 months for an NREG intensity shock to feed into higher wages. The wage effect appears to be gender neutral and biased towards unskilled labour. It is positive across different implementation stages and months. It remains significant even after controlling for rainfall; district and time fixed effects; and phase-wise linear, quadratic, and cubic time trends. The validity of our identification strategy is confirmed by placebo tests. We argue that since most of the world's poor live in rural areas, and the poorest of the poor are agricultural wage labourers, rural public works constitute a potentially important anti-poverty policy tool.

JEL classification: O1

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[2] Berg: Department of Economics, University of Oxford, email: erlend.berg@economics.ox.ac.uk. Bhattacharyya: Department of Economics, University of Sussex, email: s.bhattacharyya@sussex.ac.uk. Durgam: Institute for Social and Economic Change, Bangalore, India, email: raja@isec.ac.in. Ramachandra: Institute for Social and Economic Change, Bangalore, India, email: manjula@isec.ac.in.

Centre for the Study of African Economies
Department of Economics · University of Oxford · Manor Road Building · Oxford OX1 3UQ
T: +44 (0)1865 271084 · F: +44 (0)1865 281447 · E: csae.enquiries@economics.ox.ac.uk · W: www.csae.ox.ac.uk

1. Introduction

70 per cent of the world's 1.4 billion extremely poor live in rural areas (IFAD 2011). Some are farmers tilling their own plot of 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 some of the most effective ways of improving the welfare of the poorest people on the planet.[3] Passing minimum wage legislation is practically costless to legislators, but enforcing a rural minimum wage rate is completely unrealistic in most developing countries. On the other hand, public works programmes, which employ large numbers of unskilled workers to improve public infrastructure, constitute a positive shift to labour demand. 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 works programmes would reach well beyond the people who are directly employed by them.[4]

This paper looks at a large-scale public works 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 approximately 250 Indian districts, we find that each annual person-day of employment generated by NREG per rural inhabitant in a district increases real daily agricultural wage rates by 1.6 per cent in that district. During 2008–2010, NREG annually generated approximately 3.3 person-days of employment per rural inhabitant in the average district. We conclude that, on average, NREG boosted the real daily agricultural wage rate in India by 1.6×3.3=5.3 per cent.

The idea of tying social benefits to work requirements goes back at least to pre-

[3] A large literature confirms the negative association between agricultural wages and poverty rates, particularly in India. van de Walle (1986) documents a strong negative relationship between poverty and real agricultural wage rates in India. Kijima and Lanjouw (2005) and Eswaran *et al.* (2009) are more recent examples. 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 is a trade-off that may be worth accepting.

revolutionary France, which had 'charity workshops' where the poor could receive alms in return for work. The English Poor Law of 1834 required that the poor should reside in a 'workhouse' in order to receive welfare (Himmelfarb 1984). British administrators in colonial India frequently used public works as a tool to deliver famine relief (Dreze 1990). Katz (1996) discusses work requirements to access welfare in the United States in the 19th and early 20th century. Adolf Hitler famously used public works to combat inter-war unemployment and build inter-city highways in Germany, many of which are still in use. More recently, the concept of 'workfare' was launched in the United States in the late 1960s with the same underlying idea of tying social benefits to work requirements. Subbarao (2003) provides an overview of contemporary public works programmes in Asia and Africa.

In spite of the long history of public works and workfare, 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 an *increase in productivity* related to the public goods into which the labour is invested. The labour market effect would include, but need not be limited to, an increase in wages.[6] However, the effect on wages could be muted if the public works are implemented mainly during slack agricultural seasons when the overall demand for rural labour is low, or if the scheme mainly employs workers who would otherwise be unemployed.

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.

This paper studies the effects of NREG on agricultural wages in India. 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

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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, 2011).

generation.'

We analyse the impact of NREG on agricultural wages using monthly data from about 250 districts spread across 19 Indian states in the period May 2000 to June 2011. The phased rollout of NREG across districts in 2006, 2007 and 2008 allow us to identify difference-in-difference estimates. We use the number of person-days of work provided by NREG per rural inhabitant in a district as a measure of programme intensity. After controlling for district and time fixed effects and phase-wise linear, quadratic and cubic time trends, we find that an additional per capita person-day of NREG work in a district boosts the real daily agricultural wage rate by 1.6 per cent. Since the average district intensity is 3.3 person-days of NREG employment per rural inhabitant, this implies a 5.3 per cent increase in the real daily agricultural wage rate in the average district across India. Using monthly data on intensity, we show that it takes 6 to 11 months for an NREG intensity shock to pass through to higher wages. We find that the effect is equally strong for men's and women's wages. The scheme appears to be targeted well as it mainly affects unskilled wages as opposed to skilled wages. We find that the effect of NREG on wages is the strongest in phase I and II districts, and not significant in phase III districts. We argue that this might be because phase I and II districts started from a lower level of wages, so that the statutory NREG wage rates, which are equal across all districts in a state, were likely to exert the most upward pressure there. The wage effect appears to be increasing gradually and is driven by the gradual increase in NREG intensity. Even though the effect is significant in every month of the year, we find that the magnitude is smallest in the agriculturally slack months of March and April. Finally, we confirm the validity of the identification strategy and findings using placebo tests and several robustness checks designed to overcome potential endogeneity.

Our wage data are from the Agricultural Wages in India (AWI) series which has been published by the Indian Ministry of Agriculture since 1951. It is unique in offering monthly wage rates by district, and separate wage series for several categories of labour and by gender. It remains the most widely used source for analysis of Indian rural and agricultural wages. The AWI series is discussed in detail in section 3. We deflate all wage data to constant January 2000 prices using the Consumer Price Index for Rural Labourers, which is published by state and month by the Indian Bureau of Labour. All our findings are therefore interpretable as effects on real wages.

There are several reasons why India and NREG provide a good context in which to study

the impact of public works programmes on wages. First, NREG is an enormous programme by any standards 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 will be of broad interest. Second, empirical studies of the wage effects of public works programmes are rare in part because of the difficulty associated with finding reliable wage data. The availability of good wage data at a disaggregated regional and temporal level is a great advantage of the Indian context. Third, the scheme was introduced in 2006 and extended to all of India 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.

This paper makes six contributions. First, by using district-level monthly data and a difference-in-difference estimation strategy we estimate the overall impact of NREG on real agricultural wages. This allows us to comment on the real impact of a workfare programme in a developing country on agricultural wages and hence, arguably, on poverty. Second, by looking at male and female wages separately we study the impact of the public works programme on the gender wage gap. Third, by separating out skilled wages from unskilled wages we are able to show that NREG mainly impacts on unskilled wages. We also provide estimates by skilled profession (carpenter, blacksmith and cobbler). Fourth, using monthly NREG intensity data we estimate how long it takes for the NREG labour demand shock to pass through to higher wages. Fifth, we report the heterogeneity of the wage effect across the months of the year. And sixth, we look at heterogeneous effects across districts and find that the effect of the scheme on wages depends both on the intensity with which the scheme was implemented locally and on the initial wage rates in the district.

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. 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. They present the optimal workfare programme for screening purposes and derive a sufficient condition for this to be cheaper than welfare. More recently, Basu *et al*. (2009) and Basu (2011) 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.

Even though there is a large theoretical literature on targeting in welfare and workfare, 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.[8] Ravallion *et al*. (1993) study the impact of an official wage increase in 1988 on EGS employment but 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 as we focus on 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.

Independently and concurrently with this paper, Imbert and Papp (2012) estimate the impact of NREG on wages and employment using NSS (National Sample Survey) employment and unemployment cross-sectional data. They use rounds 60, 61, 62, 64 and 66 of the NSS data as well as the quarterly 'sub-rounds' to construct their wage series. Rounds 61, 64 and 66 are

[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] Other studies of the Maharashtra EGS include Ravallion (1991) and Gaiha (1996).

quinquennial surveys known as 'thick' rounds with large sample sizes. Rounds 60 and 62 are 'thin' with a sample size less than a half of the 'thick' rounds. They find that NREG increases public works employment by 0.3 person-days per month. Their finding that casual wage income increases by 4.5 per cent aligns well with our preferred estimate of an average effect of 5.3 per cent of the programme on field labour wage rates. Our study is different from theirs in that we use wage data from AWI instead of earnings from casual employment from NSS. In addition to a larger sample and more frequent (monthly) observations, this allows us to look for a differential effect across genders. We are also able to study the wage effect by profession, the delay and duration of the pass-through of employment generated through to wage rate increases, the effect across all three programme implementation phases, and heterogeneity of the wage effect by months of the year. The merits of using AWI are discussed in more detail in section 3. Another unique feature of this paper is that in addition to the simple treatment binary we also use data on programme intensity by district, which allows us to explain some of the very substantial heterogeneity in programme effect across districts and over time.

Our study is also related to the rapidly expanding general empirical literature on NREG. 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 (2011) 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. Shankar *et al*. (2011) assess the link between information, access and delivery of the scheme in three states (Andhra Pradesh, Maharashtra and Rajasthan) and find that information increases the propensity of access by those who are not NREG's primary target. A lack of information, however, unambiguously disadvantages the poor. Datt and Ravallion (1994) look at the impact of Maharashtra's employment guarantee on the

income of participants, but do not consider the general equilibrium effect on wage rates. 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 abovementioned studies difficult to generalize. In contrast, our results are nationally representative.

The literature focusing on the implementation and operational details of NREG is rather large. It includes Aiyar and Samji (2006), Bhatia and Dreze (2006), Chakraborty (2007), CAG (2008), Ambasta *et al.* (2008), Gopal (2009), Khera and Nayak (2009), Adhikari and Bhatia (2010). Aiyar and Samji (2006) make a case for using social audits to improve the performance of NREG. Bhatia and Dreze (2006) highlight the weaknesses in the implementation of the project in Jharkhand. Chakraborty (2007) present a budgetary appraisal and the CAG (2008) report presents the Comptroller and Auditor General of India's assessment of the performance of the scheme.

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 introduces the empirical model and outlines the identification strategy. Section 5 provides results on the overall impact of NREG and NREG intensity on agricultural wages; analyses NREG's impact on the gender wage gap in agriculture; tests NREG's impact on skilled and unskilled wages; checks the delay and duration of the pass-through of generated employment to higher wages; examines heterogeneity of the estimated coefficient by season as well across districts; and provides robustness checks and placebo tests. Section 6 concludes.

2. The National Rural Employment Guarantee

The National Rural Employment Guarantee (NREG) is the latest in a line of rural public work programmes implemented in India. In order to promote employment opportunities for the poor, the Indian government has introduced nation-wide programmes such as the Drought-Prone Area Programme in the 1970s, the National Rural Employment Programme (NREP) in 1980, the Rural Landless Employment Guarantee in 1983, the Jawahar Rozgar Yojana (JRY) in the 1990s, and the Sampoorna Grameen Rozgar Yojana in 2001. The objectives and key components of these programmes were similar. First, they all focused on providing wage employment to un- and under-employed landless agricultural labourers during the slack agricultural season. Second, most of the

programmes aimed to create productive assets in the rural areas. Third, the programmes aimed to promote decentralised governance, as the responsibility of implementing them was assigned to the Gram Panchayats (GPs), the lowest tier of government.

In spite of all the precursors, the passing of the National Rural Employment Guarantee Act in 2005 was widely received as a significant event.[9] Never before a legally binding commitment to provide employment had been made by the government. The main objective of the act is '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 creates 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 for the first 30 days during the financial year and at least half of the wage rate after that.

The government has issued guidelines to supplement the act.[10] The GPs are the main implementing agency for executing NREG projects, and contractors are not allowed. The GPs are required to organise awareness camps by arranging regular town-hall style meetings (gram sabha) in every village, detailing the objectives, entitlements and procedures associated with NREG. Adult members of rural households willing to work as unskilled manual workers can register for the programme either in writing or orally to the local GP. After verification of the details provided by the applicant household, the GP is required to issue a job card within 15 days. The job card is free of cost and bears the photograph of all adult members of the household willing to work under NREG. It is also evidence that the household members are registered for the programme.

Ahead of each financial year the GPs have to conduct gram sabha meetings to identify and prioritise NREG work and prepare a list of projects. Soil and water conservation, land

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[9] The National Rural Employment Guarantee Act (NREGA) was passed in 2005. The state-wise implementations of this act are known as National Rural Employment Guarantee Schemes (NREGS). Recently, the programme was renamed the Mahatma Gandhi National Rural Employment Guarantee (MGNREG), but this paper will refer to it as NREG for the sake of brevity.

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

development, rejuvenation of traditional water bodies, social forestry, flood control and micro-irrigation are typical NREG projects. The action plan prepared by the GP requires at least 60 per cent of the NREG spending on wages and up to 40 per cent on materials. NREG prohibits the use of contractors or machinery in the implementation of the works. The action plan prepared by the GPs needs to be approved by the Zilla Parishad (District Council). [11]

The job-card holders should submit a written application for employment using the appropriate application form, clearly mentioning the time duration for which NREG work is sought. The minimum duration of employment under NREG is fourteen days. The GP issues a dated acknowledgement of the written application for employment. The GP then is responsible for providing employment within 15 days of application for work. If the GP fails to provide NREG work within that time frame, a daily unemployment allowance has to be paid.

NREG work should be provided within 5 km of the worker's village. If the work provided is beyond the 5 km radius, an extra 10 per cent of wages should be paid to meet additional transportation and living expenses. Worksites should have facilities such as a crèche, drinking water, first aid box and shade for resting periods.

NREG wages are to be paid according to either a piece rate or a daily wage rate. The rates cannot be less than minimum wages prescribed by the state government. The act stipulates that men and women should be paid equal wages. Wages are required to be disbursed on a weekly basis but not beyond a fortnight after the work has taken place. More recently, the central government has decided that the payment of NREG wages must be made by cheque or electronic transfers to bank or post office accounts.

Table 1 presents a brief summary of NREG operations in period 2008 to 2011. 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

[11] The Indian panchayat system is organized in three tiers. At the grass root level there are the Gram Panchayats (GPs) or the village level panchayats. In the middle there are the Panchayat Samitis or the block level panchayats. Finally, at the top are the Zilla Parishads or the district level panchayats. Members of all three levels of panchayat are chosen by direct elections.

this period. In 2010–11 it increased to 54.95 million which is 34 per cent of all rural households in India.

Also note that 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 took up eight NREG projects during 2010–11.

3. Measuring Real Agricultural Wages and NREG Intensity

We use data from the Agricultural Wages in India (AWI) series. The series goes back to 1951, but we only use data for the period 2000–2011. AWI is published by the Ministry of Agriculture and provides daily wage rates for a number of categories of rural labour. In the majority of cases, the source provides separate figures for men and women. AWI covers three main categories of unskilled labour: 'field labour', 'other agricultural labour' and 'herding'. 'Field labour' is in many cases further disaggregated into ploughing, sowing, weeding and reaping. In contrast, 'other agricultural labour' is not disaggregated. Examples of the kind of work included under this category are: watering fields, carrying heavy objects, digging wells, cleaning silt from waterways and embankments. We believe that a large proportion of agricultural wage labour undertaken in India would fall under the field labour category. However, the 'other agricultural', or 'non-field', category is not unimportant. NREG documentation also makes it clear that the majority of NREG work would fall under the 'other agricultural labour' category. The third category of unskilled labour in AWI is 'herding'. We choose not to use data on herding wages here because the unit of reporting varies and is not always clearly labelled. In some cases the source reports the daily wage rate whereas on other occasions it reports a piece rate per animal. Wage rates are also provided for three categories of skilled labour: carpenters, blacksmiths, and cobblers.

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 dataset developed by Ozler *et al.* (1996) draw on the AWI district-level data and aggregate them using National Sample Survey (NSS) weights to arrive at state level numbers. The notable study of the wage impact of Maharashtra Employment Guarantee Scheme (EGS) by Ravallion *et al.* (1993) also uses AWI data.

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 for skilled versus unskilled labour.

The AWI series has several advantages over NSS (National Sample Survey) data. First, NSS employment surveys only report earnings from casual labour and not agricultural wages. Second, the sample size is smaller as data are not provided monthly by district. The NSS cross-sectional surveys are conducted every one or two years which rules out the possibility of constructing a monthly wage series by district. Third, there is substantial variation in sample size across NSS rounds. NSS conducts much smaller 'thin surveys' in between the more reliable and wider five-yearly 'thick rounds'. NSS conducts quarterly sub-rounds which aggregate up to the 'thin rounds'. In some districts in a sub-round the sample size could be as small as two households. Moreover, the samples in the sub-rounds are not stratified, significantly increasing the risk of bias (National Sample Survey Organization, 2007). Fourth, NSS do not report wages by agricultural activity and therefore cannot be used to analyse the differential impact of NREG on skilled and unskilled wages.

We deflate all wage data to constant January 2000 prices using the Consumer Price Index for Rural Labourers published by the Indian Labour Bureau. All our results are therefore interpretable in terms of real wages, expressed in January 2000 prices. The Consumer Price Index Rural Labourers is available by month and state.

Our main interest is in daily 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 sub-categories (ploughing, sowing, weeding and reaping). Missing values are ignored. That is, we take the average of those sub-categories that are reported. Three states (Andhra Pradesh, Karnataka, and Maharashtra) directly report wages for general field labour as opposed to by sub-category.

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.

The non-field unskilled labour wage series ('other agricultural labour') is also provided by

gender. Here, too, we use the average of the men's and women's rates.

We construct the binary NREG treatment variable based on the phased rollout of NREG. The variable takes the value 1 if NREG is active in a particular district and the value 0 otherwise. Note that all the phase I districts are active in phases II and III. Therefore, once a district receives NREG treatment, it remains treated. The same applies to districts in phases II and III. Therefore, there are no districts in the sample receiving reverse treatment (switching from 1 to 0).

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 major states have some districts in each phase.

Since the intensity with which NREG is implemented varies across districts, regressing wages on the binary NREG treatment variable may not be enough to capture the effects of the programme. An NREG intensity variable was therefore constructed as follows. We obtained the total number of person-days of NREG work provided in a district over the period between April 2008 and March 2010. Unfortunately data before April 2008 were not available. We divide the observations by two to arrive at the annual average. Next, we divide it by the rural population in the district from the 2011 census. The resulting variable, *the annualised number of person-days of NREG employment provided per rural inhabitant*, is our measure of district-wise NREG intensity. Intensity is computed as a single number per district and does not vary over time. However, we also use time varying NREG intensity to test seasonal variation and durability of the effect. See section 5.2 for details. Our main interest is in the coefficient on the interaction of this intensity measure with the binary treatment.

The national average for the NREG intensity measure is 3.3. That is, in 2008–10 in the average district, NREG provided 3.3 days of annual employment for each rural inhabitant. Kokrajhar district of the state of Assam registers the maximum NREG intensity at 17. In contrast, Jalgaon district of the state of Maharashtra registers the lowest non-zero NREG intensity at 0.1. NREG was rolled out in Kokrajhar in phase I and In Jalgaon in phase III.

In order to avoid concerns that our results might be driven by the AWI simply reporting NREG statutory wages rather than market wages after the introduction of the scheme, we compare

reported non-field labour wages to NREG statutory wages in January–March 2009. Only seven districts reported market wages equal to NREG statutory wages. Though these are not necessarily misreported, we drop them from the sample in order to be conservative. Furthermore, in order to err on the side of caution, we drop from the data set all districts that were subject to changes in the location of the AWI data collection centre within the district in the period.

Table 2 presents descriptive statistics for the remaining 326 districts in 20 states over the period May 2000 to June 2011. The names of these states and districts are provided in Appendix 2. Even though the numbers of observations in our regressions vary due to missing values, our preferred specification in Table 4, column 5, uses 12,834 observations from 249 districts in 19 states. This is by far the largest dataset that has been used to analyse the impact of public works programmes on wages.

Table 3 reports the correlations between the various real wage series. It appears that the correlations in wages within the unskilled labour categories are relatively high. For example, the correlation between ploughing and sowing wages is 0.88, and the correlation between weeding and sowing is 0.89. The correlation between field and non-field ('other') unskilled labour is 0.61.

However, the correlations between skilled and unskilled wages are weaker. For example, that between carpentry and weeding is 0.07. Blacksmiths' wages may be an exception as this profession is more directly related to agriculture. Blacksmiths play key role in the production and maintenance of ploughshares and other agricultural tools. Also note that cobblers' wages are correlated with agricultural wages as landless agricultural labourers and cobblers could be from the same caste stock.

Figure 2 plots average wage rates across all districts in each phase in the period 2001–2010. The vertical lines represent the dates of introduction of NREG in each phase. The figure illustrates that, at the beginning of the period phase I districts on average had much lower wage rates than phase II and phase III districts. These large gaps in wage rates remained throughout the period. The persistent difference in wage rates illustrates the non-random allocation of districts to phases.

4. Empirical Strategy

The simplest method of estimating 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 a general and contemporaneous increase in wage rates. The phased rollout of the programme[12] allows us to use the districts in which NREG was already present, or not yet present, to provide information on non-NREG wage increases, so that the estimated effect of NREG on agricultural wages would be net of other effects. In order to take account of permanent differences in wage levels across districts, district fixed effects are included. The time fixed effects (one for each month between May 2000 and June 2011) control for national macro-trends and time varying common shocks affecting all districts. The basic specification (Table 4, column 2) for the difference-in-differences strategy takes the form

$$y_{it} = \alpha_i + \beta_t + \theta NREG_{it} + \varepsilon_{it} \qquad (1)$$

where, y_{it} is the natural logarithm of real daily wages in district i in month t, α_i and β_t are district and time fixed effects, $NREG_{it}$ is a binary indicator for whether or not NREG was active in district i in month t, and ε_{it} is a random error term.

If district i receives NREG treatment at time $t = k$, then the expected wages in district i before and after NREG treatment are $E(y_{ik-1}) = \alpha_i + \beta_{k-1}$ and $E(y_{ik}) = \alpha_i + \beta_k + \theta$ respectively. The simple difference between the expected wages in district i before and after NREG treatment would not be able to disentangle the effect of NREG from other contemporaneous changes in all districts.[13] It would require comparing the change in expected wage in district i with another district j which did not receive treatment at time $t = k$. For district j, the change in expected wage before and after time $t = k$ is $E(y_{jk}) - E(y_{jk-1}) = \beta_k - \beta_{k-1}$. Therefore, the effect of NREG treatment on wages is given by $[E(y_{ik}) - E(y_{ik-1})] - [E(y_{jk}) - E(y_{jk-1})] = \theta$. This is the difference-in-differences estimator. Hence, in equation (1), θ represents the impact of NREG on agricultural

[12] NREG was rolled out across the districts of India in three distinct phases. It was introduced in Phase I districts in February 2006, in phase II districts in April 2007, and in phase III districts in April 2008.

[13] Note that $E(y_{ik}) - E(y_{ik-1}) = (\beta_k - \beta_{k-1}) + \theta$ where $(\beta_k - \beta_{k-1})$ is the contemporaneous change in all districts and θ is the effect of NREG treatment.

wages. It identifies the systematic difference in agricultural wages with and without the NREG treatment after controlling for district-specific permanent unobservable and time-varying common shocks. Throughout the paper we use robust standard errors, clustered at the district level, in order to take account of serial correlation.

Our main specification in column 5 of Table 4 interacts the binary treatment variable with district intensity:

$$y_{it} = \alpha_i + \beta_t + \theta \cdot NREG_{it} \cdot Intensity_i + \gamma Rain_{it} + [Trends] + \varepsilon_{it} \qquad (2)$$

The intensity variable is the average number of person-days of NREG work provided per rural inhabitant per year in a district. The specification also controls for rainfall and phase-wise linear, quadratic and cubic trends. The phase-wise trends address the endogenous selection of districts into phases. Any fixed differences are incorporated into the district fixed effects, but the phase-wise trends take account of the possibility that wages in different phases may be on different growth paths.

Given equation (2), it is important to consider whether wages can also affect NREG intensity. However, high-wage districts are likely to be more developed with higher per capita income. Therefore, NREG work paid at a low statutory rate is likely to be less attractive for workers from developed districts. So if there is a causal link between wages and intensity, then high wages should be associated with lower intensity. In addition, a higher agricultural market wage could potentially draw labour out of NREG work, also reducing NREG intensity. Both of these potential mechanisms suggest that if there is causality running from wages to NREG intensity, then the relationship should be negative. In other words, reverse causality in equation (2) would drive the estimate of θ down towards zero. But we find a positive and significant relationship between intensity and wages.

In the main specifications we also control for rainfall in each district, using a moving average of precipitation over the previous six months. Since agriculture in India is still predominantly rain-fed; productivity, wages and demand for labour are partly determined by local rainfall. Therefore, controlling for lagged rainfall rules out an important mechanism of potential reverse causality.

The positive relationship we find in the regression above could be spurious if the intensity measure is endogenous. But in Figure 3 we plot the district-wise treatment effects of NREG,

estimated using only the binary treatment variable interacted with district fixed effects, against intensity. The relationship is positive and statistically significant at the 5 per cent level. If the potential endogenous channels discussed above were present, they would have contributed negatively. The fact that the relationship between NREG intensity and wages is positive, even when intensity is not used as an independent variable, suggests that our findings are not driven by endogeneity in the intensity measure.

Selection is also a potential source of bias. It is apparent from Figure 1 that districts were not randomly allocated to the three phases of NREG treatment. In fact, it was an explicit aim that poorer districts should receive the scheme first. 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 and the resulting estimate would be unbiased. However, it is possible that districts in the different phases did not just have different starting points but were also on a different growth path. This would introduce systematic correlation between NREG treatment and the error term and result in biased estimates. We address this concern by introducing phase-wise trends in wages as controls in our main specification (see section 5.1). We include phase-wise linear, quadratic and cubic time trends. This allows us to isolate the effect of NREG on wages while allowing the growth paths of wages to differ quite significantly between the three phases.

We also use placebo tests to address the concern that districts in different phases were on different growth paths even before NREG was introduced. Figure 4 serves as a graphical placebo test. Field labour wage rate was regressed on phase-specific monthly fixed effects, while controlling for district fixed effects, and the resulting coefficients are plotted against time. If districts in the different phases were on different growth trends before the introduction of the scheme, then this should be apparent in the left-hand side of the plot. However it appears that there was not much of a wage trend in any of the three phases before the introduction of NREG, whereas after the introduction of the scheme a gradually increasing effect on wages appears in all three phases.

A regression placebo test is presented in Table 8. The test is described in more detail in section 5.6. This test, too, is supportive of the notion that districts across different phases were not

on different growth trends prior to the introduction of the scheme.

The fact that in most of our specification, the estimate of the effect of NREG on wages does not change when controls for phase-wise trends and rainfall are introduced, is further suggestive that selection and endogeneity have been successfully accounted for.

5. Evidence

5.1 NREG and real agricultural wages: Field and non-field unskilled labour

In Table 4, we estimate the effect of NREG treatment on the daily real wage rates for field and non-field unskilled agricultural labour. Throughout, we use the logarithm of daily wage rates measured in January 2000 prices as our dependent variable. In column 1, real wages for field labour are regressed on the binary NREG treatment variable. The coefficient is significant and indicates that daily real wage rates in NREG-treated districts are approximately 6 per cent higher than the others. However, this specification does not control for permanent district-specific unobserved heterogeneity and time-varying common shocks across all districts. Hence we include district and time fixed effects in column 2 and find that the effect is no longer significantly different from zero. This is indicative of the fact that, for a given district in a given month and year, the introduction of NREG in itself may not be associated with a significant increase in wages.

However, there was large variation in the competence, conviction and vigour with which NREG was implemented across the districts. It is possible that the effect of NREG on wages depends on the intensity with which the programme was implemented. We therefore interact the binary treatment variable with a variable capturing the intensity of NREG implementation in each district. The intensity variable is constructed as the average annual number of person-days of NREG work provided per rural inhabitant in a district over the period between April 2008 and March 2010.[14] For a given district, the interaction of the binary treatment variable with the intensity measure is equal to zero until the month when NREG is introduced in the district, and

[14] Data on the number of person-days of employment generated are not available for earlier periods.

thereafter it is equal to the un-interacted intensity measure. While this introduces more variation across the districts in the treatment variable, for a given district the variable takes on only two values: zero before implementation, and the number of annual person-days of NREG work provided per rural inhabitant in the period 2008–2010 after implementation.

Note that while there is monthly variation in the intensity measure, inspection of the data suggests that reporting of this data was intermittent. Frequently there is a month in which intensity appears to be zero, only for it to catch up to the long-run average the following month. We therefore use district-wise averages of the intensity in most specifications. However, the time variation in district intensity is exploited in section 5.2.

In column 3 of Table 4, the binary treatment variable is interacted with district intensity. This specification also controls for district and time fixed effects. We find a strong positive effect of NREG intensity on real wages. On average, NREG treatment measured by NREG intensity increases agricultural daily real wage rates by about 1.7 per cent.

As discussed above, NREG treatment was non-randomly allocated across districts. Poorer district received the scheme first. This could introduce a selection bias to our estimates especially if the districts selected under different phases had different initial wage rate as well as different wage growth paths. This is accounted for in column 4 by introducing phase-wise linear, quadratic and cubic time trends.

Average rainfall in the previous six months may have a direct impact on agricultural wages, as farming in much of India is still predominantly rain-fed. If there are any remaining concerns that wages have a causal effect on intensity, it may be alleviated by controlling for a major exogenous driver of wage levels. Therefore, in our preferred model in column 5 we control for average rainfall in the last six months. We find that on average NREG treatment measured by NREG intensity boosts the agricultural field labour daily real wage rate in a given district by 1.6 per cent. In other words, an additional per capita person-day of NREG employment in a district increases the wage rate in that district by 1.6 per cent on average. Note that the average district intensity is 3.3. That is, in the average district, NREG generated 3.3 days of employment per rural inhabitant per year between 2008 and 2010. Therefore, in the average district, NREG treatment boosts the real daily agricultural wage rate by $1.6 \times 3.3 = 5.3$ per cent.

Note that the main coefficient of interest only changes marginally between columns 3, 4

and 5. This suggests that the fixed effects specification is sufficient to take account of differences in the evolution of wage rates between districts across the three implementation phases.

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 reckoned 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 NREG on casual wages, but use NSS rather than AWI data. They report that the impact on wage income is 4.5 per cent on average, which is comparable to our main estimate that the programme increase field labour wage rates by 5.3 per cent on average. However, note that they report impact on wage income whereas we look at wage rates.

In order to address endogeneity concerns, we implement the following robustness test. First, we run the following regression:

$$y_{it} = \alpha_i + \beta_t + \gamma NREG_{it} \times \alpha_i + \theta Rain_{it} + [Trends] + \varepsilon_{it} \quad (3)$$

The intensity measure is not used here, instead the binary NREG treatment variable is interacted with the district fixed effects. The model also controls for (un-interacted) district and time fixed effects; average rainfall; and phase-wise linear, quadratic, and cubic time trends.

The effect of NREG is thus allowed to vary across districts. The district-wise effects, γ, are plotted against NREG intensity in Figure 3. The best linear fit reveals a positive and significant relationship between intensity and the effect on wages. This line has a slope of 0.0094 and a p-value of 0.017. Note that the district-wise treatment effects are net of district and time fixed effects and the phase-wise trends. Since the district-wise effects were estimate without using intensity, the potentially endogenous variable, as an independent variable; and since the main potential pathways for reverse causality would indicate a negative relationship between wages and intensity; the estimated positive relationship between the district-wise treatment effect and NREG intensity is suggestive that our main findings are not driven by endogeneity in the intensity measure.

Column 6 of Table 4 looks at the impact of NREG on non-field wages. Non-field (or 'other agricultural') unskilled labour involves watering fields, carrying heavy objects, digging wells,

cleaning silt from waterways and embankments. The estimated coefficient, at 1.9 per cent, is slightly larger than that of field labour. It corresponds to an increase of 3.3×1.9=6.3 per cent in non-field labour wages in the average district.

Though non-field labour probably accounts for a much smaller proportion of overall labour input in agriculture than does field labour, it is closely aligned with the type of work that is actually provided under NREG. It is therefore plausible that the wage effect might be somewhat larger for non-field than for field labour, even though the stock of workers from which both types of unskilled agricultural labour are drawn is probably largely overlapping.

5.2 NREG and real agricultural wages: Seasonal variation and durability

Is the effect of NREG uniform over all months of the year? In order to find out, log daily field labour wage rates was regressed on the binary treatment variable interacted with NREG intensity and 12 monthly fixed effects. We also control for district and time (month-year) fixed effects, and phase-wise linear, quadratic, and cubic time trends. The coefficients on the interaction terms between NREG treatment, intensity and monthly fixed effects are plotted in Figure 5. The 95 per cent confidence intervals are indicated with vertical bars. The effect of NREG on wages is significant for every single month of the year, and not statistically significantly different from each other. Nevertheless, it is interesting to note that the point estimates are smallest in March and April. These are agriculturally slack months in India. This is perhaps reflective of the fact that in these months, private demand for labour is low, so that the upward pressure on wages exerted by NREG may be less effective.

How long does it take for a change in NREG intensity to have an impact on wages? We use monthly intensity data to address this question. As described above, monthly, district-wise data on employment generated under NREG is not available prior to April 2008. The series also suffers from apparent delays and catch-ups in reporting. We therefore do not use it in our main specifications, but we use it here to compute moving averages of NREG intensity. We regress field labour wages on lagged intensity and report the results in Figure 6. It appears to take 6 to 11 months for a shock in NREG intensity to have a positive and significant effect on wages. The effect declines thereafter and is almost zero after 18-23 months.

Note that this is the effect of a single NREG intensity shock. Since NREG intensity is

typically continuous and increasing over time, the associated pressure on wages is also rising.

5.3 NREG and the gender wage gap

The NREG act requires at least one 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 men's. 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 across gender. In Table 5, we study the impact of NREG intensity treatment 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 otherwise. NREG's impact on the gender wage gap is tested by adding an interaction term Treated × NREG intensity × female, along with the un-interacted female binary variable to our preferred specification (Table 4, column 5). If the coefficient on the interaction term is positive and significant then arguably NREG is reducing the agricultural gender wage gap.

In column 1 we consider wage rate for field labour. The coefficient on NREG intensity is positive and significant. There is also little change in the size of effect from Table 4, column 5. The coefficient on the female binary is negative, significant and has the size -0.246. This suggests that on average, female wage rates for field labour are 21.8 per cent lower than their male counterpart in the agricultural labour market. In other words, if a typical male worker gets paid Rs 100 for a day's work then his female counterpart would get Rs 78.20 for performing the same task. The coefficient on the interaction term is small and not different from zero. This suggests that the NREG treatment is gender neutral. It does not reduce the gender wage gap, but it does not increase it either.

In column 2, we consider non-field ('other agricultural labour') wage rates. The overall effects remain qualitatively the same, and here too NREG appears to be gender-neutral. However, at -0.224, corresponding to women's wages being 20.1% lower than men's, the gender gap appears to be smaller for non-field than for field labour.

5.4 NREG and skilled labour wages

The intended beneficiaries of NREG are unskilled agricultural labourers. It is therefore

natural to expect that the effect on wages should be larger for unskilled than for skilled labour. In Table 6 we look at the impact of NREG intensity on skilled wages. AWI provides wages for carpenters, blacksmiths and cobblers under the skilled labour category. These professions require more skills and training compared to agricultural field or non-field unskilled labour. Columns 1–3 estimate the impact of NREG on carpenters', blacksmiths', and cobblers' wage rates, respectively. The effect is not different from zero for carpenters and cobblers. However, the effect is positive and significant for blacksmiths. Arguably, blacksmiths, who produce and maintain essential agricultural equipment such as ploughshares, are more integrated with core agricultural activities than either carpenters or cobblers.

5.5 NREG and real agricultural wages: Effects by implementation phase

In Table 7 we estimate the impact of NREG on wages by looking at the three implementation phases separately. We create binary variables for each implementation phase and interact them with the NREG intensity variable. In column 1, we regress the field labour wage rate on the phase-wise intensities without any controls. The coefficient on phase III districts is positive and significant. The coefficient on the phase I districts is negative and significant. Since this specification does not control for district fixed effects, time fixed effects, or phase-wise time trends, these estimates can be interpreted as a confirmation that, indeed, phase III districts are better off and phase I districts are worse off, with or without NREG.

Column 2 controls for district and time fixed effects, and we find that both the phase I and phase II effects are positive and significant, whereas there is no effect in phase III districts. In column 3, we also add average rainfall and phase-wise linear, quadratic and cubic trends, but the qualitative findings remain the same. The phase III coefficient, although positive, is not significantly different from zero.

It therefore appears that NREG had a strong effect on wages in phase I and II districts, but not in phase III districts. The results seem intuitively plausible given that the initial wage rates were much higher in phase III districts (see Figure 2). It is plausible that the generally low statutory NREG wage rates may have had much less of an impact on the labour market in these districts.

These results also suggest that NREG may be contributing to convergence in wage rates

across districts, since it seems to contribute to poorer districts catching up with richer ones.

5.6 NREG and real agricultural wages: Placebo tests

As a robustness check, Table 8 presents the results of a placebo test. Recall that the first phase of NREG implementation started in February 2006. Therefore, for the placebo test, all observations from 2006 onwards are deleted. We construct a placebo treatment indicator by shifting the NREG introduction date back by three years in all districts. 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 only on the placebo treatment interacted with district intensity, and the estimated coefficient is negative and significant. This could be interpreted as confirmation that NREG was most intensely implemented in some of the worst-off districts. However, when district and time fixed effects are introduced in column 2, the estimated effect of the placebo treatment is insignificant and close to zero. When the phase-wise linear, quadratic and cubic time trends are introduced in column 3 along with rainfall, the coefficient of interest does not change much in magnitude and remains insignificant. The placebo test therefore corroborates our interpretation of the main results and suggests that the estimated coefficients are indeed caused by NREG.

Figure 4 can also be interpreted as a graphical placebo test. Wage rates were regressed on the binary treatment variable interacted with time fixed effects (one for each month-year), while controlling for district and time fixed effects and linear, quadratic and cubic phase-wise trends. The coefficients are plotted against time relative to the NREG introduction by phase, so that all phases received NREG at time 0 in this plot. If the different phases were on different trends before NREG, then this should be visible in the left-hand side of the graph. However, no such pre-scheme trend is discernible, and for all three phases the estimated effects only gradually rise above zero after NREG introduction.

6. Concluding Remarks

In this paper we study the effects of NREG on agricultural wages at the district level in India. We analyse the impact of NREG on agricultural wages using monthly data from 249 districts spread over 19 Indian states over the period May 2000 to June 2011. The three-phased

rollout of NREG in 2006, 2007 and 2008 allows us to use difference-in-differences estimation as our identification strategy. After controlling for average rainfall, district and time fixed effects and phase-wise linear, quadratic and cubic time trends, we find that NREG intensity treatment in an average district boosts real daily agricultural wage rates by 1.6 per cent. This translates into an average annual effect of NREG on real daily agricultural wages of 5.3 per cent. NREG appears to be gender neutral as we find no statistically significant difference in the wage impact of NREG on male and female wages. It also appears to be targeted well as it only affects unskilled wages and not skilled wages. The wage effect appears to be increasing gradually and is driven by the gradual increase in the NREG intensity. However, it takes 6 to 11 months for a single NREG intensity shock to have an effect on wages and the effect does not last beyond 18 months. The effect appears to be significant across all months of the year but smaller in magnitude for agriculturally slack months of March and April. We confirm the validity of our identification strategy by using placebo tests.

It is difficult if not impossible for developing country governments to enforce statutory minimum wages. Public works 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 policy instrument to reduce poverty.

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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. Consumer price index for rural labourers from the Bureau of Labour.

NREG treatment binary. Equal to 1 if a district is treated in a given month, 0 otherwise. *Source:* Government of India.

District NREG intensity. Constructed as the average annual number of person-days provided by NREG in the district in the period April 2008 – March 2010, divided by the district's rural population from the 2011 census. *Source:* Government of India.

Average Rainfall. Average rainfall over the previous six months calculated using monthly data on rainfall at the district level. *Source:* Indiastat (2000–2003); Indian Meteorological Department, Government of India (2004–2010).

Consumer Price Index. Monthly Consumer Price Index for Rural Labourers. *Source:* Indian Labour Bureau.

2. Sample districts

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
	Kamrup, 3
	Karimganj, 3
	Kokrajhar, 1
	North Lakhimpur, 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
	Yamunanagar, 3
Himachal Pradesh	
	Bilaspur, 3
	Chamba, 1

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
	Sagar, 3
	Satna, 1
	Sehore, 3
	Seoni, 1

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
	Nagaur, 3
	Pali, 3
	Rajasamand, 3
	Sikar, 3

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
Koriya, 1
Mahasamund, 2
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
Idukki, 2
Kannoor, 3
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
Phulbani, 1
Puri, 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
Burdhwan, 2
CoochBehar, 2

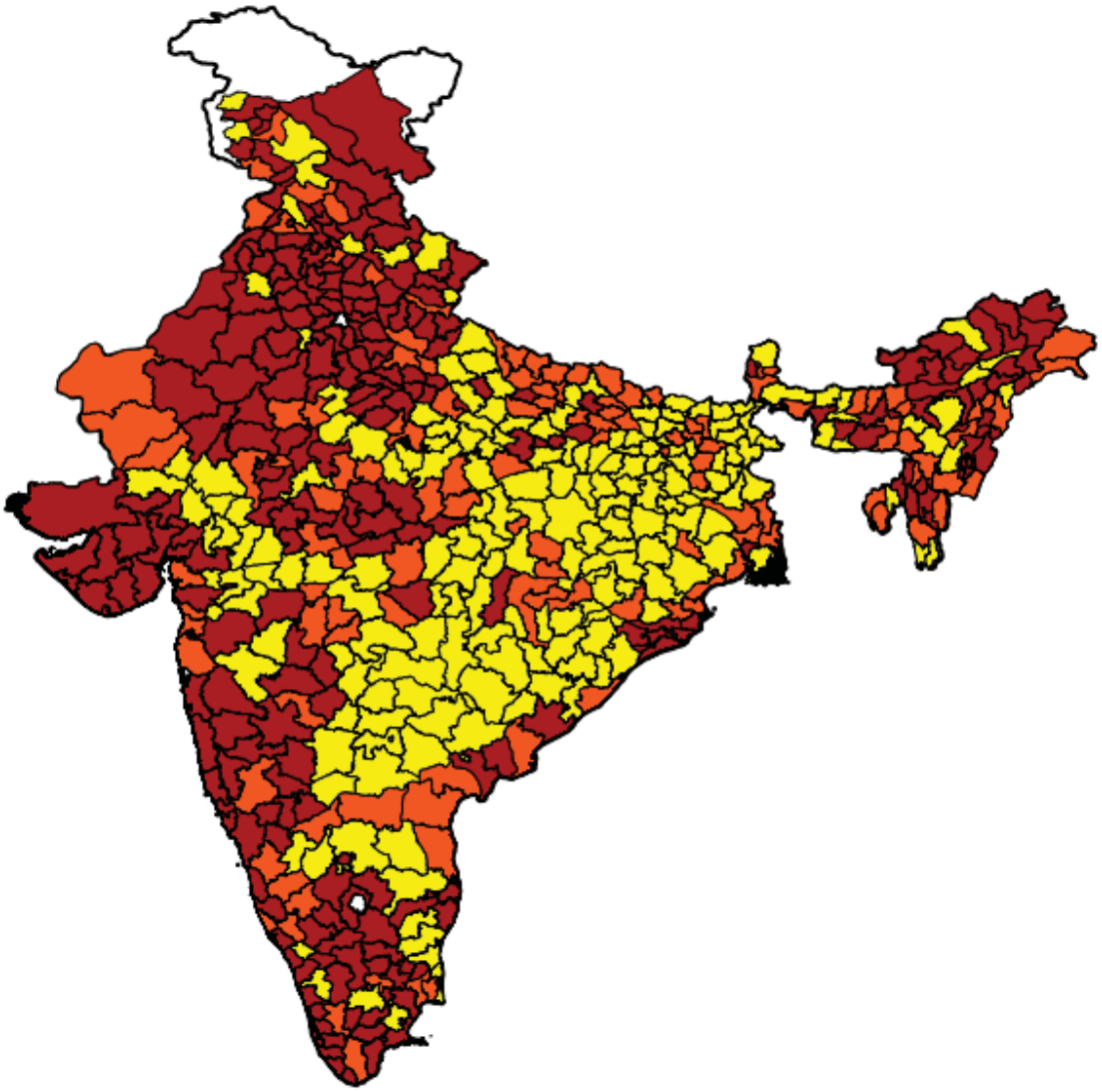
Raigarh, 1
Raipur, 2
Rajnandgaon, 1
Sarguja, 1

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

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

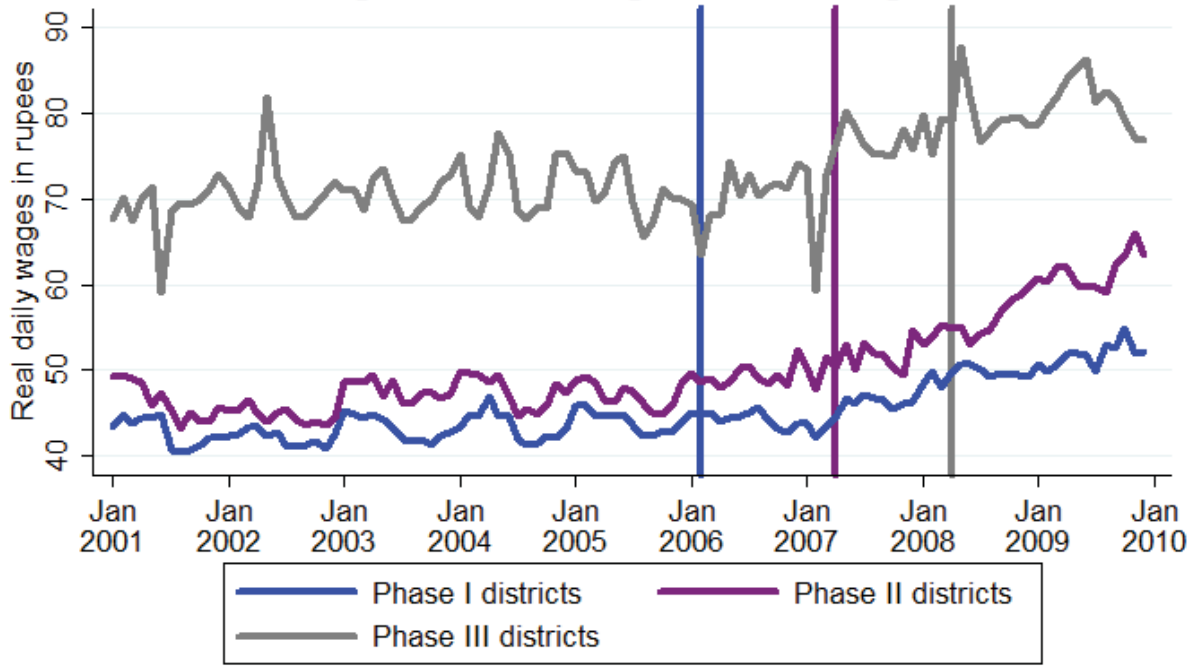
Darjeeling, 2
Hooghli, 2
Howrah, 3
Jalpaigudi, 1
Maldah, 1
Midnapur (East), 2
Midnapur (West), 1
Murshidabad, 1
Nadia, 2
North Dinajpur, 1
Purulia, 1
South Dinajpur, 1

Figure 1. District map of India



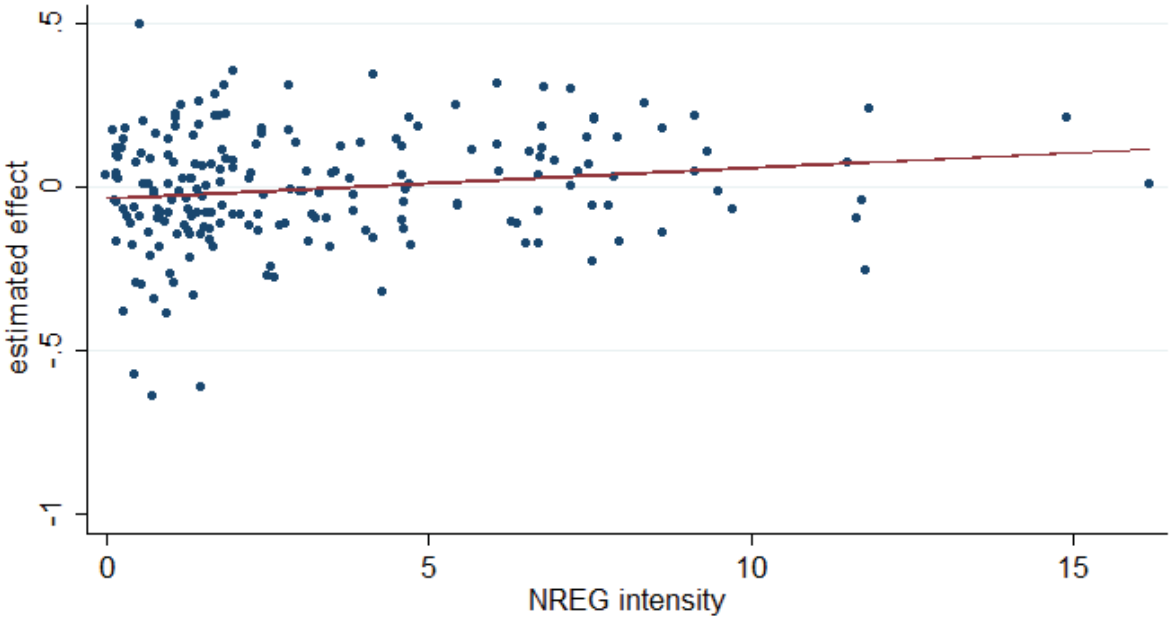
Note: The map shows all rural districts of mainland India, colour-coded according to NREG implementation phase. Phase I districts are shown in yellow, phase II districts in orange and phase III districts in brown.

Figure 2. Real agricultural wages



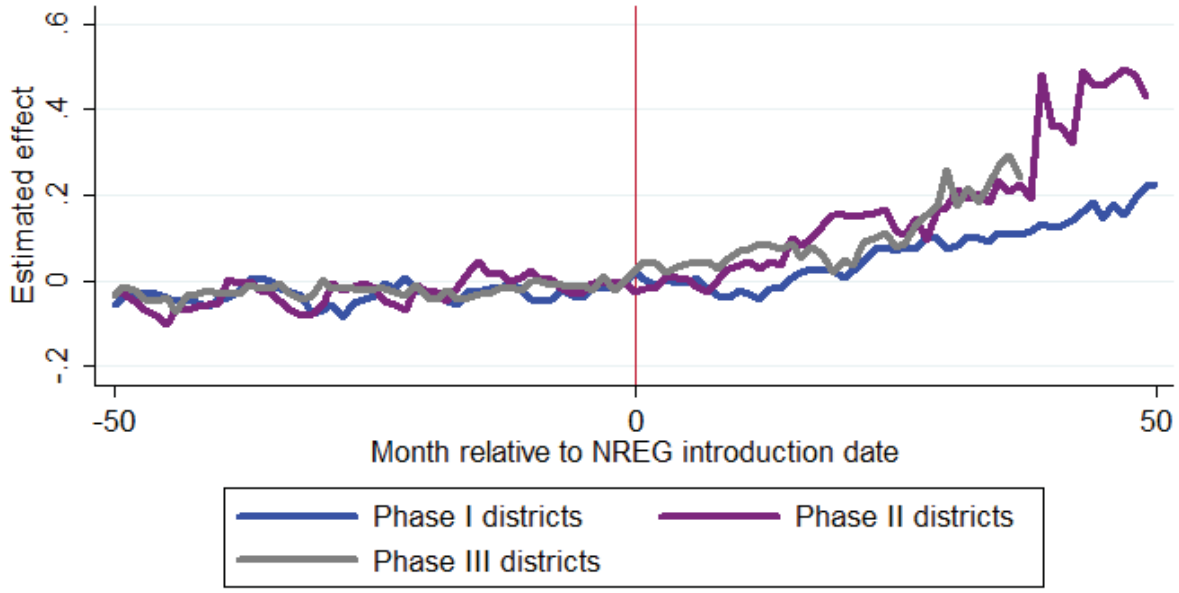
Note: The figure shows real daily field labour wages in January 2000 prices. The rates are averaged across the field labour sub-categories, across men and women and across all districts in each phase. The vertical lines show the time of introduction of NREG in each phase. Monthly observations.

Figure 3. District-wise effect of NREG on real wages



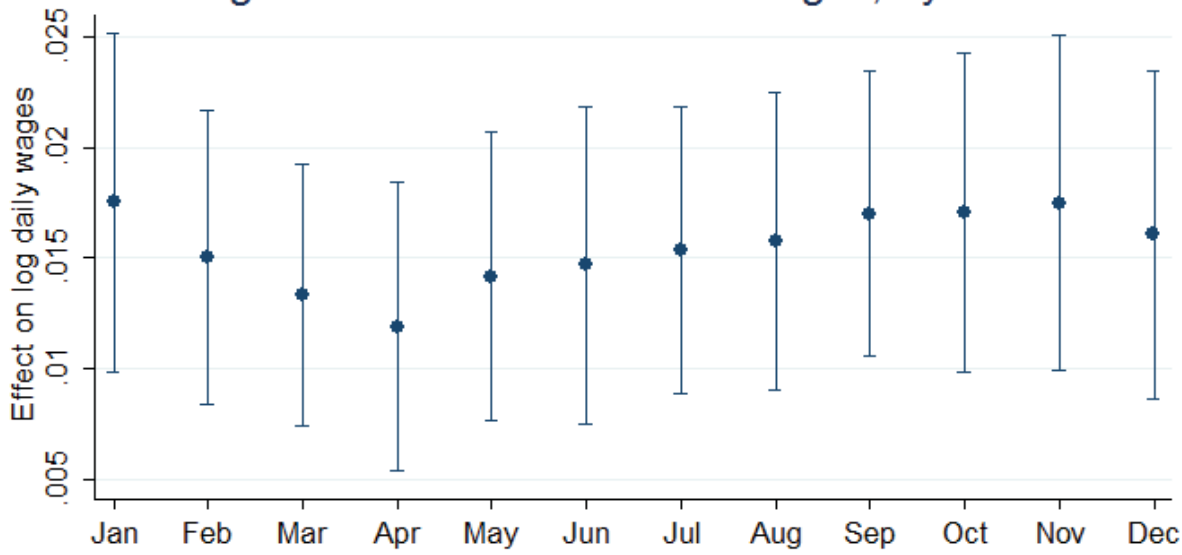
District-wise effects of NREG on field labour wages plotted against district-wise NREG intensity, and the best linear fit. The district-wise effects are the coefficients on the interaction of the treatment binary with the district fixed effects from a single regression. NREG intensity is measured as the average number of annual person-days provided under NREG per rural inhabitant in the district in the period 2008-10. The slope of the line is .0094 with a p-value of 0.017.

Figure 4. Month-wise effect of NREG on wages



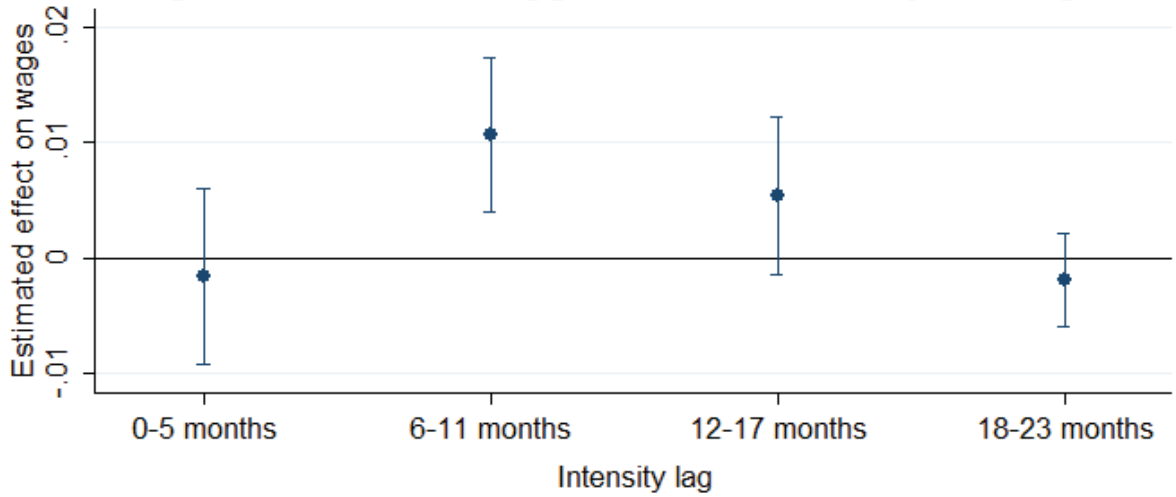
Month-wise effects of NREG on field labour wages, by implementation phase. The monthly effects are the coefficients on the interaction of time and phase fixed effects estimated in a single regression, controlling for district fixed effects. The estimated effect is expected to be constant before NREG was introduced and increase thereafter. The time scale has been shifted so that NREG is introduced at time 0 for all phases. As the levels of the coefficients are arbitrary, they have been normalised to equal zero in the month before the reform.

Figure 5. Effect of NREG on wages, by month



The graph shows the coefficients from regressing log daily field labour wages on NREG intensity (average annual person-days of NREG employment provided per rural inhabitant in the period 2008-2010) interacted with 12 monthly fixed effects. The effect is significant in every month of the year. Though the monthly coefficients are not statistically different from each other, the magnitudes of the coefficients suggest that the scheme has the smallest effect on wages in March and April, which corresponds to the agricultural off season in many parts of India. The regression also controlled for district and time fixed effects and phase-wise linear, quadratic and cubic trends. The bars represent 95% confidence intervals using robust standard errors, clustered at the district level.

Figure 6. Effect of lagged NREG intensity on wages.



The graph shows the effect of lagged NREG intensity on field labour wages. Only the second coefficient is significantly different from zero, suggesting that it takes 6-11 months for an increase in NREG intensity to feed through to higher wages. The point estimate of 0.0129 is close to that found in the main specification.
Monthly data on intensity, that is the number of days of NREG employment provided per rural inhabitant, was annualised and averaged over six-month periods. Wages were regressed on the average intensity of NREG 0-5, 6-11, 12-18 and 19-23 months before the wage observation, as well as on district and time fixed effects and linear, quadratic and cubic phase-wise trends. Standard errors were robust and clustered by district. The graph shows the coefficients on the lagged monthly intensity variables and their 95% confidence intervals.

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://164.100.12.7/Netnrega/mpr_ht/nregampr_dmu.aspx?flag=1&page1=S&month=Latest&fin_year=2010-2011
http://164.100.12.7/Netnrega/mpr_ht/nregampr_dmu.aspx?fin_year=2010-2011&month=Latest&flag=3&page1=S

Table 2. Summary statistics

	Mean	SDev	Min	Max	Obs
Unskilled labour wages, average of men's and women's rates					
Field labour	57.93	27.85	15	230	16852
Other agricultural labour	57.90	30.09	15	239	15805
Unskilled labour, wage rates by gender					
Field labour (m)	65.05	31.55	17	257	21292
- ploughing (m)	72.08	44.07	19	338	15575
- sowing (m)	67.14	33.67	19	290	13429
- weeding (m)	57.45	19.90	15	167	11413
- reaping (m)	65.68	32.28	20	270	12642
Other agricultural labour (m)	65.06	33.68	10	279	19628
Field labour (f)	51.02	23.47	13	203	16918
- ploughing (f)	50.70	12.75	16	127	502
- sowing (f)	46.03	13.01	16	135	6201
- weeding (f)	51.93	23.98	15	200	10383
- reaping (f)	56.49	30.03	14	237	10495
Other agricultural labour (f)	51.51	25.30	14	230	15824
Skilled labour wages					
Carpenter	106.31	39.33	21	292	20762
Blacksmith	90.11	35.16	19	247	17372
Cobbler	71.90	26.21	15	224	11933
NREG Measures					
District NREG Intensity	3.26	3.44	0	17	295
Rainfall data					
Monthly rainfall in district (mm)	102.65	156.94	0	1986	26191

Wages are daily rates, reported here in rupees deflated to January 2000 prices using state-wise consumer price indices, provided by district, month, labour category and sex. The two main categories of unskilled labour are field labour and 'other agricultural 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 in general 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. The 'other agricultural labour' (or 'non-field') unskilled labour category is not further broken down, but includes activities like digging and carrying. 'District NREG intensity' is constructed as the average number of person-days provided by NREG in the district per rural inhabitant per year in the period April 2008 – March 2010, divided by the district's rural population from the 2011 census. Note that 'District NREG intensity' is time invariant.

Table 3. How correlated are real wages across agricultural labour categories?

	Field labour	Ploughing	Sowing	Weeding	Reaping	Other unskilled labour	Carpenter	Blacksmith	Cobbler
Field labour	1.00								
Ploughing	0.89	1.00							
Sowing	0.95	0.88	1.00						
Weeding	0.87	0.69	0.89	1.00					
Reaping	0.72	0.38	0.55	0.62	1.00				
Other unskilled labour	0.61	0.45	0.67	0.85	0.37	1.00			
Carpenter	0.31	0.47	0.46	0.07	-0.09	0.00	1.00		
Blacksmith	0.66	0.79	0.81	0.52	0.06	0.40	0.77	1.00	
Cobbler	0.84	0.86	0.92	0.74	0.34	0.62	0.48	0.89	1.00

Table 4. NREG and real agricultural wages: Field and non-field labour

	(1) Field labour wages	(2) Field labour wages	(3) Field labour wages	(4) Field labour wages	(5) Field labour wages	(6) Non-field labour wages
Treated	0.06***	-0.01				
	(0.021)	(0.016)				
Treated × NREG intensity			0.017***	0.015***	0.016***	0.019***
			(0.003)	(0.003)	(0.003)	(0.005)
District fixed effects	No	Yes	Yes	Yes	Yes	Yes
Time fixed effects	No	Yes	Yes	Yes	Yes	Yes
Phase-wise linear, quadratic and cubic time trend	No	No	No	Yes	Yes	Yes
Average rainfall over the previous six months	No	No	No	No	Yes	Yes
Observations	16852	16852	15574	15574	12834	12009
Districts	291	291	265	265	249	221
States	20	20	20	20	19	18

The dependent variables are log daily wages in fixed January 2000 prices. Sample months are between May 2000 and June 2011. 'Treated' is a binary variable equal to 1 if NREG was active in the district at the time, and 0 otherwise. 'NREG intensity' is the average annual number of person-days provided per rural inhabitant in the period 2008–10. Standard errors, in parentheses, are robust and clustered at the district level.
* p<0.10, ** p<0.05, *** p<0.01

Table 5. NREG and the gender wage gap

	(1) Field labour wages	(2) Non-field/other labour wages
Treated × NREG intensity	0.015***	0.019***
	(0.003)	(0.005)
Treated × NREG intensity × Female	0.0003	0.0001
	(0.002)	(0.002)
Female	-0.25***	-0.22***
	(0.013)	(0.014)
District fixed effects	Yes	Yes
Time fixed effects	Yes	Yes
Phase-wise linear, quadratic and cubic time trend	Yes	Yes
Average rainfall over the previous six months	Yes	Yes
Observations	28244	26219
Districts	278	253
States	20	20

The dependent variables are log daily wages in fixed January 2000 prices, observed between May 2000 and June 2011. Standard errors, in parentheses, are robust and clustered at the district level. 'NREG intensity' is average annual number of person-days provided per rural inhabitant in the period 2008–10.
* p<0.10, ** p<0.05, *** p<0.01

Table 6. NREG and skilled labour wages

	(1) Carpenter	(2) Blacksmith	(3) Cobbler
Treated × NREG intensity	0.006	0.01**	0.005
	(0.005)	(0.005)	(0.005)
District fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
Phase-wise linear, quadratic and cubic time trend	Yes	Yes	Yes
Average rainfall over the previous six months	Yes	Yes	Yes
Observations	14760	12203	8662
Districts	274	231	177
States	20	17	14

The dependent variables are log daily wages in fixed January 2000 prices, observed between April 2000 and June 2011. Standard errors, in parentheses, are robust and clustered at the district level. 'NREG intensity' is the average annual number of person-days provided per rural inhabitant in the period 2008–10.
* p<0.10, ** p<0.05, *** p<0.01

Table 7. NREG and real agricultural wages: Phase-wise estimates

	(1) Field labour wages	(2) Field labour wages	(3) Field labour wages
Phase I binary × NREG intensity	-0.016***	0.016***	0.018***
	(0.006)	(0.004)	(0.004)
Phase II binary × NREG intensity	0.013	0.026***	0.016**
	(0.012)	(0.005)	(0.006)
Phase III binary × NREG intensity	0.061***	-0.005	0.002
	(0.019)	(0.008)	(0.008)
District fixed effects	No	Yes	Yes
Time fixed effects	No	Yes	Yes
Phase-wise linear, quadratic and cubic time trend	No	No	Yes
Average rainfall over the previous six months	No	No	Yes
Observations	15574	15574	12834
Districts	265	265	249
States	20	20	19

The dependent variable is log daily field labour wages in fixed January 2000 prices, observed between May 2000 and June 2011. Standard errors, in parentheses, are robust and clustered at the district level. 'NREG intensity' is the average annual number of person-days provided per rural inhabitant in the period 2008–10.
* p<0.10, ** p<0.05, *** p<0.01

Table 8. NREG and real agricultural wages: Placebo test

	(1) Field labour wages	(2) Field labour wages	(3) Field labour wages
Treated (placebo) × NREG intensity	-0.029*** (0.006)	0.0001 (0.002)	0.0004 (0.003)
District fixed effects	No	Yes	Yes
Time fixed effects	No	Yes	Yes
Phase-wise linear, quadratic and cubic time trend	No	No	Yes
Average rainfall over the previous six months	No	No	Yes
Observations	7655	7655	5305
Districts	189	189	173
States	18	18	16

The dependent variable is log daily field labour wages in fixed January 2000 prices, observed between May 2000 and December 2005. A placebo treatment was generated by pushing forward the NREG introduction date by three years in each district and deleting all observations from January 2006 onwards. 'NREG intensity' is the average annual number of person-days provided per rural inhabitant in the period 2008–10. The coefficients reported here are associated with the placebo treatment. Standard errors, in parentheses, are robust and clustered at the district level.
* p<0.10, ** p<0.05, *** p<0.01