Labor Market Effects of Social Programs: Evidence from India's Employment Guarantee*

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Abstract

A growing body of evidence shows that social programs, beyond their direct effect on beneficiaries, can also affect non beneficiaries, via changes in market equilibrium. We still know very little, however, about the effects of social programs on labor markets. In particular, public employment schemes, which are a popular form of poverty relief in developing countries, may crowd out private sector work and increase wages, thus redistributing income to households which are net suppliers of labor. This paper brings the first empirical evidence that these effects not only exist, but are of first order importance. Using the gradual roll out of a large rural workfare program in India, we estimate its effect on private employment and wages by comparing districts that received the program earlier relative to those that received it later. Our results suggest that public sector hiring crowds out private sector work and increases private sector wages. We use these estimates to compute the implied welfare gains of the program by consumption quintile. Our calculations show that the welfare gains to the poor from the equilibrium increase in private sector wages are large in absolute terms and large relative to the gains received solely by program participants.

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1 Introduction

Recent studies have shown that policy interventions in developing countries have important effects on non-participants. Food distribution policies affect consumer prices (Jayachandran et al., 2010), and direct cash transfers can increase the consumption of non-beneficiaries through risk-sharing networks (Angelucci and Giorgi, 2009). The impact of policy interventions on labor market equilibrium, however, has received little attention. This is despite the fact that casual labor is an important income source for the poor (Banerjee and Duflo, 2007) and that even non-labor market interventions such as cash transfers and infrastructure creation have been shown to have important impacts on labor supply (Ardington et al., 2009; Dinkelman, 2011).

The first objective of this paper is to estimate the impact of the flagship Indian anti-poverty program, the National Rural Employment Guarantee Act (NREGA), on equilibrium wages and employment. The second objective is to use the resulting estimates along with a model of rural labor markets to calculate how the welfare gains from the program are distributed across the population. We compare the gains due to the estimated equilibrium rise in wages to the gains due solely to participation in the program and find that for poor households the gains due to the equilibrium rise in wages represent a substantial fraction of the total gain from the program.

The NREGA provides a particularly good opportunity to study the labor market impacts of a large workfare program. Started in 2006, the NREGA provides short-term manual work at a wage comparable to or higher than the market rate. According to government administrative data, in 2010-11 the NREGA provided 2.3 billion person-days of employment to 53 million households making it the largest workfare program in operation today.[1] Further, the program was introduced at the district level, an administrative unit large enough to reasonably approximate a distinct labor market (Rosenzweig, 1978; Topalova, 2010).

Assessing the labor market impact of large-scale policy interventions is complicated by the fact that a plausible counterfactual for areas affected by the program rarely exists, and by the fact that even large-scale programs are often introduced within an area too small to be considered a distinct labor market.[2] We exploit the fact that the program was introduced gradually throughout India starting with the poorest districts in early 2006 and extending to the entire country by mid 2008. We estimate the impact of the program on employment

[1] Figures are from the official NREGA website `nrega.nic.in`.

[2] The well-studied Mexican Progresa program for example was rolled out at the village level (Angelucci and Giorgi, 2009).

and wages by comparing changes in outcomes in districts that received the program between April 2006 and April 2007 to those that received it after April 2008.

We show that the introduction of the workfare program is correlated with a substantial increase in low-wage, low-skilled public employment and a roughly equivalent fall in private work (waged, self employed or domestic work) among low-skilled persons. Having established an impact on private work, we document that daily wages of casual laborers increase by roughly 5.5% in early districts relative to late districts. A number of results suggest that these results are due to the program and not to pre-existing differential trends in early and late phase districts. First, both the employment and wage results are concentrated during the agricultural off-season when the majority of employment is generated by the program. Second, the results are concentrated in five "star" states that have implemented the act the best based on independent studies (Khera, 2011). Finally, average earnings for workers with salaried jobs, which are higher paying, better jobs than casual work, actually *fall* in early districts relative to late districts.

Our second objective is to use the wage and employment estimates combined with household-level data on consumption, casual labor supplied, and casual labor hired to calculate how the welfare gains from the increase in wages are distributed across rural households. An advantage of the method that we use is that it can be applied to calculate how the welfare gains from any counterfactual wage increase are distributed. We show that the rise in wages redistributes income from richer households (net buyers of labor) to poorer households (net suppliers of labor). We then use individual-level data on program wages and participation to estimate the magnitude of the direct gains from participation in the program. Our estimates show that for households in the bottom three consumption quintiles, the estimated welfare gain due to the wage change represents 20-60% of the total welfare gain from the program. Further we find that households in the richest quintile are actually made worse off due to the program as a result of the increase in wages.

The results contribute to the literature in three ways. First, we show that a particular, widely adopted anti-poverty policy has significant effects on equilibrium wages and employment. It has been suggested that government hiring may crowd out private sector work and therefore lead to a rise in equilibrium private sector wages (Ravallion, 1987; Basu et al., 2009). However, the empirical evidence on the equilibrium impacts of workfare programs is limited. The few existing studies include two concurrent studies, which confirm that the NREGA raised unskilled wages (Azam, 2012; Berg et al., 2012).

Second, we modify the theoretical framework presented in Deaton (1989) and Porto

(2006) in order to quantify the extent to which labor market equilibrium effects both benefit and hurt different segments of the population. This framework allows us to calibrate the welfare implications of a policy using empirical estimates of its aggregate impact on wages. A similar methodology could improve our understanding of the equilibrium impacts of other policy interventions in developing countries which affect labor supply (Ardington et al., 2009; Dinkelman, 2011).

Finally, the results contribute to the literature on the structure and functioning of labor markets in developing countries (Rosenzweig, 1980; Behrman, 1999) as well as the broader literature that uses the impact of policy interventions to infer how markets operate (Card and Krueger, 1992). Specifically, the almost one-for-one fall in private employment with the increase in public employment as well as the rise in wages suggest that the spot market for casual labor may be approximated by competitive labor market. If one is willing to assume that labor markets are competitive, the results allow us to estimate an elasticity of labor demand.

The following section describes the workfare program in more detail. Section 3 proposes a simple model of rural labor markets which provides a framework for estimating the distributional effects of the program. Section 4 presents our data and empirical strategy. Section 5 presents the main empirical results. Section 6 uses these results to estimate the welfare gains due to the program and Section 7 concludes.

2 The Workfare Program

The National Rural Employment Guarantee Act (NREGA), passed in September 2005, entitles every household in rural India to 100 days of work per year at a state-level minimum wage. In 2010-11 the NREGA provided 2.3 billions person-days of employment to 53 million households.[3] The India-wide budget was Rs. 345 billion (7.64 billion USD) representing 0.6% of GDP.

The act was gradually introduced throughout India starting with 200 of the poorest districts in February 2006, extending to 120 districts in April 2007, and to the rest of rural India in April 2008. The National Rural Employment Guarantee Act sets out guidelines detailing how the program is to be implemented in practice. Whether and how these guidelines are actually followed varies widely from state to state and even from district to district (Sharma, 2009; Dreze and Khera, 2009; Institute of Applied Manpower Research, 2009; The

[3]Figures are from the official NREGA website `nrega.nic.in`.

World Bank, 2011). Drawing from existing field studies, we provide an overview of how the act operates in practice.

2.1 Poverty Reduction through Employment Generation

One of the chief motivations underlying the act is poverty reduction through employment generation. In this respect, the NREGA follows a long history of workfare programs in India (see Appendix Section A). The fact that employment generation is the primary goal of the program clarifies the reasoning behind many features of the program's design and implementation. For instance, although a nominal goal of the act is to generate productive infrastructure, The World Bank (2011) writes

> "the objective of asset creation runs a very distant second to the primary objective of employment generation...Field reports of poor asset quality indicate that [the spill-over benefits from assets created] is unlikely to have made itself felt just yet."

Indeed, the act explicitly bans machines from worksites and limits material, capital and skilled wage expenditure to 40% of total expenditure. Wages paid for unskilled work are born entirely by the central government while states must pay 25% of the expenditure on materials, capital and skilled wages. Together, these restrictions create a strong incentive to select projects that require mainly unskilled labor.

2.2 Short-term, Unskilled Jobs

The work generated by the program is short-term, unskilled, manual work such as digging and transporting dirt by hand. Households with at least one member employed under the act in agricultural year 2009-10 report a mean of only 38 days of work and a median of 30 days for *all* members of the household during that year. The jobs provided by the program are similar to private sector casual labor jobs, In fact, India's National Sample Survey Office (NSSO), which collects the main source of data used in this paper, categorizes employment under the NREGA as a specific type of casual labor. Out of those who report working in public works in the past week, 46% report that they usually or sometimes engage in casual labor, while only 0.1% report that they usually or sometimes work in a salaried job.[4] The

[4] Authors' calculations based on NSS Round 66 Employment and Unemployment Survey. The Employment surveys are described in detail in Section 4.1.

similarity of these public sector jobs and casual labor jobs motivates our focus on casual wages in the empirical analysis.

2.3 Wages and Payment

Wage rates are set at the state level, and NREGA workers are either paid a piece-rate or a fixed daily wage. Under the piece-rate system, which is more common, workers receive payment based on the amount of work completed (e.g. volume of dirt shoveled). The resulting daily earnings are almost always below the state-set wage levels. Theft by officials also reduces the actual payment received.[5] Despite the fact that actual daily earnings often fall short of stipulated wage rates, NREGA work appears to be more attractive than similar private sector work available to low-skill workers. Based on a nationally representative India-wide survey during the agricultural year 2008-09, both male and female workers report earning an average of 79 Rupees per day for work under the act.[6] Reported earnings are 12% higher than the average daily earnings for casual workers (National Sample Survey Office, 2010). These figures may actually understate the attractiveness of NREGA work for the typical rural worker if search costs or other frictions drive the private sector wage rate above the marginal value of time (Walker and Ryan, 1990).

2.4 Seasonality and Cross-State Variation in Implementation

The provision of public employment varies seasonally. Local governments start and stop works throughout the year, with most works concentrated during the first two quarters of the year prior to the monsoon. The monsoon rains make construction projects difficult to undertake, which is likely part of the justification. Field reports, however, document government attempts to stop works during the rainy season so that they do not compete with the labor needs of farmers (Association for Indian Development, 2009).

The above generalizations mask considerable state and even district variation in the implementation of the program. Dreze and Khera (2009) and Khera (2011) rank Andhra Pradesh, Madhya Pradesh, Rajasthan, Tamil Nadu and Chhatisgarh as top performers, though even in these states implementation falls short of the requirements of the act.

[5] Based on a survey in the state of Orissa of 2000 individuals who show up as working in the government administrative data, only 1000 both exist and report having worked (Niehaus and Sukhtankar, 2008). Of these 1000, most received less than the stipulated minimum wage.

[6] Authors' calculations based on NSS Employment and Unemployment Survey Round 64. The Employment surveys are described in detail in Section 4.1.

Throughout the paper, we will refer to these give states as "star" states. The leading explanations for the gap in implementation between these star states and others are some combination of political will (by both the state and by the central government), existing administrative capacity, and previous experience providing public works (See Appendix B).

2.5 Impacts of the Program

Few researchers have studied the impacts of the NREGA and even fewer have studied the impact on aggregate wages and employment. As The World Bank (2011) writes:

> There is no rigorous national or state-level impact evaluation of the program, making it impossible to estimate the impact of NREGA on key parameters such as poverty, labor markets, and the local economy.

Prior to the implementation of the act, Murgai and Ravallion (2005) use a similar approach to the one used here to estimate *ex ante* the income gains from hypothetical changes in equilibrium wages, as well as income gains from participation in the program. Their approach, however, focuses solely on the gains to casual laborers and does not consider the losses to those hiring casual laborers.

Azam (2012) independently documents that the phase-in of the program correlates with an increase in rural casual wages and public works employment. His analysis focuses on the heterogeneous impact of the program by gender and finds that female wages increase by more for women than for men. In contrast to the present study, the author classifies domestic workers as inactive and shows an increase in labor force participation among women. Using a similar identification strategy but with a different series of wage data with no information on employmentBerg et al. (2012) find effects of a similar magnitude to those found here. Using survey data from 1,000 households in the state of Andhra Pradesh, Ravi and Engler (2009) estimate an increase in monthly per capita consumption for participant households on the order of 6% by comparing them to non participating households in the same villages. The results presented here suggest that non participating households also benefited from the program and hence that this estimate is biased downwards.

3 Model

In this section, we present a model with the purpose of clarifying how an increase in public sector hiring will impact aggregate employment and wages. We use the framework to trace out the equilibrium distributional impact of a workfare program across households. The model draws heavily from Deaton (1989) and Porto (2006), both of whom apply a similar framework to analyze the distributional effects of price changes. The key difference here is that we focus on the labor market rather than the market for consumption goods.

3.1 Households

Consider an economy consisting of N households indexed by i. Household i owns a production function $F_i(D_i)$ where D_i is labor used (demanded) by the household. We assume that $F_i'(\cdot) > 0$ and $F_i''(\cdot) < 0$. Households may buy or sell labor at wage W. Profits for household i are given by $\pi_i(w) \equiv F_i(D_i(W)) - WD_i(W)$ where the labor demand function $D_i(W)$ solves $F_i'(D_i(W)) = W$.

Motivated by the evidence on rationing of public works employment presented in the previous section, we assume that the government provides public works employment at wage $W_g > W$. The government must therefore determine the amount of employment to provide each household, denoted by L_i^g. Throughout, we will assume that the household uses the market wage as the relevant marginal value of private sector employment, rather than the government wage. This will be the case as long as households that work in public works also supply at least some amount of labor to the market. Given that periods of public works employment for the typical worker are quite short (often under thirty days per year), we believe this assumption to be reasonable. We discuss later the case in which the opportunity cost of time is below the market wage.

Each household has utility function $u(c_i, l_i)$ over household consumption c_i and leisure l_i. We assume the function is increasing and concave in both arguments. Households choose consumption and leisure to solve:

$$\max_{c_i, L_i} u(c_i, T - L_i)$$
$$\text{s. t. } c_i + W(T - L_i) = WT + y_i \tag{1}$$

where L_i is total (public and private) sector labor supplied by the household and non-labor income y_i is defined to be $y_i \equiv \pi_i(W) + (W_g - W)L_i^g$. Let the solution to this optimization problem for L_i be denoted by $L_i^s(w, y_i + WT)$.[7]

[7]Note that the government wage from public sector work W_g only enters through it's impact on non-labor income. This is because we assume that public works rationing is such that households that receive public works employment supply at least some private sector labor so that the marginal wage rate for households is W rather than W_g.

3.2 Equilibrium

Let aggregate labor demand be defined as the sum of the household demand functions $D(W) \equiv \sum_i D_i(W)$. Define aggregate labor supply to be the sum of the individual labor supply functions $L^s \equiv \sum_i L_i^s(W, \pi_i + WT + (W_g - W)L_i^g)$. In the subsequent analysis, we assume that both of these functions are differentiable. Because we assume $W_g > W$, the government must decide how the public works employment is to be rationed across households by choosing $\{L_i^g\}$. Aggregate public works employment is given by $L^g \equiv \sum_i L_i^g$. Labor market clearing implies that:

$$L^g + D(W) = L^s \tag{2}$$

3.3 Implications of Government Hiring

Consider a small change in L^g resulting from a small change in each of the L_i^g. In Appendix C.1, we show that given a small change in L_g, the impact on the equilibrium wage is:

$$\frac{dW}{dL^g} = \frac{1 - \sum_i \frac{dL_i^s}{dy_i}(W_g - W)\frac{dL_i^g}{dL_g}}{-D'(w) + \sum_i \left(\frac{dL_i^s}{dW}|_u + \frac{dL_i^s}{dy_i}(L_i^s - L_i^g - D_i)\right)} \tag{3}$$

where $\frac{dL_i^s}{dW}|_u$ is the substitution effect, i.e. the partial derivative of labor supply with respect to the wage holding utility constant.

We can compute the change in aggregate private sector employment as $\frac{dD}{dL^g} = D'(W)\frac{dW}{dL^g}$. This equation allows us to estimate the elasticity of labor demand using the ratio of the percentage change in the wage divided by the percentage change in employment. In Section C.5, we discuss why this ratio might not correspond to the labor demand elasticity if employers exercise market power.

From Equation 3, we see that an increase in government hiring will raise wages as long as the income effect is not too large ($\sum_i L_{y_i}^s(W_g - W) < 1$). The increase will be larger if demand is less elastic (small $-D'(W)$) or if labor supply is less elastic (small $\sum_i \left(\frac{dL_i^s}{dW}|_u + L_{y_i}^s(L_i^s - L_i^g - D_i)\right)$). The equation also reveals that the change in wages depends on how exactly the work is distributed throughout the population through the income effects. When considering how the empirical results extrapolate to other situations, it is important to keep in mind that we are observing the equilibrium impacts of a particular (non-transparent) rationing rule for government employment.

3.4 Impact on Household Welfare

Having derived the impact on wages and employment, we next turn to an analysis of the welfare effects of the program. Let the expenditure function corresponding to the dual of the utility maximization problem above be given by $e(W, u_i)$. The expenditure function gives the total income required to achieve utility level u_i given a wage rate of W. Since this is a one-period model, expenditure equals income, so we can write:

$$e(W, u_i) = \pi_i(W) + WT + (W_g - W)L_i^g + z_i \tag{4}$$

where z_i is exogenous income, $e(W, u_i)$ is the expenditure or total income required to achieve utility level u_i and $\pi_i(W) + WT + (W_g - W)L_i^g + z_i$ is total income. For fixed z_i, when L_g changes, Equation 4 will no longer hold because the expenditure required to achieve the same utility will change (the left hand side) and because the household's available income will change (the right hand side). Appendix C.2 derives the change in z_i required to maintain the equality, and therefore maintain the same utility level, following a small change in L_g:

$$\begin{aligned} -dz_i &= (L_i^s - L_i^g - D_i)W\frac{dW/W}{dL_g} + (W_g - W)dL_i^g \\ &= \textit{Net Casual Labor Earnings} \times \frac{dW/W}{dL_g} + (W_g - W)dL_i^g \end{aligned} \tag{5}$$

We interpret $-dz_i$ as the amount of money that a social planner would have to take from household i in order for the household to have the same level of utility before and after the implementation of the program. In this sense, it is a measure of the welfare effect of the program and is usually referred to as the compensating variation (Porto, 2006).[8]

3.5 Discussion

We use the above theoretical framework to interpret the empirical results and calculate the welfare impact of the program. Before we proceed to the empirical analysis, we pause to

[8]The impact on welfare is not the same as the impact on consumption. In Appendix C.3, we derive the impact of the program on consumption of household i. The key difference compared with Equation 5 is that the impact on consumption includes the change in consumption due to the income effect on the labor supply. As in Porto (2006), this term drops out in the welfare analysis due to the envelope condition since the first order condition for utility maximization implies that households are indifferent between work and leisure at the margin. As a result, the aggregate impact of the program on welfare is *not* the same as the aggregate impact on output. Aggregate output will fall by less than $L^g W$ as long as labor supply is not perfectly inelastic.

discuss some of the assumptions of the framework presented above, and how the results might change if those assumptions do not hold.

3.5.1 Worker Productivity

To the extent that the program increases wages by changing worker productivity, Equation 5 will not capture the true welfare impacts of the program. Specifically, employers will not lose from the increase in wages. Though there is limited existing evidence, the discussion in Section 2.1 suggests that the infrastructure created by the program is unlikely to have had a large effect on worker productivity during the period that we analyze. Worker productivity may have still increased through other channels. For example, the increased income due to the program may allow workers to make investments in their health leading to higher productivity (Rodgers, 1975; Strauss, 1986). To the extent that changes in wages are due to productivity changes, our framework will underestimate the welfare gains for households that hire labor.

3.5.2 Impact on Prices and Second Order Effects

Similar to the analysis in Deaton (1989), Deaton (1997), and Porto (2006), all of our results hold only for "small" increases in government employment. Large changes may have significant second order effects such as effects on output prices. For example, to the extent that the program increases the income of the poor relative to the rich, the demand for food may rise leading to a rise in food prices. A rise in food prices will disproportionately hurt the poor to the extent that they are net purchasers of food. These effects may be important and are certainly interesting, however, in the interest of making progress, we ignore them in this analysis.

3.5.3 Disguised or Under-employment

We assume throughout that the marginal value of time is given by the market wage rate W. This assumption is seemingly at odds with one of the fundamental justifications for public works schemes, which is the apparent high levels of disguised unemployment or under-employment in low-income rural areas (Datt and Ravallion, 1994). The theoretical literature has suggested a number of possible explanations for why the opportunity cost of labor might be below the private sector wage rate (Behrman, 1999).

Appendix C.4 extends the model to allow the opportunity cost of labor to be lower than the private sector wage. Similar to Basu et al. (2009), we suppose that a friction exists such that households that supply L days of labor to the labor market only receive $p_i L$ days of work.

There are three main take-ways from this extension. First, net labor buying and net labor selling households still lose or gain due to the equilibrium wage change in proportion to their net labor earnings. Second, adding unemployment to the model in this way makes clear that for some workers the marginal value of time could be less than the wage rate. In the empirical analysis later, we will assess how the transfer benefit varies under different assumptions for the marginal value of time. Finally, the impact of the workfare program on unemployment will depend critically on the nature of labor market frictions, and whether workers can work for the workfare program after they find out they will be unsuccessful in finding work. For example, if p_i reflects the fact that workers must spend the day traveling to a nearby town to search for work and find it with probability p_i, then providing an additional day of public works in the village itself will reduce unemployment by only p_i days. If workers report being unemployed because there is a temporary drop in demand for work, however, then hiring a worker through a workfare program might reduce unemployment one for one.

The labor market friction discussed here leads to a violation of the separability of household labor supply and production decisions. Although we will not test the relevance of labor market frictions in this study, it is worth noting the separability assumption has held up reasonably well to empirical tests (Benjamin, 1992).

3.5.4 Productivity Heterogeneity across Workers

One justification for workfare programs is that only workers below a certain productivity choose to participate (Besley and Coate, 1992). This effect is absent from our model since we assume that the wage is the same across all workers. We have in mind that the labor market in the model corresponds to the casual labor market. The survey data that we use in the sequel divides jobs into two broad categories, casual and salaried. Casual jobs are lower paying with a low skill premium. As discussed in Section 2.2 above, only 0.1% of workers who participate in the workfare program also report participating in salaried work in the past year, while 46% of program participants report usually or sometimes working in casual labor. If we think of the labor market in the model as only the market for casual labor, then the model already implicitly includes a substantial selection effect. In the empirical analysis, we allow for individual-level heterogeneity in wages by including controls for education, caste, and gender in the wage regressions.

4 Data and Empirical Strategy

With the theoretical framework above in mind, we next describe how we estimate the employment and wage effects of the NREGA and the data sets that we use.

4.1 Data

We use two main sources of data in the analysis: the nationally representative Employment and Unemployment survey (here on, "NSS Employment Survey") carried out by India's National Sample Survey Office (NSSO) and person-level data from the 2001 census aggregated to the district level. We use the 2001 census data to construct controls, which are described in Appendix D. For the calibration in Section 6, we use the ARIS-REDS data set, described in Appendix D.3.

Our identification relies on changes at the district level. Districts are administrative units within states. Because the workfare program is applicable only to persons living in rural areas, we drop districts that are completely urban and only use data for persons located in rural areas. Our sample includes districts within the twenty largest states of India, excluding Jammu and Kashmir. We exclude Jammu and Kashmir since survey data is missing for some quarters due to conflicts in the area. The remaining 493 districts represent 97.6% of the rural population of India. Appendix D details how we adjust the data to account for district splits and merges. The median district in our sample had a rural population of 1.37 million in 2008 and an area of 1600 square miles.

Bias due to migration is unlikely to be a major concern. Rural to rural inter-district migration for employment is limited. Out of all adults 18 to 60 with secondary education or less living in rural areas, only 0.1% percent report having migrated from a different rural district for employment within the past year. Similarly, the number of adults 18 to 60 with secondary education or less who report having migrated for employment from rural to urban areas in the past year is 0.11% of the total population of rural adults 18 to 60 with secondary education or less.[9] Low levels of migration are similarly documented in Munshi and Rosenzweig (2009) and Topalova (2010). Surveys used to measure migration may not fully capture short-term trips out of the village for work. Papp (2012) presents evidence that the workfare program studied here reduces short-term migration from rural to urban areas in a group of villages in northwest India. To the extent that this type of migration is common

[9] Authors' calculations using NSS Employment and Unemployment Survey Round 64. The Employment surveys are described in detail in Section 4.1.

throughout India, our difference-in-differences estimates presented later will underestimate the true equilibrium impact on wages.

We use five rounds of the NSS Employment Survey, each conducted from July to June in order to capture one full agriculture cycle. The survey is stratified by urban and rural areas of each district. Surveying is divided into four sub-rounds each lasting three months. Although the sample is not technically stratified by sub-round, the NSSO states that it attempts to distribute the number of households surveyed evenly within each district sub-round. We discuss in detail later the extent to which this goal is accomplished in practice. The NSSO over-samples some types of households and therefore provides sampling weights. [10] Unless otherwise stated, all statistics and estimates computed using the NSS data are adjusted using these sampling weights

The NSS Employment Survey is conducted on an irregular basis roughly every two years. We use data spanning January 2004 to December 2005 to form the pre-program period.[11] For the post-program period, we use data spanning July 2007 to June 2008. Data from July 2009 to June 2010 is also available, though at this point the program had been introduced to all districts for at least two years.

4.2 Construction of Outcomes

Our main outcomes are individual measures of employment and wages. We construct the employment measures as follows. The NSS Employment Survey includes detailed questions about the daily activities for all persons over the age of four in surveyed households for the most recent seven days. We restrict the sample to persons aged 18 to 60 with secondary education or less. We then compute for each person the fraction of days in the past seven days spent in each of four mutually exclusive activities: private sector work, public works, not in the labor force, and unemployed. Private sector work includes wage work, self-employment and domestic work. Domestic work could arguably be categorized as not in the labor force as well. However, given that most households engage in small-scale agriculture, many activities could equally well be categorized as domestic work or self-employment. In the context of the model presented in Section 3, we believe both domestic work and self-employment naturally fall under the definition of household labor.

Our wage measures are computed as follows. The NSSO makes the distinction between

[10] See National Sample Survey Organisation (2008) for more details about the sampling weights.

[11] We also have access to data from January to June 2006. However the program officially started in February 2006 and we find evidence that a pilot public works program in 150 of the initial 200 districts may have started as early as January 2006, so we leave out these six months.

two types of wage work depending on the duration and formality of the relationship with the employer: salaried work is long term and often involves a formal contract, and casual work is temporary and informal. The NSSO asks individuals who worked in casual labor over the past seven days their total earnings from casual labor. For each individual we compute the average earnings per day worked in casual labor (the "casual wage"). Similarly, we compute average earnings per day of salaried work (the "salaried wage").

Although the NSSO makes an effort to survey villages within each district throughout the year, in practice during some district-quarters no households were surveyed. Even if households were surveyed, if none of the surveyed adults worked in casual labor, we do not have a measure of wages for that district-quarter. Table A.1 presents the number of non-missing observations for each district-quarter for the employment and wage outcomes, and Appendix D provides further discussion.

4.3 Empirical Strategy

Our empirical strategy compares changes in districts that received the program earlier to districts that received the program later. The program was first introduced in 200 districts in February 2006, extended to 120 districts in April 2007, and finally to the rest of rural India in April 2008. Our analysis compares the 255 districts selected to be part of the first two phases ("early" districts) to the 144 districts which received the program in 2008 ("late" districts). We use for our pre-period January 2004 to December 2005, and for our post-period July 2007 to June 2008. The pre-period contains two full years and the post period contains one full year, so that our results are not driven by yearly seasonal fluctuations in employment and wages.

Late districts technically received the program in April 2008. We use the entire agricultural year July 2007 to June 2008 both to increase sample size and so that we can observe effects throughout the entire agricultural year. Even in the second quarter, we find a significant differential rise in public works in early relative to late districts, likely due to the fact that public works employment did not start immediately in late districts in April 2008.[12]

Early phase districts were selected to have lower agricultural wages, a larger proportion of "backward" castes and lower agricultural output per worker (Gupta, 2006). These targets were balanced with the goal of spreading early phase districts across states. As a result, some

[12]Prior to the official start date in February 2006, the government launched a pilot program known as the Food for Work Program in November 2004 in 150 of the initial 200 districts. Confirming existing field observations (Dreze, 2005), we find little evidence of an increase in public works during this pilot period.

early phase districts in richer states rank significantly better based on the three indicators than later phase districts in poorer states. Further, political considerations seem to have played some role in the selection of early districts (Gupta, 2006).

Figure 1 shows the geographic distribution of early and late districts across India. Early districts are relatively well spread out, though there is a concentration of early districts in northern and eastern India, where rural poverty is higher. Because early districts were purposefully selected based on variables that are correlated with labor market outcomes, a simple comparison of early and late districts is unlikely to be informative of the program impact. For this reason, we compare changes over time in early districts relative to late districts. Such an approach controls for time-invariant differences across districts.

These difference-in-differences estimates will be biased if outcomes in early districts are trending differentially from outcomes in late districts. We are able to partly address this concern by including controls meant to capture differential changes across districts. Our district-level controls are shown in Table 1 and include pre-program measures of poverty, literacy, population composition by caste, population density, labor force participation, workforce composition as well as land irrigation. We interact these time-invariant controls with a dummy for post-program status to pick up trends correlated with the controls. We also include time-varying controls: annual percentage deviation from average rainfall, its square, and a dummy variable for the one year preceding a state or local election.

Concern remains that program and control districts experienced differential trends uncorrelated with our controls. We present three additional specifications to explore to what extent differential trends are a concern. As discussed in Section 2.4, field studies report that employment generation due to the program is concentrated during the dry season during the first half of the year from January to May. We therefore allow the program effect to differ by half of the year. Second, as detailed in Section 2.4, wide variation exists in the extent to which states have put in place the systems required to generate the employment levels required under the act. Based on the ranking by Dreze and Oldiges (2009), we identify five "star" states, which have implemented the program better than the rest of India and compare changes within these states to the rest of India. Finally, we estimate a specification which compares early to late districts prior to the introduction of the program between 2004 and 2005.

4.4 Regression Framework

Our main results come from estimating variations of

$$Y_{idt} = \beta T_{dt} + \delta Z_d \times \mathbf{1}_{\{t>2006\}} + \gamma X_{dt} + \lambda X_{dt} \times \mathbf{1}_{\{Early\}} + \alpha H_i + \eta_t + \mu_d + \varepsilon_{idt}$$

where Y_{idt} is the outcome (e.g. earnings per day worked) for individual i surveyed in district d in quarter t, T_{dt} is a dummy equal to one for program districts in the post period (July 2007 to June 2008), Z_d are time-invariant district controls, X_{dt} are time-varying district controls, H_i are individual controls, η_t are year-quarter fixed effects, and μ_d are district fixed effects. All estimates are adjusted for correlation of ϵ_{idt} over time within districts by clustering at the district level. For many of our specifications, we include interactions of T_{dt} with other variables such as season dummies or dummies for whether the district is in a star state, in order to exploit the variation in public employment provision across seasons and states.

Because we are interested in the impact of the program on the labor market equilibrium, the relevant level of analysis is not the individual but the district. We re-weight individual observations so that the sum of all weights within a district-quarter is constant over time for each district and proportional to the rural population (see Appendix D.4 for details).

5 Results

We next present descriptive statistics for early and late districts. We then turn to our empirical estimates of the effect of the program on public employment, private sector work and wages.

5.1 Summary Statistics

Table 1 presents the pre-period means for the controls used for early and late districts as well as districts in star states and non-star states. As expected given the criteria used to choose early districts, early districts are poorer based on every measure (literacy, poverty, share of low caste population, fraction in the labor force in agriculture). Star states, on the other hand, seem to be slightly richer but more agricultural than other states, with a larger share of tribal (ST) population. Recall from Section 2.4 that star states are states identified by field studies as having implemented the program better than other states.

Table 2 presents the pre-period means for the outcomes used in the paper for early and late districts as well as districts in star states and non-star states. The allocation of days between private sector work, public sector work, unemployment and not of the labor force is similar in early and late districts. As expected given the stated selection criteria used by the government, casual labor earnings per day are 15-22% lower in program districts prior to the introduction of the program.

5.2 Change in Public Works Employment

Table 4 presents simple difference-in-differences estimates of the change in public works in early compared with late districts. Comparing 2007-08 and 2004-2005, the fraction of days spent in public works employment increases by 1.12 percentage points during the dry season in program districts. As expected, the increase during the rainy season is less than a quarter as large. The change for late districts is much smaller and insignificant. Table 4 also shows that differences in public employment provision between early and late districts persist and even widen after the program is extended to all of India by 2009-10. The lack of catch-up by late districts could reflect a learning component to implementation where districts that have the program for longer generate more employment. Alternatively, the differences could reflect differential demand for work or targeting by the government.[13]

The specifications in Table 4 gradually build to the main specification by first adding district fixed effects, then quarter fixed effects, and finally controls. The estimated impact of the program on the fraction of total time spent working in casual public employment over the whole year is 0.79 percentage points. The rise in public employment represents 0.87% of the private workforce (including domestic work). The fifth column adds an interaction between the program dummy and season. The results confirm that the rise in public works is concentrated during the dry season. The last column interacts the program dummy with a dummy for whether a district is within a star state. The results show that the increase in public works is concentrated in star states.

5.3 Change in Private Sector Employment

We divide daily activities into four mutually exclusive categories: public works, private sector work (including casual labor, salaried work, domestic work and self employment), unemployment and not in the labor force. The results for our main specification using these outcomes are presented in Table 5. The first three columns do not include controls. Without controls unemployment appears to rise in early districts relative to late districts, though including controls decreases the coefficient considerably. It appears that the rise in public

[13] The lack of catch-up by late districts is why we chose not to make use of the potential second difference-in-differences estimate comparing late districts and early districts from 2007-08 to 2009-10 in our main specification. However, the main results still hold if we include the 2009-10 data.

employment is offset by a fall in private sector work rather than time spent outside the labor force or unemployment. We cannot reject that private employment falls one-for-one with public employment generation.

Although the estimates are noisy, unemployment does not appear to fall in early districts relative to late districts. As discussed in Section 3.5.3, this could be because workers do not know they will be unemployed on a given day until they have invested the time searching or traveling to find a job. As a result, they do not have the option of choosing to work for the workfare program only on days on which they would have been unemployed. Alternatively, unemployment might not fall because the rationing mechanism is such that only workers who otherwise would have had work are allowed to participate in the program. Finally, many unemployed workers may be mis-categorized as in domestic work or self-employment, and therefore the fall in private sector work may really represent a fall in unemployment.

5.4 Change in Private Sector Wages

If labor markets were perfectly competitive then the fall in private sector work during the dry season would be matched with a rise in wages as employers moved up their demand curves. On average, adults 18 to 60 with secondary education or less spend 90% of their time in private sector work. With an elasticity of labor demand of ϵ_d, we would expect a rise in wages of $100 \times (.0158/.90)/\epsilon_d = \frac{1.75}{\epsilon_d}$ percent. In this section we present the differential trends in casual daily earnings for workers in early compared with late districts.

The first column of Table 6 presents the results for our main specification using log casual earnings per day without controls. The estimates for the dry season show that daily earnings rise by 4.1 log points more in early relative to late districts. During the rainy season, wages rise by a statistically insignificant 1.1 log points. One concern is that differential state-level trends in inflation are driving the results. The second column presents the results using log casual daily earnings deflated using a state-level price index for agricultural laborers constructed by the Indian Labour Bureau. The third column introduces the district-level controls listed in Section 4.3 and in Table 1. The rise in wages could be driven by selection if the low-wage casual laborers are more likely to work for the program. Column four show results with worker-level controls for age, caste, religion, marital status and education. The results are robust to all of these changes in specification, with the estimated effect on daily earnings in the dry season ranging from 3.6 to 5.5 log points. In the following, we use the regression with worker and district-level controls in Column Four as our preferred specification.

As discussed in Section 2.2, less than 0.1% of people who worked for the government program report also working in a salaried job in the past year. Salaried jobs are generally higher paying, regular jobs, and are considered more attractive than the work provided by the workfare program. For this reason, we may expect the program to have a limited effect on salaried wages.[14] Columns Five and Six of Table 6 present the results for the main specification with deflated log salaried wages as the outcome. The coefficient on the interaction between the dry season and program dummies is a statistically significant negative 16.5%. This result suggests that the rise in casual wages is not part of general inflation across wages of all jobs. It does raise the concern, however, that the estimated increase in casual wages may be an underestimate if the fall in salaried wages indicates a general negative demand shock for all types of labor.

Assuming that labor markets are competitive, and that changes in the wage are due to shifts along the demand curve, we can now use our estimate of the increase in the wage of 5.5% and the fall in private sector work to compute a labor demand elasticity. The elasticity of labor demand is $\epsilon_d = \frac{1.75}{5.5} = 0.32$, which is perfectly in line with the 0.25 to 0.40 range estimated by Binswanger and Evenson (1980) for farm employment in India.

5.5 Star States

We next present the changes in labor market outcomes for early districts in star states compared with the rest of India. Before turning to the results, it is important to emphasize that "star" states are by definition selected based on their implementation of the program. As a result, it is certainly possible that even conditional on controls, labor market outcomes in these states would have changed differentially absent the program. This important caveat notwithstanding, we believe documenting the trends is of interest.

Table 7 presents our main specification with the program dummy interacted with whether the district is in one of the star states as well as a dummy for the rainy or dry season. The first column shows the results for public employment. The results confirm that the field studies are correct in labeling these states as star states. In fact, there seems to be very little employment generation outside these states.

Columns Two through Four show that the fall in private sector work documented for all of India is concentrated within the early districts of star states during the dry season. The estimates are consistent with a one-for-one crowding out of private employment by public

[14] Although this argument is plausible, the program certainly could have an impact on wages for salaried workers without directly hiring them. See for example Basu (2011).

sector work. Neither unemployment nor not in the labor force seem to be affected by the program.

Column Five shows that in star states, daily casual earnings increase by a strongly significant 10% in the dry season. During the rainy season, wages increase by an insignificant 4.8%. The coefficients for other states are on the order of 1-3% and insignificant. The results are robust to adding person-level controls, which provides some assurance that the results are not driven by selection of workers.

Column Six shows that salaried earnings decrease in star states and in other states by about the same amount in the dry season (15%). This result confirms that there may be a negative demand shock which affected early phase districts contemporaneous with program implementation. However, it does not seem to have affected star states differently from the rest of India.

5.6 Pre-Program Trends in Outcomes

The differential change in employment and daily earnings documented above for early relative to late districts may represent changes unrelated to the program. The fact that the effects are concentrated during the dry season and in states where the program is best implemented suggests that the results are not driven by differential trends. However, to investigate differential trends further, Table 8 presents a similar specification to the one in Table 7 except that the sample is restricted to 2004 and 2005 prior to the introduction of the program and the program dummy is set to one for early districts in 2005. In other words, we estimate the differential changes across early and late districts prior to the program.

Reassuringly, we do not find any differential increase of public employment nor decrease in private sector employment in early relative to late phase districts prior to the implementation of the program. The point estimates for daily casual earnings are all insignificant, and very small for the dry season in star states.

Using an earlier NSS employment survey and a similar methodology, Azam (2012) finds no evidence of differential trends in casual wages between early and late districts when comparing agricultural year 1999-2000 to 2004-05. W ith a different data set of agricultural wages which covers the period from 2000 to 2009, Berg et al. (2012) not find no differential trends in casual wages between early and late districts either.

6 Estimating the Distributional Impact

The previous analysis suggests that the workfare program not only increased government work but also led to an increase in private sector casual labor wages. This change benefits net labor suppliers and hurts net labor buyers. Recall from Section 3 that the compensating differential for household i given by Equation 5 is

$$-dz_i \;=\; \textit{Net Casual Labor Earnings}_i \times \frac{dW/W}{dL_g} + (W_g - W)dL_i^g \tag{6}$$

In this section, we use the estimates from the previous section combined with pre-program household-level labor supply, household-level hired labor, program wages, program participation, and consumption to estimate the terms in this equation for different consumption quintiles in rural India.

6.1 Gains and Losses from Wage Change

The first term of Equation 6 *Net Casual Labor Earnings*$_i \times \frac{dW/W}{dL_g}$ is the change in welfare due to the equilibrium change in the wage. To estimate this term, we use 5.5% for the wage change ($\frac{dW/W}{dL_g}$) based on the estimates in Table 6

Net casual labor earnings is more difficult to estimate because in the NSS Employment Survey we only observe casual labor earnings, not payments. For this reason we turn to the 1999-00 ARIS/REDS data set, which is a nationally representative survey of households in rural India. The ARIS/REDS survey includes questions on total casual earnings as well as total payments to hired casual laborers. Appendix D.3 describes the ARIS/REDS data set in more detail.

Appendix D.3 describes the method we use to estimate net casual labor earnings in 2004-05 for each consumption quintile using the 1999-00 ARIS/REDS data. Our method allows for the possibility that some casual labor earnings reported by rural households may come from urban employers, and it allows for the fact that the total amount of casual labor payments is different in 1999-00 and 2004-05. We are forced, however, to assume that the ratio of casual labor payments to casual labor earnings for households in each consumption quintile is constant over this period. The resulting estimates of casual labor payments by consumption quintile are given in Row Seven of Table 9.

We observe casual labor earnings directly in the NSS Employment Survey, and these earnings are reported in the third row of Table 9. Net casual earnings (Row Eight) are given by total casual earnings (Row Three) less total casual payments (Row Seven). As expected, net casual earnings decrease as we move from the bottom to top quintiles. The resulting net gain from the wage change is 5.5% multiplied by net labor earnings for each quintile (Row Ten).

6.2 Direct Gains from Participation

We next turn to quantifying the second term in Equation 6. The term $(W_g - W)dL_i^g$ is the direct gain for program participants from working for the program. The welfare gain due to program participation is $(W_g - W)\Delta L_g$. Ideally, we would estimate ΔL_g using a direct measure of how many days households in each consumption quintile worked for the program. However, since we measure employment on any kind of public works and not only employment provided by the program, we instead estimate the change in public works by quintile using our main specification with the program dummy interacted with a dummy for each consumption quintile. That is, we estimate:

$$Y_{idt} = \sum_q \beta_q T_{dt} \times D_{idt}^q + \gamma X_{dt} + \lambda X_{dt} \times \mathbf{1}_{\{Early\}} + \delta Z_d \times \mathbf{1}_{\{t>2006\}} + \alpha H_i + \eta_t^q + \mu_d^q + \varepsilon_{idt} \quad (7)$$

where Y_{idt} is the fraction of time spent on public works by individual i at date t in district d. D_{idt}^q is a dummy variable equal to one if individual i belongs to consumption quintile q. Quintiles are defined separately for each year of data. T_{dt} is a dummy for program districts in the post period (July 2007 to June 2008), X_{dt} are time-varying district controls, Z_d are time-invariant district controls, H_i individual controls, η_t^q are year-quarter-quintile fixed effects, and μ_d^q are district-quintile fixed effects. [15]

The estimates of β_q for each quintile provide an estimate of increase in public works (ΔL_g) for each quintile. These estimates are presented in Row 11 of Table 9. As compared to our main specification, this method of estimating the increase in public works employment relies on the additional assumption that the composition of each consumption quintile did not change differentially in early and late phase districts and was not affected by the program. Given the short time lag between the pre and post-program periods, and given the relatively small size of the income transfer due to the program, we believe that large changes in the

[15] We also estimate the effect of the program on employment and wage outcomes for different consumption quintiles by regressing these outcomes on an interaction of the program dummy with a dummy for each quintile and by including district-quintile and time-quintile fixed effects. Regressions results are shown in Table A.3.

distribution of consumption are unlikely. To the extent that such changes exist however, our estimates only an approximate program participation across quintiles.

We estimate W_g using daily earnings for program participants. Based on the NSS 2007-08 Employment Survey, average daily earnings for program participants were 15% higher than average casual daily earnings in early districts. This figure is likely an underestimate of the initial public-private wage gap, since private wages have moved closer to the government wage as a result of the program. The estimated wage increase following program implementation between 2004-05 and 2007-8 is 5.5%. Hence, for the calibration we set the government wage to be 20% higher than the mean casual wage in 2004-05.

In our model, participants' next best option is simply the casual wage rate W. The outside option, however, could be much lower than the private sector wage rate, possibly even zero. Datt and Ravallion (1994) find that for a similar Indian workfare program in the state of Maharashtra, despite the fact that casual wages and public works wages were similar, the estimated foregone earnings from the program were only 20-30% of the earnings from the workfare program. This is likely a lower bound on the value of a participant's next best option, as it only considers productive activities.

For the calibration, we consider two extreme cases. One in which the outside option is the market wage and one in which the outside option is 50% of the market wage. The implied direct transfer $(W_g - W)\Delta L_g$ under these two assumptions is presented in Rows 14 and 15 of Table 9.

6.3 Comparing Equilibrium and Direct Gains

Figure 2 presents the estimated gain due to the change in wages, the gain due to participation in the program assuming a low outside option equal to 50% of the market wage, and the sum of the two for each quintile. For the three poorest quintiles, the equilibrium wage effect is of comparable magnitude to the gains from participation. Twenty to 30% of the total gain is do to the increase in wages. For the richest quintile, the increase in labor costs more than offsets the gains from participation resulting in a welfare loss for these households.

The numerical estimates plotted in Figure 2 are presented in Table 9. Rows 17 and 18 present the total estimated gain for each consumption quintile assuming an outside option equal to 50% and 100% of the market wage. Rows 18 and 19 show the fraction of the total gain due to the equilibrium change in wages is between 20% and 60% of the total gain, given an outside option of 50% or 100% of the private sector wage. Rows 20 and 21 show the gain as a fraction of total expenditure. Although richer households lose from the program, the impact as a fraction of total expenditures is less than one percent.

6.4 Estimating the effect of the program on consumption by quintiles

Whereas welfare is not directly measurable, we use households' expenditures to provide some empirical support for the validity of the calibration exercise. [16] In order to measure the distributional impact of the program on households' expenditures, we compare changes in expenditures in early and late phase districts for households with similar expenditure levels, using the non-linear difference-in-difference method proposed by Athey and Imbens (2006). The method consists of matching in the pre-program period (2004-5) each quintile of the distribution of households per capita expenditures in early districts to the quintile of the distribution in late districts with the same level of expenditures. We then compare changes in expenditures of each quintile in the distribution of early districts between the pre-program and the post program period (2007-8) to changes in expenditures of the corresponding quintile in the distribution of late districts.

The results are shown in Table A.4. As compared to households with similar level of expenditures in late phase districts, households in early phase districts experienced a *fall* in real per capita expenditures of about 7.5% on average between 2004-5 and 2007-8. This may be due to a negative income shock (for example a bad harvest) which affected more households in early than in late phase districts: we found some of evidence of this in the fall of salaried wages.[17] Interestingly, poorest households fared relatively better than richest households: per capita expenditures fall by less than 3% for households in the poorest quintile and by 10% for households in the richest quintile. Our calibration predicted a gain of 5% for the poorest quintile and a loss of 1% for the richest quintile: these results suggest that absent the program, poorest and richest quintile would have experienced a similar drop in consumption of about 8 to 9%.

[16] It is a very crude approximation, given that consumption only represents one dimension of welfare (Section C.3 discusses this point in more detail).

[17] As opposed to earlier specifications, the non linear difference in difference estimated here does not include any time varying control, such as rainfall.

7 Discussion

This paper provides some of the first evidence on the equilibrium impacts of workfare programs in a developing country context. Like many social programs in developing countries, workfare programs involve a transfer to the rural poor funded by (mostly urban) taxpayers. We show that through their effect on labor markets, workfare programs trigger a redistributive effect within rural areas, from households which are net labor buyers to households which are net labor sellers. Anecdotal evidence suggests that farmers have opposed the implementation of the scheme during the peak season of agriculture precisely because of its effect on wages (Association for Indian Development, 2009). These political economy considerations could explain the poor implementation of the program in some of the poorest states of India (Bihar, Jharkhand, West Bengal), despite the large potential demand for public employment.

The results in this paper shed light on the structure of rural labor markets in developing countries. Different researchers have suggested that casual labor markets in developing countries and India in particular are usefully characterized by monopsonistic competition on the part of employers (Binswanger and Rosenzweig, 1984), the neoclassical model of labor markets (Rosenzweig, 1980), and even by a model in which workers hold the market power (Osmani, 1991), among many other models. Our results appear consistent with a neoclassical model of labor markets. Specifically, the hiring of workers by the government leads to a reduction in time spent working by households and an increase in wages consistent with previous estimates of the elasticity of demand for labor. As a result, our findings appear consistent with Rosenzweig (1980)'s finding that at least at a macro level, casual labor markets are roughly approximated by the neoclassical model of labor markets.

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Figure 1: Map of Early and Late Districts

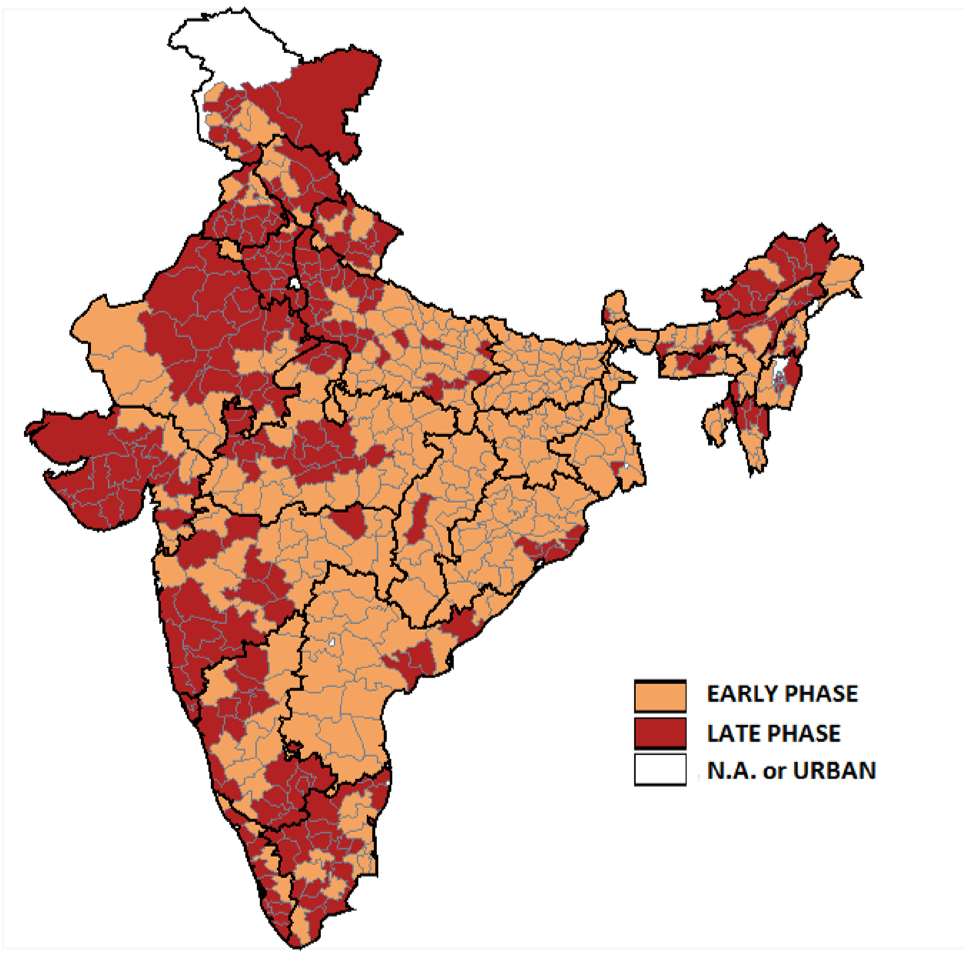
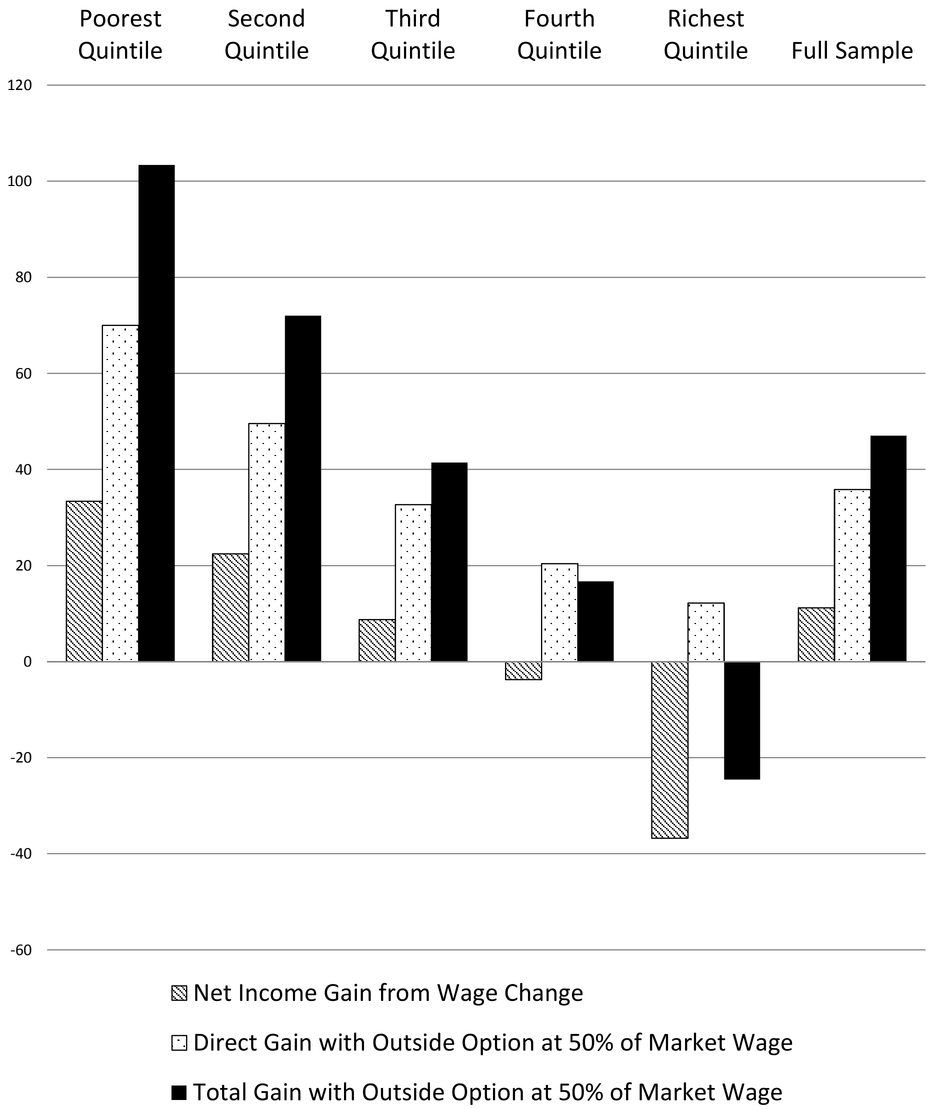


Figure 2: Welfare Gains by Expenditures Quintiles



Note: Welfare gains are expressed in Indian Rupees, per household per month. Net Income Gain from Wage change is obtained by multplying the average net casual labor earnings in each consumption quintile by the wage increase estimated in Table 6. Direct Gain with Outside Option at 50% of Market Wage is obtained by multplying the increase in time spent on public works by prim-age adults in the dry season for each consumption quintile estimated in Table A3 by the average number of prime age adult s per household and by the difference between the average wage on public works and 50% of the average market wage. Total Gain with Outside Option at 50% of Market Wage is the sum of the first two figures for each quintile. See Table A4 for more details.

Table 1: District Controls Summary Statistics

	Early	Late	Difference (1) - (2)	Star States	Other States	Difference (4) - (5)	Source	Time-varying?
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Literacy Rate	0.553	0.646	-0.093***	0.581	0.590	-0.009	2001 Census	No
Fraction SC	0.187	0.174	0.013*	0.182	0.182	-0.001	2001 Census	No
Fraction ST	0.134	0.049	0.085***	0.150	0.083	0.067***	2001 Census	No
Poverty Rate	0.321	0.210	0.111***	0.227	0.301	-0.074***	NSS	No
Population Density (per sq. km)	485	399	85***	238	540	-301.871***	2001 Census	No
Irrigated Cultivable Land per Capita (ha)	0.082	0.119	-0.037***	0.118	0.087	0.031***	2001 Census	No
Non irrigated Cultivable Land per Capita (ha)	0.174	0.177	-0.002	0.241	0.148	0.093***	2001 Census	No
In South	0.198	0.279	-0.081**	0.493	0.121	0.372***		No
Female Labor Force Participation Ratio	0.378	0.369	0.008	0.503	0.323	0.18***	2001 Census	No
Male Labor Force Participation Ratio	0.635	0.630	0.004	0.662	0.621	0.041***	2001 Census	No
Fraction Ag Casual Laborers	0.197	0.164	0.033***	0.236	0.164	0.072***	NSS	No
Fraction Non-Ag Casual Labor	0.047	0.065	-0.018***	0.061	0.051	0.01**	NSS	No
Fraction Cultivators	0.274	0.254	0.02*	0.319	0.245	0.074***	NSS	No
Fraction Non-Ag Business	0.091	0.090	0.002	0.087	0.092	-0.005	NSS	No
Fraction Salaried Work	0.046	0.072	-0.026***	0.060	0.054	0.007*	NSS	No
Fraction Labor Force in Agriculture	0.757	0.668	0.089***	0.776	0.703	0.073***	2001 Census	No
Annual Rainfall	0.149	0.177	-0.028	0.116	0.178	-0.062	IMD	Yes
Annual Rainfall (square)	0.611	0.851	-0.24**	0.716	0.694	0.022	IMD	Yes
Election Year	0.411	0.329	0.081*	0.349	0.393	-0.045		Yes
Number of Districts	286	207		143	350			
Number of Individual Observations	274,877	166,958		120,901	320,934			

This table presents means of the controls used in the paper for different samples. Column (1) is restricted to districts that received the workfare program prior to April 2007. Column (2) includes only districts that received the program after April 2007. Column (4) restricts the sample to star states. Star states are identified by field reports as having implemented the administrative requirements of the act particularly well. Star states include Andhra Pradesh, Madhya Pradesh, Tamil Nadu, Rajasthan, and Chhatisgarh. Column (5) includes districts in non-star states. With the exception of the poverty rate, controls constructed using NSS use data from Rounds 60, 61, and 62 from Jan 2004 to December 2005 of the Employment survey. The poverty rate is constructed using Round 61 of the NSS Consumer Expenditure survey. Employment variables from the NSS are computed using the reported usual activity during the past year for adults 18 to 60 only. Literacy and labor force participation are restricted to persons over the age of six. Annual rainfall is expressed as the percentage deviation from the average rainfall for that district from 1975 to 2010. Election year is a dummy variable indicating that state or local (village) elections are to be held in the following year. The standard errors of the differences in columns (3) and (6) are computed assuming correlation of individual observations over time within each district. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 2: Summary Statistics of Outcomes in 2004, 2005 for Early and Late Districts

Panel A: Men

	Early (1)	Late (2)	(1) - (2) (3)	Star States (4)	Other States (5)	(4) - (5) (6)
Public Work (Casual)	0.002	0.001	0.001	0.004	0.001	0.003
Private Work	0.867	0.851	0.016	0.850	0.866	-0.016
Cultivator	0.378	0.346	0.031	0.372	0.363	0.009
Non-Ag Self-employed	0.139	0.141	-0.002	0.120	0.148	-0.028
Casual Labor	0.258	0.247	0.011	0.261	0.251	0.011
Salaried Work	0.053	0.087	-0.035	0.067	0.065	0.002
Domestic Work	0.016	0.010	0.006	0.008	0.016	-0.008
Unemployed	0.068	0.078	-0.01	0.084	0.067	0.016
Not in Labor Force	0.062	0.069	-0.007	0.062	0.066	-0.004
Log Daily Casual Earnings	3.834	4.075	-0.241***	3.909	3.929	-0.02
Log Daily Salaried Earnings	4.386	4.375	0.011	4.228	4.446	-0.218***
Number of Individual Observations	44,859	29,834		20,682	54,011	

Panel B: Women

	Early (1)	Late (2)	(1) - (2) (3)	Star States (4)	Other States (5)	(4) - (5) (6)
Public Employment (Casual)	0.001	0.001	0	0.003	0.000	0.003
Private Sector Work	0.939	0.936	0.003	0.913	0.948	-0.035
Cultivator	0.182	0.215	-0.033	0.256	0.169	0.087**
Non-Ag Self-employed	0.040	0.038	0.002	0.059	0.031	0.028
Casual Labor	0.107	0.104	0.002	0.150	0.088	0.062**
Salaried Work	0.012	0.018	-0.005	0.018	0.013	0.006
Domestic Work	0.581	0.547	0.035	0.411	0.632	-0.221***
Unemployed	0.029	0.035	-0.005	0.051	0.024	0.027*
Not in Labor Force	0.030	0.028	0.002	0.032	0.028	0.004
Log Daily Casual Earnings	3.430	3.559	-0.129***	3.435	3.508	-0.074*
Log Daily Salaried Earnings	3.557	3.634	-0.077	3.399	3.709	-0.31***
Number of Individual Observations	51,025	33,269		23,406	60,888	

This table presents means of the main outcomes used in the paper for different samples. All samples are restricted to persons aged 18 to 60 with secondary education or less. Column (1) is restricted to districts that received the workfare program prior to April 2007. Column (2) includes only districts that received the program after April 2007. Column (4) restricts the sample to star states. Star states are identified by field reports as having implemented the administrative requirements of the act particularly well. Star states include Andhra Pradesh, Madhya Pradesh, Tamil Nadu, Rajasthan, and Chhatisgarh. Column (5) includes districts in non-star states. The standard errors of the differences in columns (3) and (6) are computed assuming correlation over time within districts. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 3: Public Works Difference-in-Differences Estimates by Implementation Group

		Early Districts		Late Districts		Diff-in-Diff	
		Rainy	Dry	Rainy	Dry	Rainy	Dry
		Jul to Dec	Jan to Jun	Jul to Dec	Jan to Jun	Jul to Dec	Jan to Jun
		(1)	(2)	(3)	(4)	(5)	(6)
						(1) - (3)	(2) - (4)
(1)	Pre (1/04 to 12/05)	0.0011	0.0026	0.0008	0.0027		
		(0.0003)	(0.0005)	(0.0002)	(0.0007)		
		N=71674	N=101946	N=46943	N=64142		
(2)	Post (2007-08)	0.0034	0.014	0.0009	0.004		
		(0.0006)	(0.0025)	(0.0004)	(0.001)		
		N=50721	N=50536	N=28116	N=27757		
(3)	(2) - (1)	0.0023***	0.0114***	0.0002	0.0013	0.0021***	0.0101***
		(0.0006)	(0.0025)	(0.0004)	(0.0012)	(0.0008)	(0.0028)
(4)	Post (2009-10)	0.0089	0.0179	0.006	0.0118		
		(0.0014)	(0.0032)	(0.0014)	(0.0029)		
		N=33638	N=33374	N=22248	N=22003		
(5)	(4) - (2)	0.0055***	0.0038	0.0051***	0.0078***	0.0003	-0.004
		(0.0077)	(0.0153)	(0.0053)	(0.0091)	(0.0025)	(0.0062)

The sample is composed of adults aged 18 to 60 with secondary education or less. Each cell is a mean of the variable public works, with standard errors in parentheses and the number of individual observations below. Public works is an estimate of the fraction of days spent working in public works employment. For example, row (1), column (2) is the mean of public works for all districts that received the program prior to April 2007 (early districts) with the sample restricted to the first six months (dry season) of 2004 and 2005. 2007-08 and 2009-10 correspond to agricultural years (July to June). The public works program was introduced in early districts between February 2006 and April 2007. The program was introduced to late districts in April 2008. All means are computed using sampling weights. Standard errors are adjusted for correlation of the errors at the district level, ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 4: Main Specification Public Works

	Public Works (1)	Public Works (2)	Public Works (3)	Public Works (4)	Public Works (5)	Public Works (6)
Program	0.00680***	0.00678***	0.00611***	0.00791***		
	(0.00140)	(0.00139)	(0.00153)	(0.00205)		
Program X Dry					0.0112***	
					(0.00300)	
Program X Rainy					0.00424***	
					(0.00142)	
Program X Dry X Star States						0.0341***
						(0.00787)
Program X Rainy X Star States						0.00610***
						(0.00203)
Program X Dry X Other States						-6.76e-05
						(0.00160)
Program X Rainy X Other States						0.000474
						(0.00128)
Observations	441,835	441,835	441,835	441,835	441,835	441,835
R-squared	0.003	0.021	0.023	0.025	0.025	0.031
District FE	No	Yes	Yes	Yes	Yes	Yes
Quarter*Year FE	No	No	Yes	Yes	Yes	Yes
District Controls	No	No	No	Yes	Yes	Yes

Each column presents results from a separate regression. The sample is composed of all adults aged 18 to 60 with secondary education or less interviewed from January 2004 to December 2005 and from July 2007 to June 2008. Public works is an estimate of the fraction of total time spent on public works. Program is a dummy variable equal to one for early districts during July 2007 to June 2008. Dry is a dummy variable equal to one for the first two quarters of the year. Rainy is a dummy variable equal to one for the second two quarters of the year. All estimates are computed using weights proportional to district population. District controls are listed in Table 1. District controls that do not vary over time are interacted with a dummy for 2007-08 (post-program). All estimates are computed using sampling weights. Standard errors in parentheses are adjusted for correlation of the errors at the district level. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 5: Main Specification Time Allocation

	Public	Private	Unemployed	Not in Labor Force
	(1)	(2)	(3)	(4)
Program X Dry	0.00991***	-0.0234***	0.0158***	-0.00230
	(0.00272)	(0.00613)	(0.00412)	(0.00313)
Program X Rainy	0.00230***	-0.00576	0.00736*	-0.00390
	(0.000833)	(0.00488)	(0.00424)	(0.00256)
Observations	441,835	441,835	441,835	441,835
District Controls	No	No	No	No
	Public	Private	Unemployed	Not in Labor Force
	(5)	(6)	(7)	(8)
Program X Dry	0.0112***	-0.0158**	0.00671	-0.00219
	(0.00300)	(0.00663)	(0.00431)	(0.00354)
Program X Rainy	0.00424***	0.00285	-0.00281	-0.00428
	(0.00142)	(0.00610)	(0.00485)	(0.00319)
Observations	441,835	441,835	441,835	441,835
District Controls	Yes	Yes	Yes	Yes

Each column presents results from a separate regression. All regressions include district and year-quarter fixed effects. The sample is composed of all adults aged 18 to 60 with secondary education or less interviewed from January 2004 to December 2005 and from July 2007 to June 2008. Private, unemployed, and not in the labor force are estimates of the fraction of total time spent working in private sector work (including domestic work), unemployed or not in the labor force. Program is a dummy variable equal to one for early districts during July 2007 to June 2008. Dry is a dummy variable equal to one for the first two quarters of the year. Rainy is a dummy variable equal to one for the second two quarters of the year. All estimates are computed using weights proportional to district population. District controls are listed in Table 1. District controls that do not vary over time are interacted with a dummy for 2007-08 (post-program). All estimates are computed using sampling weights. Standard errors in parentheses are adjusted for correlation of the errors at the district level. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 6: Main Specification Daily Earnings

	Log Daily Casual Earnings	Log Deflated Daily Casual Earnings	Log Deflated Daily Casual Earnings	Log Deflated Daily Casual Earnings	Log Deflated Daily Salaried Earnings	Log Deflated Daily Salaried Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Program X Dry	0.0414**	0.0359**	0.0536***	0.0548***	-0.134**	-0.165***
	(0.0178)	(0.0182)	(0.0186)	(0.0175)	(0.0643)	(0.0555)
Program X Rainy	0.0112	0.00402	0.0198	0.0276	-0.0111	0.000635
	(0.0176)	(0.0184)	(0.0195)	(0.0185)	(0.0664)	(0.0544)
Observations	85,508	85,508	85,508	85,452	17,378	17,370
District Controls	No	No	Yes	Yes	Yes	Yes
Worker Controls	No	No	No	Yes	No	Yes

Each column presents results from a separate regression. All regressions include district and year-quarter fixed effects. The sample is composed of all adults aged 18 to 60 with secondary education or less interviewed from January 2004 to December 2005 and from July 2007 to June 2008. Log daily casual earnings is the log of earnings per day worked for people who report working in casual labor. Daily salaried earnings are earnings from salaried work, which tend to be higher-paying longer-term jobs. Deflated earnings are deflated using the monthly, state-level price index for agricultural labourers from the Indian Labour Bureau. Program is a dummy variable equal to one for early districts during July 2007 to June 2008. Dry is a dummy variable equal to one for the first two quarters of the year. Rainy is a dummy variable equal to one for the second two quarters of the year. District controls are listed in Table 1. District controls that do not vary over time are interacted with a dummy for 2007-08 (post-program). All estimates are computed using sampling weights. Standard errors in parentheses are adjusted for correlation of the errors at the district level. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 7: Program Effects by Implementation Group

	Public	Private	Unemployed	Not in Labor Force	Log Deflated Casual Daily Earnings	Log Deflated Salaried Daily Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Program X Dry X Star States	0.0341***	-0.0358***	0.00282	-0.00109	0.103***	-0.152**
	(0.00787)	(0.0117)	(0.00692)	(0.00540)	(0.0252)	(0.0735)
Program X Rainy X Star States	0.00610***	-0.00163	-0.00126	-0.00321	0.0478*	-0.0666
	(0.00203)	(0.00858)	(0.00702)	(0.00462)	(0.0254)	(0.0842)
Program X Dry X Other States	-6.76e-05	-0.00564	0.00850*	-0.00278	0.0305	-0.167***
	(0.00160)	(0.00652)	(0.00479)	(0.00380)	(0.0191)	(0.0623)
Program X Rainy X Other States	0.000474	0.00760	-0.00320	-0.00488	0.0128	0.0296
	(0.00128)	(0.00651)	(0.00518)	(0.00359)	(0.0201)	(0.0590)
Observations	441,835	441,835	441,835	441,835	85,452	17,370
District Controls	Yes	Yes	Yes	Yes	Yes	Yes
Worker Controls	No	No	No	No	Yes	Yes

Each column presents the results of a separate regression. All regressions include district and year-quarter fixed effects. The sample is composed of all adults aged 18 to 60 with secondary education or less interviewed from January 2004 to December 2005 and from July 2007 to June 2008. The unit of observation is a person. The outcomes are defined as in Table 4, 5 and 6. Program is a dummy variable equal to one for early districts during July 2007 to June 2008. Dry is a dummy variable equal to one for the first two quarters of the year. Rainy is a dummy variable equal to one for the second two quarters of the year. District controls are listed in Table 1. District controls that do not vary over time are interacted with a dummy for 2007-08 (post-program). Worker controls are listed in the notes of Table 6. Star states is a dummy variable equal to one for districts within star states. Other states is a dummy variable equal to one for districts that are not in star states. See Table 2 for a description of star states. All estimates are computed using sampling weights. Standard errors in parentheses are adjusted for correlation of the errors at the district level. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 8: Pre-existing Trends

	Public	Private	Unemployed	Not in Labor Force	Log Deflated Daily Casual Earnings	Log Deflated Daily Salaried Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Program X Dry X Star States	0.00432	-0.00879	0.00797	-0.00350	0.00519	0.0697
	(0.00331)	(0.00899)	(0.00690)	(0.00488)	(0.0241)	(0.0844)
Program X Rainy X Star States	0.00243	0.0187	-0.0144*	-0.00666	0.0526	0.0549
	(0.00234)	(0.0118)	(0.00827)	(0.00694)	(0.0326)	(0.102)
Program X Dry X Other States	-0.00207*	-0.00196	0.00617	-0.00215	0.0225	0.0811
	(0.00125)	(0.00670)	(0.00500)	(0.00397)	(0.0195)	(0.0646)
Program X Rainy X Other States	-0.000579	0.00410	-0.00521	0.00168	0.0338	-0.0701
	(0.00142)	(0.00812)	(0.00572)	(0.00498)	(0.0237)	(0.0778)
Observations	284,705	284,705	284,705	284,705	49,479	12,033
District Controls	Yes	Yes	Yes	Yes	Yes	Yes
Worker Controls	No	No	No	No	Yes	Yes

Each column presents the results of a separate regression. All regressions include district and year-quarter fixed effects. The sample is composed of all adults aged 18 to 60 with secondary education or less interviewed from January 2004 to December 2005. For columns (1) to (5), the unit of observation is a district-quarter. For columns (6), the unit of observation is a person. The outcomes are defined as in Table 4, 5 and 6. Program is a dummy variable equal to one for early districts during January 2005 to December 2005. Dry is a dummy variable equal to one for the first two quarters of the year. Rainy is a dummy variable equal to one for the second two quarters of the year. District controls are listed in Table 1. District controls that do not vary over time are interacted with a dummy for 2005 (post-program). Worker controls are listed in the notes of Table 6. Star states is a dummy variable equal to one for districts within star states. Other states is a dummy variable equal to one for districts that are not in star states. See Table 2 for a description of star states. All estimates are computed using sampling weights. Standard errors in parentheses are adjusted for correlation of the errors at the district level. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 9: Welfare Gains by Expenditure Quintile

		Expenditure Quintile						
		Poorest	Second	Third	Fourth	Richest	Full Sample	Construction
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Household Expenditures and Income								
(1)	Monthly Consumption Per Capita	275.5	381.9	473.2	597.4	1023	511.8	NSS 2004-5
(2)	Total Monthly Consumption	1831	2362	2655	3138	4548	2769	NSS 2004-5
(3)	Total Earnings per Month for Adults doing Casual Labor	836	796	618	497	294	638	NSS 2004-5
(4)	Casual Earnings as Fraction of Household Consumption	0.46	0.34	0.23	0.16	0.06	0.23	NSS 2004-5
(5)	Average Earnings per Day Worked by Adults	40.0	43.7	47.1	51.0	57.0	44.8	NSS 2004-5
Gain from wage change								
(6)	Fraction of Casual Labor Costs Paid by Quintile	4.0%	6.2%	10.3%	14.2%	30.1%	12.8%	NCAER 1999
(7)	Estimated Monthly Labor Cost per Household	126	197	328	452	961	408	(6) x Full (3) x 5
(8)	Net Labor Earnings per Month	710	599	289	45	-666	230	(3) - (7)
(9)	Wage change	5.6%	5.6%	5.6%	5.6%	5.6%	5.6%	Estimated
(10)	Net Income Gain from Wage Change	39.7	33.6	16.2	2.5	-37.3	12.9	(8) x (9)
Gain from Government employment								
(11)	Increase in Days in Public Employment per HH per Month	1.79	1.02	0.98	0.79	0.44	1.00	Estimated
(12)	Average Private Sector Wage	44.8	44.8	44.8	44.8	44.8	44.8	NSS 2004-5
(13)	Average Government Wage	53.7	53.7	53.7	53.7	53.7	53.7	(12) + 20%
(14)	Direct Gain with Outside Option at Market Wage	24.0	9.1	8.8	7.1	3.9	8.9	(11) x [(13)-(12)]
(15)	Direct Gain with Outside Option at 50% of Market Wage	56.0	32.0	30.7	24.7	13.7	31.2	(11) x (13)
Total Gain								
(16)	Total Gain with Outside Option at Market Wage	63.7	42.7	25.0	9.6	-33.4	21.8	(10) + (14)
(17)	Total Gain with Outside Option at 50% of Market Wage	95.7	65.5	46.9	27.2	-23.6	44.1	(10) + (15)
Gain from Wage Change as Fraction of Total Gain								
(18)	Assuming Outside Option is Market Wage	62.4%	78.6%	64.9%	26.4%	**	59.0%	(10)/(16)
(19)	Assuming Outside Option is 50% of Market Wage	41.5%	51.2%	34.6%	9.3%	**	29.1%	(10)/(17)
Total Gain as Fraction of Total Expenditures								
(20)	Assuming Outside Option is Market Wage	3.5%	1.8%	0.9%	0.3%	-0.7%	0.8%	(16)/(2)
(21)	Assuming Outside Option is 50% of Market Wage	5.2%	2.8%	1.8%	0.9%	-0.5%	1.6%	(17)/(2)

Columns (1) to (5) correspond to different quintiles based on household per capita expenditure. Column (6) is all households. The last column indicates how each figure is obtained. Rows (1) to (5) use data from the NSS 2004-05 Employment Survey to compute averages for each quintile using survey sample weights. The fraction of casual labor costs paid by quintile (sixth row) is computed using data from the 1999-00 ARIS-REDS survey as follows. First we use monthly per capita expenditure to define quintiles. Second, by quintile, we aggregate all wages paid by the household to adult laborers. Third, we aggregate all income from casual labor supplied outside the household by all adults aged 18 to 60. The means in row (6) are obtained for each quintile by dividing total wages paid by total wage income received across all households. The wage change in row (9) is equal to the estimate of the program impact during the dry season from the specificiation in Table 8 with workers controls. The increase in days in public employment per household per month reported in row (11) is obtained from the regressions reported in Table A3. Row (18) and (19) , nothing is reported for the fifth quintile because the "gain from wage change" is a loss for this quintile.

FOR ONLINE PUBLICATION

A History of Public Works Programs in India

India has a long history of providing public works dating back to British rule. Three large-scale public works programs deserve specific mention. First is the Maharashtra Employment Guarantee Scheme passed in 1976 and still in force today. The NREGA is in part based on the design of the Maharashtra EGS. The NSS Employment Survey shows a significant amount of work in public works employment both before and after the introduction of the NREGA in the state of Maharashtra.

Second, the Sampoorn Grameen Rozgar Yojana (SGRY) started in 2001 with the purpose of generating employment across India and was still active until 2008. The total allocation to the SGRY was 35 billion Rupees per year from 2004-2008 (Afridi, 2008).

Finally, the National Food for Work Program was introduced as a pilot for the NREGA in 150 of the phase one districts, with an allocation of 60 billion Rupees in fiscal year 2005-06 (Afridi, 2008). As a comparison, during fiscal years 2006-07 and 2007-08, the allocation for the NREGA was 116 billion Rupees.

B Determinants of Government Employment Provision

The central government funds most of the expenditure for the NREGA (all of labor and 75% of material expenditures). However, the responsibility of implementing the scheme is left to the states and the lower administration levels (districts and village councils). In principle, local officials are meant to respond to worker demand for work, but the process required to provide work requires considerable administrative capacity: selection of public works projects, funding applications, opening of works, sanction of expenditures, and payments to workers and suppliers of materials. When the scheme started in each district, awareness campaigns also had to be implemented by the administration, sometimes with the help of civil society organizations. Depending on the administrative capacity of each state, NREGA implementation was initially more or less successful.

During the initial period that we study, the states of Andhra Pradesh, Rajasthan, Tamil Nadu, Madhya Pradesh and Chattisgarh provided significantly more employment than other states (Khera, 2011). This was partially due to demand for work in these states. However, very poor states such as Bihar, Jharkhand, Orissa, and Uttar Pradesh where demand should be high saw little employment generation. In this second group of states, lack of administrative capacity and rampant corruption hampered public employment delivery, despite large potential demand (Khera, 2011; Dutta et al., 2012). In the 2009-10 NSS employment survey, workers were asked whether they had, and whether they desired NREGA employment. Using answers to these questions, Dutta et al. (2012) confirm that three years after the scheme started, demand for work is still more rationed in the poorest states of India.

C Theoretical Appendix

C.1 Impact of Government Hiring on Wage

Recall the market clearing condition

$$L^g + D(W) = L^s \tag{8}$$

To determine the impact on wages we differentiate the market clearing condition with respect to L_g:

$$1 + D'(W)\frac{dW}{dL_g} = \sum_i \big(\frac{dL_i^s}{dW}|_{y_i}\frac{dW}{dL_g} + \frac{dL_i^s}{dy_i}\frac{dy_i}{dL_g}\big) \tag{9}$$

where $\frac{dL_i^s}{dW}|_{y_i}$ is the derivative of household i's labor supply with respect to the wage holding non-labor income fixed. The slutsky decomposition yields:

$$\frac{dL_i^s}{dW}|_{y_i} = \frac{dL_i^s}{dW}|_u + \frac{dL_i^s}{dy_i}L_i^s \tag{10}$$

where $\frac{dL_i^s}{dW}|_u$ is the substitution effect, i.e. the partial derivative of labor supply with respect to the wage holding utility constant. We have that:

$$\begin{aligned}\frac{dy_i^s}{dL_g} &= \pi_i'(W)\frac{dW}{dL_g} + (W_g - W)\frac{dL_i^g}{dL_g} - \frac{dW}{dL_g}L_i^g \\ &= -D_i\frac{dW}{dL_g} + (W_g - W)\frac{dL_i^g}{dL_g} - \frac{dW}{dL_g}L_i^g \end{aligned} \tag{11}$$

where the second equality follows from the envelope theorem for the profit function $\pi_i'(W) = -D_i$. Plugging Equations 10 and 11 into Equation 9 and re-arranging yields:

$$\frac{dW}{dL^g} = \frac{1 - \sum_i \frac{dL_i^s}{dy_i}(W_g - W)\frac{dL_i^g}{dL_g}}{-D'(w) + \sum_i \left(\frac{dL_i^s}{dW}|_u + \frac{dL_i^s}{dy_i}(L_i^s - L_i^g - D_i)\right)} \tag{12}$$

which is the desired result.

C.2 Compensating Variation Derivation

Recall the equation equating expenditure to income:

$$e(W, u_i) = \pi_i(W) + WT + (W_g - W)L_i^g + z_i \tag{13}$$

We derive the change in z_i required to maintain the equality, and therefore maintain the same utility level, following a change in L_g. We do this by differentiating Equation 13 with respect to L_g:

$$\frac{de(W, u_i)}{dW}\frac{dW}{dL_g} = \pi_i'(W)\frac{dW}{dL_g} + T\frac{dW}{dL_g} + (W_g - W)\frac{dL_i^g}{dL_g} - L_i^g\frac{dW}{dL_g} + dz_i \tag{14}$$

By the envelope theorem $\frac{de(W,u_i)}{dW} = T - L_i^s$ and $\pi_i'(W) = -D_i$. Using these results and re-arranging yields:

$$\begin{aligned} -dz_i &= (L_i^s - L_i^g - D_i)W\frac{dW/W}{dL_g} + (W_g - W)dL_i^g \\ &= \textit{Net Casual Labor Earnings} \times \frac{dW/W}{dL_g} + (W_g - W)dL_i^g \end{aligned} \tag{15}$$

C.3 Impact on Household Consumption

In this section, we derive the impact of a workfare program on household consumption. The impact on consumption is different from the impact on welfare because it also includes labor supply effects. Household consumption is given by:

$$c_i = \pi_i(W) + WL_i^s(W, y_i) + (W_g - W)L_i^g \tag{16}$$

Assuming a small change in L^g ($\{L_i^g\}$), we can totally differentiate to obtain:

$$\begin{aligned}\frac{dc_i}{dL^g} &= (W_g - W)\frac{dL_i^g}{dL^g} \\ &+ WL_{yi}^s(W_g - W)\frac{dL_i^g}{dL^g} \\ &+ (L_i^s - D_i - L_i^g)\frac{dW}{dL^g} \\ &+ W\Big[\frac{dL_i^s}{dW}|_u + L_{y_i}^s(L_i^s + T - L_i^g - D_i)\Big]\frac{dW}{dL^g} \end{aligned} \tag{17}$$

The first term is the income gain due to participation in public works. The impact of this increase in income on labor supply is captured by the second term. It is negative if leisure is a normal good. Together, these first two terms yield the increase in consumption that would be observed by matching participants and non-participants in program areas.

The two last terms express the "indirect benefit", i.e. income gains accruing to households through equilibrium effects. The third term is the change in income due to the equilibrium change in the wage (holding labor supply constant). The last term captures the labor supply response due to the change in income from the equilibrium change in the wage. It is composed of a positive substitution effect and an income effect, which could be negative for households that are net buyers of labor.

C.4 Unemployment

We extend the model to include a friction in the labor market such that households that supply L days of labor to the labor market only receive p_iL days of work. This extension is similar to the way unemployment is modeled in Basu et al. (2009). One can think of p_i as including search costs as well as potential discriminatory practices by employers against certain types of households. We assume that household i's production function is of the form $F_i(\cdot) = A_iG(\cdot)$ with $G'(\cdot) > 0$ and $G''(\cdot) < 0$. There are three cases to consider. Less productive households (low A_i) will be net labor supplying households and will face a marginal value of time of p_iW and therefore set $A_iG'(D_i) = p_iW$. Very productive households (high A_i) will be net labor buying households and will face a marginal value of time of W and therefore set $A_iG'(D_i) = W$. Finally, a non-trivial subset of households with A_i in the middle of the distribution will neither buy nor sell labor to the market so that $A_iG'(D_i) \in [p_iW, W]$.

Proposition 1: There exists a threshold A_e such that households are net labor

buyers if and only if $A_i > A_e$

Proof: Let $L^s = L^s(\tilde{W}, \tilde{Y})$ be the solution to the maximization problem

$$\max_{L,c} u(c, T-L) \tag{18}$$

$$\text{s. t. } c + \tilde{W}(T-L) = Y + \tilde{W}L \tag{19}$$

Let $D_i = D(\tilde{W}, A_i)$ be such that $A_i G'(D_i) = \tilde{W}$. Fixing W, define A_e (and D_e) such that

$$D_e = D(W, A_e) \tag{20}$$

$$L^s(W, A_e G'(D_e)) = D_e \tag{21}$$

Note that since $L^s_Y \leq 0$ and $D_A(\tilde{W}, A_i) \geq 0$, the pair A_e and D_e exists and is unique. A household with $A_i = A_e$ therefore supplies and demands D_e labor. Since the marginal cost of hiring labor is W while the marginal value of working in the labor market is $p_i W < W$, the household will always supply labor to its own production function at least up to D_e. Therefore, households with $A_i = A_e$ are neither net labor supplying nor net labor buying households. For $A_i > A_e$, we will have $D(W, A_i) > L^s(W, A_i G'(D(W, A_i)))$, so that the household will be a net labor buyer as long as it can hire labor at W and as long as the marginal value of time is given by W as well. Since net labor buyers supply labor only to their own farm, this will be the case. Net labor buyers will always face an effective marginal wage of W. Therefore, if $A_i < A_e$, then $D(W, A_i) < L^s(W, A_i G'(D(W, A_i)))$, so that households will not be net buyers of labor.

Proposition 2: There exists a threshold A_w such that households are net labor buyers if and only if $A_i < A_w < A_e$

Proof: Fixing W, define A_w (and D_w) such that

$$D_w = D(p_i W, A_w) \tag{22}$$

$$L^s(p_i W, A_e G'(D_w)) = D_w \tag{23}$$

A household with $A_i = A_w$ will supply and demand D_w units of labor but because $pW < W$ we have $D_w < D_e$ and $A_w < A_e$. For a household with $A_i < A_w$, we will have $D(p_i W, A_i) < L^s(pW, A_i G'(D(p_i W, A_i)))$, so that the household will be a net labor supplier. Net labor suppliers will always face an effective marginal wage of $p_i W$. For a household with $A_i > A_w$, we will have $D(p_i W, A_i) > L^s(pW, A_i G'(D(p_i W, A_i)))$, so that the household will not be a net labor supplier.

Proposition 3: For $A_i \in [A_w, A_e]$, households will be neither net suppliers or buyers of labor.

Proof: This follows directly from the first two propositions. For $A_i \in [A_w, A_e]$, labor supply and demand D will solve $D = L^s(A_i G'(D), A_i G(D))$. Note that for $A_i \in [A_w, A_e]$, the labor supply and demand will satisfy $A_i G'(D) \in [p_i W, W]$.

C.5 Imperfect Competition

We assume that the marginal productivity of labor is equal to the wage rate. Some have noted the presence of market power on the part of employers (Binswanger and Rosenzweig, 1984). If employers have market power then government hiring may actually increase private sector wages *and* employment. We refer the interested reader to Basu et al. (2009), who provide a full analysis. Here, we sketch the main intuition and discuss the implications for the interpretation of the empirical results. A monopsonistic employer with production function $F(L)$ facing an inverse labor supply curve $W(L)$ sets the wage and employment such that:

$$F'(L^*) = W(L^*) + W'(L^*)L^* \tag{24}$$

This is the well-known result that the marginal productivity of labor will be above the wage rate if employers exercise their market power. The extent of the distortion depends on the slope of the labor supply curve ($W'(L)$). If the selection rule used by the government to hire workers under the workfare program shifts $W'(\cdot)$ down (makes labor supply more elastic), then all things equal, L^* must increase to maintain the equality in Equation 24. Since the workfare program also reduces the available workforce, the net effect on private sector work is ambiguous.

For the present analysis, the important issue is whether, given the rise in wages due to the program, Equation 5 still captures the welfare impact of the program under imperfect competition. For labor suppliers, the welfare impact is the same. For labor buyers, however, Equation 5 no longer correctly captures the welfare impact of the program since the welfare impact now depends on how the inverse labor supply function changes, which in turn will be a function of the particular rationing rule used by the government.

D Data Appendix

D.1 National Sample Survey Organisation: Employment Surveys

Sample: The main data source used in this paper is the National Sample Survey rounds 60, 61, 62, 64 and 66. These surveys are conducted on an irregular basis roughly every two years. Rounds 61, 64 and 66 are "thick" rounds, with a sample size of roughly 70 thousand rural households, while rounds 60 and 62 are "thin" rounds, with roughly 35 thousand rural households. The survey is usually conducted from July to June, with the sixtieth round conducted from January to June being an exception. The surveys are stratified by urban and rural areas of each district. Surveying is divided into four sub-rounds each lasting three months. Although the sample is not technically stratified by sub-round, the NSSO states that it attempts to distribute the number of households surveyed evenly within each district sub-round.

Table A.1 presents evidence on how the sample is distributed throughout the years in practice. For employment outcomes, a district is missing in a given quarter if no household was interviewed. From Table A.1 we see that for thick rounds, we have observations for all district-quarters. For "thin" rounds, there are a number of instances in which surveying did not take place in a particular district-quarter.

For casual wages, a district is missing in a given quarter if no household was surveyed or if no prime-age adult reported doing casual work in the past week. As a result the proportion of missing observations is larger for wages than for the employment variables. During thick rounds, the fraction of missing observations is as high as five percent and for the thin rounds it is as high as 20%. One might worry that by reducing private employment the program may increase the probability that a district is missing in a given quarter. However, this does not seem to be a major concern given that the fraction of early districts among non-missing observations is constant across quarters.

Outcomes: Our main outcomes are individual measures of employment and wages, which are constructed as follows. The NSS Employment Survey includes detailed questions about the daily activities for all persons over the age of four in surveyed households for the most recent seven days. We compute for each person the fraction of days in the past seven days spent in each of four mutually exclusive activities: non-government work, public works, not in the labor force, and unemployed. Individuals who worked in casual labor over the past seven days are asked their total earnings from casual labor. For each individual we compute average earnings per day worked in casual labor. We perform a similar computation using days spent doing salaried labor to construct our measure of daily salaried earnings.

D.2 District Controls

Table 1 provides a list of district controls and their sources. Here, we describe how the district controls are constructed.

Census A number of the districts controls are computed from the primary census abstract of 2001. In all cases, we use information for rural areas only, which we then aggregate to the district level. We compute "fraction of scheduled tribes" and "fraction of scheduled castes" by dividing by total population. "Population density" is obtained by dividing total population by total area. "Literacy rate," "male labor force participation ratio" and "female labor force participation ratio" are computed by dividing by total population aged six and over. "Fraction of labor force in agriculture" is obtained by dividing the number of rural individuals who report working as cultivators or agricultural laborers as their main or secondary occupation by the total number of workers. Finally, we use information from the census village directory to compute "irrigated cultivable land per capita" and "non irrigated cultivable land per capita."

Rainfall To control for monthly rainfall at the district level over the period 2003-2010, we combine two data sets. For the period 2004-2010, we use data from the Indian Meteorological Department (IMD), which reports online district-level monthly averages of precipitation. These measures come from sub-district meteorological stations which record daily precipitation. The other data is the University of Delaware Air Temperature & Precipitation dataset.[18] The researchers used station-level information on rainfall, and when missing, interpolated to obtain average monthly rainfall for each point in a grid of 0.5 by 0.5 degrees from 1975 to 2008. In order to match the grid with Indian districts, we averaged information over all grid-points which fell in each district. Finally, we regressed IMD measures on Delaware measures separately for each district in 2004-2008, and predicted rainfall before 2004 using this model and Delaware rainfall data. From the combined 1975-2010 dataset, we constructed the two control variables, "Rainfall annual" which is the percentage deviation to the average precipitation since 1975 and its square "Rainfall annual square".

[18]Provided by the NOAA/OAR/ESRL PSD, Boulder, Colorado, USA `http://www.esrl.noaa.gov/psd/`

Other controls "Pre-election year" is a dummy for whether state assembly or Panchayati Raj (local) elections are to be held in the following year. To construct this control, we used online reports from the Electoral Commission of India[19] and from the State Election Commissions of each states. "In south" is a dummy which takes the value one if a district belongs to one of the following four states: Andhra Pradesh, Karnataka, Kerala and Tamil Nadu.

D.3 ARIS-REDS Household Hired Labor

For our calibration exercise in Section 6, we require estimates of labor hired by households, information which is not available in the NSS Employment Surveys. For this reason, we use the ARIS-REDS survey data, collected by the National Council of Applied Economic Research (Delhi) in 1999-00.[20] The ARIS-REDS survey covers a nationally representative rural sample of Indian households, with detailed information on household expenditures, on household members' employment income and on operating costs of households' farm and non-farm businesses.

For each household, we sum all income earned by prime-age household members from casual labor and total labor costs for farm and non-farm businesses. For each consumption quintile, we then compute the total casual payments as a fraction of total casual earnings for all households across all quintiles. Let e_t^q and p_t^q denote casual earnings and casual payments, respectively, for households in consumption quintile q at date t. We compute for each quintile $f_{2000}^q = \frac{p_{2000}^q}{\sum_q e_{2000}^q}$. The resulting fractions are reported in the sixth row of Table 9. As expected the fraction of total casual earnings paid by households in the lower quintiles is much lower than the fraction paid by households in the upper quintiles. These fractions sum to less than one across consumption quintiles because some casual labor earnings come from urban employers.

In order to estimate casual labor payments by households of each consumption quintile in 2004-2005, we make the assumption that casual labor payments made by each consumption quintile as a fraction of total earnings is constant over time, i.e. $f_{2005}^q = f_{2000}^q$. We then multiply total casual labor earnings from the NSS Employment Survey by the fractions in row six for each consumption quintile to obtain our estimate of casual labor payments by quintile: $\widehat{p_{2005}^q} = f_{2005}^q * \sum_q e_{2005}^q$. Our estimates are shown in row seven of Table 9

[19] `http://www.eci.nic.in/ecimain1/index.aspx`

[20] `http://adfdell.pstc.brown.edu/arisredsdata/readme.txt`

D.4 Weighting

The NSSO provides sample weights which ensure that the weighted mean of each outcome is an unbiased estimate of the average of the outcome for the population National Sample Survey Office (2010). For the purpose of our analysis, we re-weight observations so that the sum of all weights within each district is constant over time and proportional to the rural population of the district as estimated from the NSS Employment Surveys. Another approach would be to assign all districts equal weight. We prefer population weights since they reduce the concern that the results are driven by small districts with noisy employment or wage estimates. More concretely, let w_i be the weight for person i, and let Ω_{dt} be the set of all persons surveyed in district d at time t. Then the new weight for person i is $w_i \times \frac{\omega_d}{\sum_{i \in \Omega_{dt}} w_i}$ where ω_d is the population weight for district d.

D.5 Construction of District Panel

During the period covered by the analysis, some districts split while other districts merged together. Constructing the district panel requires matching districts both over time as well as across data sets. Fortunately, the NSS district definitions for surveying stayed constant from 2004 to 2008, despite splits and merges. We therefore use the NSS district definitions from this period and match other data sets to these. Specifically, we match the NSS 2004-2008 data with the NSS 2009-10 survey, Census 2001 survey, NREGA phases 2005, ARIS-REDS 1999-00 survey, and Indian Meteorological Department 2004-2010 data. Matching with the University of Delaware Air Temperature & Precipitation data is done geographically, using a shape file of districts with 2005 borders: all grid points that fall within a district's border are matched to that district.

Table A.1: Balance of District Panel

	Q3 Jul-Sep (1)	Q4 Oct-Dec (2)	Q1 Jan-Mar (3)	Q2 Apr-Jun (4)
Employment Variables				
2003-04	--	--	485	485
2004-05	493	492	490	491
2005-06	432	446	--	--
2006-07	--	--	--	--
2007-08	493	493	491	493
2008-09	--	--	--	--
2009-10	493	493	492	493
Casual Wages				
2003-04	--	--	472	470
2004-05	475	477	475	479
2005-06	397	413	--	--
2006-07	--	--	--	--
2007-08	477	479	482	480
2008-09	--	--	--	--
2009-10	473	473	471	477

Each cell shows the number of districts with non-missing observations per district-quarter. There are 493 districts in the panel. The NSS attempts to survey an equal number of villages in each districts during each quarter. During thick rounds (2004-05, 2007-08, 2009-10), this is generally possible. During thin rounds (2005-06, 2003-04), this is less likely to be achieved. Casual wages are only available for district-quarters during which at least one respondent reports working in casual labor.

Table A.2: Log Deflated Casual Earnings Difference-in-Difference Estimates

		Early Districts		Late Districts		Diff-in-Diff	
		Rainy	Dry	Rainy	Dry	Rainy	Dry
		Jul to Dec	Jan to Jun	Jul to Dec	Jan to Jun	Jul to Dec	Jan to Jun
		(1)	(2)	(3)	(4)	(5)	(6)
						(1) - (3)	(2) - (4)
(1)	Pre (1/04 to 12/05)	3.7184	3.7119	3.9147	3.9246		
		(0.017)	(0.0159)	(0.0318)	(0.0299)		
		N=71674	N=101946	N=46943	N=64142		
(2)	Post (2007-08)	3.9955	4.1113	4.1647	4.2717		
		(0.0177)	(0.0164)	(0.0282)	(0.0314)		
		N=50721	N=50536	N=28116	N=27757		
(3)	(2) - (1)	0.2771***	0.3994***	0.25***	0.3471***	0.0271	0.0522**
		(0.0151)	(0.0148)	(0.0174)	(0.0174)	(0.023)	(0.0228)
(4)	Post (2009-10)	4.2857	4.4362	4.5104	4.5736		
		(0.0169)	(0.0199)	(0.0302)	(0.0326)		
		N=33638	N=33374	N=22248	N=22003		
(5)	(4) - (2)	0.2901***	0.3249***	0.3457***	0.302***	-0.0556**	0.023
		(0.5673)	(0.7243)	(0.5957)	(0.6491)	(-0.0284)	(0.0752)

The sample is composed of adults adults aged 18 to 60 with secondary education or less. Each cell is a mean of the log of average casual earnings, with standard errors in parentheses and the number of individual observations below. Log daily casual earnings is the log of earnings per day worked for people who report working in casual labor. For example, row (1), column (2) is the mean of the log of deflated casual earnings for all districts that received the program prior to April 2007 (early districts) with the sample restricted to the first six months (dry season) of 2004 and 2005. 2007-08 and 2009-10 correspond to agricultural years (July to June). The public works program was introduced in early districts between February 2006 and April 2007. The program was introduced to late districts in April 2008. All means are computed using sampling weights. Standard errors are adjusted for correlation of the errors at the district level, ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table A.3: Outcomes by Consumption Quintile

	Public	Private	Unemployed	Not in Labor Force	Log Deflated Daily Earnings
	(1)	(2)	(3)	(4)	(5)
Dry Season					
Program X Dry X Quintile 1	0.0176**	-0.0236*	0.0202**	-0.0142**	0.0676**
	(0.00758)	(0.0137)	(0.00962)	(0.00688)	(0.0322)
Program X Dry X Quintile 2	0.00995***	-0.00722	0.00922	-0.0120*	0.0680**
	(0.00364)	(0.0102)	(0.00712)	(0.00662)	(0.0276)
Program X Dry X Quintile 3	0.00994***	-0.00597	-0.00393	-4.35e-05	0.0185
	(0.00326)	(0.0103)	(0.00687)	(0.00637)	(0.0296)
Program X Dry X Quintile 4	0.00795***	-0.0206**	0.0133**	-0.000567	0.0657*
	(0.00231)	(0.00859)	(0.00637)	(0.00616)	(0.0338)
Program X Dry X Quintile 5	0.00465**	-0.00857	0.000835	0.00309	0.0224
	(0.00194)	(0.00996)	(0.00548)	(0.00832)	(0.0469)
Rainy Season					
Program X Rainy X Quintile 1	0.00726***	0.000327	-0.00217	-0.00541	0.0258
	(0.00218)	(0.0118)	(0.0105)	(0.00704)	(0.0279)
Program X Rainy X Quintile 2	0.00453***	0.00397	-0.00397	-0.00453	0.0608**
	(0.00165)	(0.00946)	(0.00723)	(0.00602)	(0.0310)
Program X Rainy X Quintile 3	0.00642***	-0.000452	-0.00150	-0.00447	-0.0125
	(0.00162)	(0.00842)	(0.00569)	(0.00637)	(0.0314)
Program X Rainy X Quintile 4	0.00219	0.00574	0.00473	-0.0127*	0.0142
	(0.00189)	(0.00914)	(0.00623)	(0.00672)	(0.0372)
Program X Rainy X Quintile 5	0.00336**	0.0130	-0.0104*	-0.00599	0.0277
	(0.00139)	(0.0111)	(0.00546)	(0.0108)	(0.0591)
Observations	498,994	498,994	498,994	498,994	87,545
District x Quintile FE	Yes	Yes	Yes	Yes	Yes
Quarter x Year FE	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	No	No	No	Yes

The unit of observation is a household. Outcomes are equal to sum of the employment outcomes described in Table 2 over all prime age persons within a household. The sample uses all persons 18 to 60 with no restriction based on education. Dry is a dummy variable equal to one for the first two quarters of the year. Rainy is a dummy variable equal to one for the second two quarters of the year. Quintile 1 to 5 are dummy variables equal to one if the individual is in a household with expenditure in that quintile. Quintile 1 is the poorest quintile. District controls are listed in Table 2. District controls that do not vary over time are interacted with a dummy for 2007-08 (post-program). The sample includes all observations from January 2004 to December 2005 and from July 2007 to June 2008. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table A.4: Effect of the Program on Consumption by Consumption Quintile

Average	Expenditure Quintile				
	Poorest	Second	Third	Fourth	Richest
-0.0741***	-0.0288***	-0.0529***	-0.0722***	-0.102***	-0.1074***
(0.0153)	(0.0109)	(0.0138)	(0.0136)	(0.0162)	(0.0227)

Estimates are obtained using the Athey Imbens (2006) non-linear difference-in-difference method. Standard errors in parentheses are obtained by bootrstrapping and adjusted for correlation of the errors at the district level. ***, **, and * indicate significance at the 1, 5, and 10% levels.