Government Hiring and Labor Market Equilibrium: Evidence from India's Employment Guarantee

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Abstract

This paper presents evidence on the equilibrium labor market impacts of a large Indian rural workfare program. Our identification strategy compares changes in outcomes in districts that received the program earlier to districts that received it later. These difference-in-differences estimates reveal that following the introduction of the program, public employment increased by .3 days per prime-aged person per month (1.3% of private sector employment) more in early districts than in the rest of India. Casual wages increase by 4.5% more in early districts, and private sector work falls by 1.6%. These changes are concentrated in the dry season, during which the majority of public works employment is provided. Estimates are larger for districts in states known to have better implemented the technical and administrative requirements of the act. We use the estimates along with household-level data on labor supply and demand, consumption, and program participation to compute the implied welfare gains by consumption quintile. Our estimates suggest 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. We conclude that the equilibrium labor market impacts are a first order concern when comparing workfare programs with other anti-poverty programs such as a cash transfer.
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1 Introduction

Workfare programs are common anti-poverty policies. Many developing countries have programs that hire workers at competitive wage rates in the interest of increasing the income of the poor.[1] A substantial literature estimates the income and consumption benefits of these programs by comparing participants with matched non-participants [Datt and Ravallion, 1994, Ravi and Engler, 2009]. Workfare programs may change the labor market equilibrium and in particular may lead to an increase in private sector wages [Ravallion, 1987, Basu et al., 2009]. As a result, comparisons of participants with non-participants within the same labor market may understate the true income gains to net labor sellers and overstate gains to net buyers of labor. The literature has made few attempts to quantify how large the equilibrium effects are in practice, owing mainly to the fact that we rarely observe even an approximate counter-factual labor market equilibrium.

This paper uses the gradual roll-out of a large rural workfare program in India to estimate the program's impact on wages and aggregate employment. We use a difference-in-differences strategy comparing changes in districts that received the program earlier to districts that received it later. Armed with a model of rural labor markets, we use these estimates to calculate how the welfare gains from the program are distributed across the population. We compare gains due to the estimated equilibrium rise in wages to the gains that would be estimated by comparing participants and non-participants. Our results suggest that for households in the bottom half of the consumption distribution, the gains from the rise in equilibrium wages are of a similar magnitude to the direct gains from participating in the program. We conclude that in weighing the relative merits of a workfare program and other anti-poverty policies such as a cash-transfer, the potential impact on equilibrium wages

[1]Recent examples include programs in Malawi, Bangladesh, India, Philippines, Zambia, Ethiopia, Sri Lanka, Chile, Uganda, and Tanzania. However, the practice of imposing work requirements for welfare programs stretches back at least to the British Poor Law of 1834.

cannot be ignored.

Much of the existing literature on the welfare effects of workfare programs focuses on the targeting benefits of these programs relative to a cash transfer [Besley and Coate, 1992, Gaiha et al., 2009]. The basic argument is that because a workfare program entails a work component, participants are self-selected to have lower outside options than non-participants. In this framework, the change in income due to the program is simply the difference between the wage provided by the program and the income the participant would have earned had she not participated in the program. This theoretical framework motivates estimating the income gains from workfare programs by comparing participants with matched non-participants, which is a common approach in the literature [Datt and Ravallion, 1994, Ravi and Engler, 2009]. While informative, these comparisons of participants and non-participants abstract from potential equilibrium impacts.

A largely theoretical literature explores the equilibrium impacts of workfare programs [Basu et al., 2009, Ravallion, 1987]. Basu et al. [2009] show that public employment schemes may increase private wages by increasing the reservation wage of private sector workers. In the simplest case of a perfectly competitive labor market, the rise in wages comes with a decline in private employment as employers move down their demand curves. However, the authors show that if employers have market power, a public workfare program may actually lead to a rise in private sector work.

The empirical literature on the equilibrium impacts of workfare programs is limited. To the extent that empirical studies are attempted, the analysis is restricted to considering hypothetical wage increases [Murgai and Ravallion, 2005]. We contribute to the theoretical literature with a model that clarifies how the income gains estimated by comparing participants to non-participants differ from the equilibrium welfare effects of a workfare program. In particular, an equilibrium rise in wages will benefit net labor sellers, and to the extent that the change in wages is not due to an increase in worker productivity, the rise in wages will hurt net labor buyers. The model provides a straightforward framework for assessing the importance of the income changes due to changes in equilibrium wages relative to changes in income strictly due to participation in the program. The model draws heavily from the work of Deaton [1989] and Porto [2006].

We apply the framework to estimate the distributional effects of India's National Rural Employment Guarantee Act (NREGA). The NREGA provides short-term manual work mostly during the agricultural off-season at a wage comparable to or higher than the market rate. According to government administrative data, in 2009-10 the program provided 1.36 billion person-days of employment to 54 millions households. The program was phased in gradually across India starting with the poorest districts in early 2006 and extending throughout the entire country by mid 2008. We estimate the impact of the program 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. Our pre-period is January 2004 to December 2005 and our post period is July 2007 to June 2008. For reasons discussed in detail in Section 4, we consider districts that received the program in April 2008 as a viable control group even during the period from April to June of 2008 after the program had technically started in those districts.

The empirical analysis proceeds in four steps. We first show that the introduction of the workfare program is correlated with a substantial increase in low-wage, low-skilled public employment. This is an important finding in its own right as it suggests the program did not just crowd out existing government employment. Further, although government administrative data suggests high levels of employment under the act, many field studies have documented widespread over-reporting of employment by corrupt officials [Niehaus and Sukhtankar, 2008, Khera, 2011]. We find that public employment provision is highly seasonal with the majority of employment provided during the first two quarters of the year when rainfall is low. During these first two quarters of the year, the increase in public employment is equivalent to hiring 1.3% of the low-skilled private sector rural workforce. Field studies confirm this seasonal pattern and suggest that the seasonality is driven by supply constraints rather than a lack of demand on the part of potential workers. The monsoon rains make provision of work difficult and farmers actively lobby for works to be suspended during the rainy season as it is the peak period of agricultural labor demand.

The results confirm field evidence that employment generation under the act varies widely by state. Indeed, five states are responsible for most of the increase in government employment. In these states, the increase in public employment is equivalent to 4% of the low-skilled private sector rural workforce. District-level regressions suggest that these differences are not explained by differences in factors correlated with demand such as the level of wages, poverty rate, or literacy rate. We conclude that the field studies are accurate in attributing much of the cross-state differences in public employment generation to supply-side differences in the administrative capacity or political will to implement the program.

Second, we document that average daily earnings of casual laborers increase by roughly 4.5% during the dry season in districts that received the program relative to control districts. A number of results suggest that these differential changes in wages are at least in part due to the program. We do not find a relative increase in wages during the rainy season, when employment generation is low. Consistent with cross-state variation in implementation, the differential increase in wages is almost twice as large (9%) in the five states where field studies suggest (and our estimates confirm) that the program is implemented the best. 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, suggesting our estimates are not just picking up differential trends in inflation.

The fall in salaried wages in early districts relative to late districts highlights the fact that since early districts were selected to be poorer districts than late districts, late districts are unlikely to provide a perfect counterfactual for early districts. As a result, the changes that we document may be due to some other factor correlated with poverty. Adding district-level poverty rates and other controls interacted with a dummy for the post-treatment period actually slightly strengthens the wage results. Still, differential district-level trends remain a concern for our identification strategy. During the two years prior to the program, wages in early districts increase by more in early districts than late districts, though this increase is concentrated outside the star states and during the rainy season.

Third, we document the differential changes in aggregate employment across program and control districts. We find the introduction of the program is correlated with a 1.6% fall in the fraction of days spent doing any kind of private work (waged, self employed or domestic work) among low-skilled persons. Interestingly, we find no evidence of a fall in the fraction of people reporting being unemployed or out of the labor force. Finally, program districts in star states show a much larger fall of 3.7% of private sector work, which is roughly equivalent in magnitude with the increase in public employment. These results are consistent with one for one crowding out of private sector work by the public works program, with no change in unemployment or participation in the labor force. Importantly, especially for women, we include domestic work in our measure of private sector work.

The fourth empirical step uses the wage and employment estimates combined with household level data on consumption and casual labor supply and demand to compute how the welfare gains from the increase in wages are distributed across rural households. We show that wage increases redistribute 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 for participants relative to non-participants. Our estimates suggest that the changes in welfare due to the wage change are large in absolute terms and large relative to the direct welfare gains for participants. For the bottom three quintiles, the estimated welfare gain due to the wage change represents 20-60% of the total welfare gain from the program.

This paper relates to three strands of the literature. First, it contributes to the small but growing literature of papers which examine the impact of the NREGA itself [Ravi and Engler, 2009]. Second, it contributes to the literature documenting the equilibrium impacts of social programs on non-participants [Angelucci and Giorgi, 2009, Jayachandran et al., 2010]. Third, it contributes to the literature on rural labor markets in developing countries [Rosenzweig, 1978, Binswanger and Rosenzweig, 1984, Stiglitz, 1974]. Finally, it contributes to the policy debate concerning the relative merits of workfare programs relative to other anti-poverty programs such as a cash transfer [Kapur et al., 2008].

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 net 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 2.27 billions person-days of employment to 53 million households. The India-wide budget was Rs. 345 billion (7.64 billion USD), which represents 0.6% of GDP.

The act was gradually phased in throughout India starting with 200 of the poorest districts in February 2006, extended to 120 districts in April 2007, and finally to the rest of rural India in April 2008. Our empirical strategy, described in detail in Section 4.3, compares outcomes in districts that received the program earlier relative to those that received it later.

The National Rural Employment Guarantee Act sets out detailed guidelines about how the program is to be implemented in practice. However, whether and how these guidelines are actually followed varies widely by state and even district [Sharma, 2009, Dreze and Khera, 2009, Institute of Applied Manpower Research, 2009, The World Bank, 2011]. Field studies reveal substantial discrepancies between the law and practice with many people unaware of their full set of rights under the program. Based on existing field studies, we describe how the act operates in practice. However, it should be kept in mind that how the act is implemented is changing over time, and precisely how the act operates in practice is still an active area of research.

2.1 Poverty Reduction through Employment Generation

The main motivation 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). Since it is first and foremost a poverty alleviation scheme, the NREGA is often compared to a cash transfer programs [Kapur et al., 2008]. The fact that poverty reduction through 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. Further, the act limits material, capital and skilled wage expenditure to 40% of total expenditure and in practice the actual expenditure is even lower (27% in 2008-09).[2] 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

[2]Figures are from the official NREGA website *www.nrega.nic.in*.

that require mainly low-wage, manual work potentially at the expense of the productivity benefits of the resulting infrastructure.

2.2 Short-term, Unskilled Jobs

The work generated by the program is short-term, unskilled, manual work. The most common activities include 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.[3] The jobs provided by the program are very similar to private sector casual labor jobs, which are also short-term, low-wage, often manual jobs usually in agriculture or construction. In fact, India's National Sample Survey Office, 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, forty-six percent report that they usually or sometimes engage in casual labor, while only .1% report that they usually or sometimes work in a salaried job.[4] The 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

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

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

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 agricultural year 2008-09, both male and female workers report earning an average of Rs. 79 per day for work under the act.[6] These self-reported NREGA earnings should be interpreted with some caution. Because of well-documented delays and corruption in the payment system, workers may not report actual NREGA earnings. With this caveat in mind, 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 Employment, Rationing and Awareness

Perhaps a more direct way to assess whether NREGA work is more attractive than available work is to ask people. The studies that ask find high levels of unmet demand [Dreze and Khera, 2009]. Although the act stipulates a minimum employment guarantee of 100 days of work per household per year, actual employment falls well short of the 100 day guarantee, even for households that report wanting to work the full 100 days.

One may naturally wonder, if the act guarantees 100 days and households want 100 days, why workers do not simply demand 100 days of work. In some areas, activists have mobilized workers to to do just this [Khera, 2011]. However, as The World Bank [2011] summarizes

> In practice, very few job card holders formally apply for work while the ma-

[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.

jority tend to wait passively for work to be provided. At the same time, there appears to be considerable latent demand for work - i.e., not all people who demand work are provided work, while even those who are provided work would like more days of employment.

Even those who demand work are not guaranteed work. During agricultural year 2009-10, an estimated 19% of households reported attempting to get work under the act without success.[7]

2.5 Timing of Works

Work appears to be not only rationed at the individual and household levels but also 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. However, field reports 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].

2.6 Cross-State Variation in Implementation

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 star performers, though even in these states implementation falls short of the requirements of the act. In the empirical analysis, we confirm that these states generated significantly more employment under the act than other states in India. Further, the differences in employment generation are not

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

explained by district-level correlates of demand for public works such as poverty, illiteracy or wages. 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.

2.7 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 writes:

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

Sharma [2009] looks at changes in wages at the state-level for the two years prior to the introduction of the program and the two years after the introduction. He finds that although nominal wages increased, aggregate price levels also rose wiping out all gains except for a slight rise in wages for women. It is difficult to conclude much from these estimates since the NREGA was introduced at the district rather than state level. Moreover, nothing is done to account for an India-wide trend in prices or wages.

Ravi and Engler [2009] use survey data from 1,000 households in Andhra Pradesh from June 2007 to December 2008 and match NREGA participants with non-participants based on observable characteristics such as caste, gender, and land ownership. They find an increase in monthly per capita consumption for participant households on the order of 6%. The results presented here suggest this estimate is biased downwards as we present evidence that the NREGA raised the wage level as well, so that comparing persons in the same labor market understates the true impact of the program.

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 then use the framework to trace out the equilibrium distributional impact of the program across households. The model draws heavily from Deaton [1989] and Porto [2006], both of whom apply a similar framework to analyze the distribution effects of price changes. The key difference here is that we focus on the labor market, though much of the analysis is similar.

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 that this assumption is reasonable.

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:

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

where L_i is total (public and private) sector labor supplied by the household. Let the solution to this optimization problem for L_i be denoted by $L_i^s(w, y_i)$ where $y_i = \pi_i(W) + W(T - L_i^g) + W_g L_i^g$. Note that the government wage from public sector work W_g only enters through it's impact on 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 $L^s(W, y_1, \ldots, y_N) \equiv \sum_i L_i^s(W, \pi_i + WT + (W_g - W)L_i^g)$. In the subsequent analysis, we assume that both of these functions are differentiable. The government sets an aggregate level of public works employment $L^g \equiv \sum_i L_i^g$. Note that because we assume $W_g > W$, the government must decide how the public works employment is to be rationed across households. That is, it must choose the L_i^g's. Labor market clearing implies that $L^g + D(W) = L^s(W, \{y_i\})$.

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. To determine the impact on wages we totally differentiate the market clearing condition to get

$$\frac{dW}{dL^g} = \frac{1 - \sum_i L^s_{y_i}(W_g - W)\frac{dL_i^g}{dL_g}}{\sum_i \frac{dL_i^s}{dW}|_u - D'(w) + \sum_i L^s_{y_i}(L_i^s + T - L_i^g - D_i)} \tag{2}$$

where $\frac{dL^s}{dw}|_u$ is the substitution effect, i.e. the partial derivative of labor supply with respect to the wage holding utility constant, and $L^s_{y_i}$ is the income effect for household i. Note that in deriving the equation we use the envelope theorem for the profit function $\pi_i'(W) = -D_i$. The change in aggregate private sector employment is given by $\frac{dD}{dL^g} = D'(W)\frac{dW}{dL^g}$. As a result, we can 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 3.5.6, we discuss later why this ratio might not correspond to the labor demand elasticity if employers exercise market power.

From equation 2, we see that an increase in government hiring will raise wages as long as the income effect is not too large ($\sum_i L^s_{y_i}(W_g - W) < 1$). The increase will be larger if demand is less elastic (small $-D'(W)$) or if labor supply is less elastic (small $\sum_i(\frac{dL_i^s}{dW}|_u + L^s_{y_i}L_i^s)$). Note also that in equilibrium, the net labor demanding households (households with high D_i relative to L_i^s) may actually increase their labor supply due to the income effect of rising labor costs.

Another important implication of equation 2 is that the change in wages depends on how exactly the work is distributed throughout the population, since this makes a difference for the income effects. When interpreting the subsequent empirical results, it is important to keep in mind that we are observing the equilibrium impacts of a particular (non-transparent) rationing rule for government employment, and this should be considered when using the results here to extrapolate to other situations.

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:

$$e(W, u_i) = \pi_i(W) + WT + (W_g - W)L_i^g + z_i \tag{3}$$

where z_i is exogenous income. Differentiating equation 3 yields:

$$\begin{aligned} -dz_i &= (L_i^s - L_i^g - D_i)W\frac{dW/W}{dL_g} + (W_g - W)dL_i^g \\ &= \textit{Net Casual Labor Earnings} \times \frac{dW/W}{dL_g} + (W_g - W)dL_i^g \end{aligned} \tag{4}$$

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].

3.5 Discussion and Extensions

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 discuss some of the assumptions and results of the framework presented above as well as some possible extensions.

3.5.1 Worker Productivity

Our analysis assumes that the workfare program does not directly increase workers' productivity. As a result any rise in wages represents a pure redistribution from employers to workers. To the extent that the program increases wages by changing worker productivity, equation **??** 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. However, it is possible that worker productivity 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 Welfare vs. Output and Consumption Effects

It is important to note that the impact on welfare is not the same as the impact on consumption. In Appendix Section C.1, we derive the impact on consumption of household i. The key difference compared with equation 4 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.

3.5.3 Impact on Prices and Second Order Effects

A closely related issue is that similar to the analyses in Deaton [1989], Deaton [1997], and Porto [2006], all of our results hold only for "small" increases in government employment. Large changes will have significant second order effects. Perhaps most importantly, output prices may change. 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 may disproportionately hurt the poor to the extent that they are net purchasers of food.

Second, the effect of the program on income may lead to a net rise or fall in total (public and private) labor supply. Specifically, as emphasized in the model, the income of net labor demanding households will fall leading to an increase in labor supply while the income of net labor supplying households and program participants will rise leading to a fall in labor supply. Similarly, the rise in wages could induce workers to increase or decrease labor supply. Both the price and labor supply effects may be important and are certainly interesting, however, in the interest of making progress, we ignore them in this analysis.

3.5.4 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].

Here, we consider one possible reason the opportunity cost of labor might fall below the private sector wage, stemming from frictions in the labor market. The analysis is similar to Basu et al. [2009]. In particular, suppose that a friction exists such that households that supply L days of labor to the labor market only receive p_iL days of work. 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 $F_i(\cdot) = A_iG(\cdot)$. There are three cases to consider. Households with a low productivity household production technology (low A_i) will be net labor supplying households and will face a marginal value of time of p_iW and therefore set $A_iG'(D_i) = p_iW$. These households are "under-employed" in the sense that their opportunity cost of leisure is less than the wage rate. 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 $A_iG'(D_i) \in [p_iW, W]$. Details of the proofs are given in Section C.2.

There are four 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. Third, for some households (those with zero net labor market supply), hiring them into a public works program will reduce output and total days worked but have no effect on observed wages. Finally, the impact of the workfare program on unemployment will depend critically on 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, then providing an additional day of work will reduce unemployment by one day with probability p_i. However, if workers report being unemployed because there is a temporary drop in demand for work, 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].

3.5.5 Productivity Heterogeneity across Workers

One justification for workfare programs is that only workers below a certain productivity choose to participate in them [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 wage with a much lower skill premium. As discussed in Section 2.2 above, there is indeed significant evidence of selection in that workers who participate in the workfare program are very unlikely to report also participating in salaried work in the past year (.1%), while 46% report usually or sometimes working in casual labor. Therefore, 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.

3.5.6 Imperfect Competition

We assume that the marginal productivity of labor is equal to the wage rate. Some observers 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:

$$F'(L^*) = W(L^*) + W'(L^*)L^* \tag{5}$$

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 5. 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 4 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 4 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.

3.5.7 Intra-Household Dynamics

Our model abstracts from intra-household dynamics. Specifically, we make the rather strong assumption that the labor supply decision of the entire household can be approximated using the unitary household model. In practice, this is unlikely to hold. For example, to the extent that the workfare program provides women with a chance to work that they would not normally have, the program may increase their bargaining power. We make this assumption not because we believe that intra-household dynamics are unimportant, but rather as a means to make progress on the problem of characterizing the equilibrium welfare impacts of workfare programs.

4 Data and Empirical Strategy

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

4.1 Data

We use two main sources of data in the analysis: nationally representative expenditure and employment household surveys 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 detail in the Appendix Section D. For the calibration in Section 6, we use the ARIS-REDS data set, which is described in detail in Appendix Section D.3.

We use the district as our primary unit of analysis and restrict the sample to adults aged 18 to 60 with secondary education or less. 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 Section 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.[8]

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 .1% percent report having migrated from a different rural district for employment within the past year.[9] 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 .11% of the total population of rural adults 18 to 60 with secondary education or less.[10] Low levels of migration are similarly documented in Munshi and Rosenzweig [2009] and Topalova [2010].

An important caveat is that the surveys used to measure migration may not fully capture short-term trips out of the village for work. Coffey et al. [2011] among others have documented that at least in some areas of India, short-term trips anywhere from two weeks to six months are common. Papp [2011] 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 short-term inter-district migration is common throughout India, our difference-in-differences estimates presented later will underestimate the true equilibrium impact on wages.

We use five rounds of the NSSO Employment and Unemployment survey (here on, "NSS Employment Survey"). The Employment survey is conducted from July to June in order to capture one full agriculture cycle and 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

[8] Authors' calculations using NSS Employment and Unemployment Survey Round 64 and 2001 census data. These data sets are described in detail in Secion 4.1.

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

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

later the extent to which this goal is accomplished in practice. The NSSO over-samples some types of households and therefore provides sampling weights.[11] 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. 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. 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 district-level 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. For each district-quarter we aggregate the person-level estimates using survey sampling weights to construct employment estimates at the district-quarter level. During the analysis, we weight each district using weights proportional to the total rural population in a district.

Our wage measures are computed as follows. Individuals who worked in casual labor over

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

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 then aggregate these estimates to the district-level using survey sampling weights. In the sequel, we make use of the individual-level controls by performing the wage analysis at the individual level.

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, it is possible that none of the surveyed adults worked in casual labor in which case 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 Section 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.

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, though these 150 districts show an increase in public works employment starting in January 2006 one month before the official start of the program. Our results are robust to adding a dummy variable for the pilot period.

Early phase districts were purposefully selected to have lower agricultural wages, a larger proportion of "backward" castes and lower agricultural output per worker [Gupta, 2006]. However, these targets were balanced by the goal of spreading early phase districts across states. As a result, some 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 distribution of early and late districts across India. Early districts are relatively well spread out, though there is a concentration of early districts in the Northern and Eastern parts of 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 include pre-program measures of the literacy rate, fraction scheduled tribe, fraction scheduled caste, poverty rate, population density, female and male labor force participation ratio, fraction of prime-age adults employed in agricultural casual labor, non-agricultural casual labor, cultivation, non-ag business, and salaried work, fraction of the labor force employed in agriculture, irrigated land per capita, and unirrigated cultivable land per capita. We interact these time-invariant controls with a dummy for post-program status to pick up trends correlated with the controls. We include time-varying controls for annual rainfall, dummy variables for whether annual rainfall was in the top or bottom quintile for long-run rainfall in the district, and a dummy variable for the one year preceding a state or local election.

Concern remains that program and control districts experience 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 2.5, 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 2.6, 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

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

where Y_{dt} is the outcome (e.g. earnings per day worked) for district d in quarter t, T_{dt} is a dummy for program districts in the post period (July 2007 to June 2008), X_{dt} are time-varying controls, Z_d are time-invariant controls, η_t are year-quarter fixed effects, and μ_i are district fixed effects. All estimates are adjusted for correlation of ϵ_{dt} over time within districts. For many of our specifications, we also include interactions of T_{dt} with other variables such as season dummies or dummies for whether the district is in a star state.

The simplest possible difference-in-differences estimator would restrict time trends to a pre and post dummy and would include only a dummy for whether a district was an early district rather than a full set of district fixed effects. Adding a full set of district fixed effects does not materially affect the results. However, the district fixed effects provide assurance that the results are not driven by the fact that the wage and to a lesser extent the employment panels are unbalanced.[12] Similarly, for the basic difference-in-differences specification, adding year-quarter fixed effects as opposed to simply one dummy for the post period July 2007 to June 2008 has little effect on the results. However, our main specification splits the program effect by season by replacing T_{dt} with $T_{dt} \times Dry_t$ and $T_{dt} \times Rainy_t$. If we do not control for seasonal variation, $T_{dt} \times Dry_t$ will pick up not only the impact of the program but also the long-run difference between dry and rainy seasons. Using quarter fixed effects or simply a dummy for season is appropriate if seasonality is the same each year. However, accelerating wage growth over the period introduces differential seasonality in the pre and post periods. As a result, a specification with a post dummy and season dummies will lead to an over-estimate of $T_{dt} \times Dry_t$ and an under-estimate of $T_{dt} \times Rainy_t$. For this reason, we use year-quarter dummies for the wage regressions. Because they do not materially change the employment results, for consistency we use year-quarter dummies in the employment regressions as well.

While most of our analysis relies on district-level aggregates, we also use the individual-level data to ease concerns that our results are driven by selection. If the program employs casual laborers with productivity lower than the average casual laborer, then observed average earnings of the remaining workers will rise even if the wages for the remaining workers

[12] Table A.1 shows the balance of the wage and employment panels.

remain constant. We estimate regressions analogous to the one above but at the individual level with controls for education, caste, religion, and age:

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

where H_i are controls for individual i surveyed in district d at time t. We re-weight observations so that the sum of all weights within a district-quarter is the same as the weights used in the district-level analysis (see Appendix Section D.4 for details). As before, standard errors are clustered at the district level.

5 Results

5.1 Summary Statistics

Table 1 presents the means of the main outcomes used in the paper by year. Table 2 presents the means for the controls used for early and late districts as well as districts in star states and other states during the pre-period. As expected given the criteria used to choose early districts, early districts are poorer based on every measure. Star states, on the other hand, seem to be slightly richer than other states.

Table 3 presents the means for the outcomes used in the paper for early and late districts as well as districts in star states and other states for the pre-period. The allocation of days between private sector work, public sector work, unemployment and out 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% higher 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 1.16 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 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. Regardless of the explanation, 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 2009-10 data.

Table 5 documents the heterogeneity in public works generation across states. While public employment in early districts of star states rises by 2 percentage points over the whole year, public employment rises by only .44 percentage points in other districts.

The specifications in Table 6 gradually build to the main specification with district and year-quarter fixed effects. The estimated impact of the program on the fraction of total time spent working in casual public employment over the whole year is .74 percentage points. The last column confirms that the rise in public works is concentrated during the dry season.

In gauging the magnitude of these effects, it is important to keep in mind that the coefficient on program represents the fraction of days spent in public works out of *all* days. Therefore, someone working five days a week would contribute only $5/7 = .71$. One useful metric is to compare the increase in public works to total private employment. On average, adults 18 to 60 with secondary education or less spent 90% of her time working in private employment (including domestic work). The rise in public employment therefore represents .78% of the private workforce.

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 7. The first four 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 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. However, given the large standard errors, the test lacks power.

Although the estimates are noisy, unemployment does not appear to fall in early districts relative to late districts. As discussed in Section 3.5.4, 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 in 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 selected to work for the program. The results for unemployment and not in the labor force should be interpreted with care given the difficulties in distinguishing between under-employed, unemployed, and not in the labor force.

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 $100 \times (.0148/.90)/\epsilon_d = \frac{1.6}{\epsilon_d}$ percent. In this section we present the differential trends in casual daily earnings for workers in early compared with late districts.

Table A.2 shows the results of the simple difference-in-differences exercise. The third row shows a general rise in wages across all districts, with the largest rise concentrated in the dry season in early districts. The difference-in-differences estimates in columns five and six confirm that during the dry season, wages in early districts rise relative to wages in late districts, with no differential change during the rainy season.

The first column of Table 8 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 4.5 log points more in early relative to late districts. During the rainy season, wages rise by a statistically insignificant .7 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 2.

The rise in wages could simply be the result of the program hiring low wage workers. Columns four and five show results using the person-level data with worker-level controls for age, caste, religion, and education in column five. To make sure that the results of the individual-level regressions are not driven by re-weighting of different districts based on the number of casual workers in a district, we adjust the weights for each individual so that the aggregate weights within each district-quarter matches the district-level regressions.[13] Column four shows that the re-weighting "works" in the sense that without person-level controls, the individual-level regressions match closely with the district-level regressions. Moving to column five, we see that person-level controls have little effect on the estimated coefficients.

As discussed in Section 2.2, less than .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] Column six of Table 8 presents 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 13%. This result suggests that the rise in casual wages is not part of general inflation across wages of all jobs. However, it does raise the concern that the estimated increase in casual wages may be an underestimate if the fall in salaried wages indicates a general negative demand side 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 4.5% and the fall in private sector work to compute a labor demand elasticity. The elasticity of labor demand is $\epsilon_d = \frac{1.6}{4.5} = .35$, which is in the same range as previous estimates from rural labor markets in India [Binswanger et al., 1984].

[13] Specifically, we multiply the weight for casual worker i in district d in quarter t by the district weight used in the district-level regressions divided by the sum of all weights for casual workers in district d in quarter t. See Appendix Section D.4 for more details.

[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].

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 9 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, which 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.

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 3.5%. The coefficients for other states are on the order of 1-3% and insignificant. The results are robust to adding person-level controls (column six), which provides some reassurance that the results are not driven by selection.

6 Estimating the Distributional Impact

Recall from Section 3 that the compensating differential for household i given by equation 4 and written here for convenience is

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

In this section, we use the estimates from the previous section combined with pre-program household-level labor supply and demand, 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 previous analysis suggests that the workfare program led to an increase in the wage for those employed in casual labor. This change benefits net labor suppliers and hurts net labor buyers. We use household-level data on labor supply and demand and consumption to determine the distributional implications of the wage change captured by the first term in equation 6. For the percent increase in wages $\frac{dW/W}{dL_g}$, we use 4.5% based on the estimates in Table 8.

Net casual labor earnings is more difficult 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 Section D.3 describes the ARIS/REDS data set in more detail.

It is not immediately clear how to use the answers from the 1999-00 ARIS/REDS data set to estimate casual labor payments for the 2004-05 NSS sample of households. The method we use is by no means free of problems but we think reasonable. Using the 1999-00 survey, we compute the total casual earnings for all households across all consumption quintiles. For each consumption quintile, we compute the total casual payments as a fraction of total casual earnings for all households across all quintiles. These fractions sum to less than one across consumption quintiles because some casual labor earnings come from urban employers. The resulting fractions are reported in the sixth row of Table 10. 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.

We next turn to the NSS Employment Survey data and multiply the fractions in row six by the total casual labor earnings across all consumption quintiles to get our estimate of casual labor payments by quintile. The results are presented in row seven. This method does not restrict casual labor earnings in rural areas to only come from rural employers and it allows for the fact that the total amount of casual labor payments is different in 1999-00 and 2004-05. However, we are forced to assume that the fraction of earnings paid by each consumption quintile is constant over this period.

We observe casual labor earnings directly in the NSS Employment Survey, and these are reported in the third row of Table 10. Net casual earnings (row eight) are given by total casual earnings (row three) less total casual payments (row seven). The resulting net gain from the wage change is .045 multiplied by net labor earnings for each quintile, presented in the tenth row. As expected, net casual earnings decreases as we move from the bottom to top quintiles.

6.2 Direct Gains from Participation

We next turn to quantifying the second term in equation 6, the direct gains for program participants. The welfare gain due to program participation is $(W_g - W)\Delta L_g$. We estimate ΔL_g, the increase in days worked for each consumption quintile by estimating our main specification with the program dummy interacted with a dummy for each consumption quintile. Ideally, we would use a direct measure of how many days households in each quintile worked. However, since public employment is non-zero in the pre-period, we instead estimate the change un public works by quintile. This method has the drawback that to the extent that the workfare program causes households to move from one quintile to another, the estimates will be biased. Given the estimated rise in wages and public employment, this bias is likely to be small. The increase in public works by consumption quintile is given in the eleventh row of Table 10.[15]

W_g is the 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. For the calibration we set the government wage to be 15% higher than the mean casual wage in 2004-05.

W is the value of a participant's next best option. In our model W is simply the casual wage rate. However, the outside option 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 30% of the government wage and one in which it is 80% of the government wage. The implied direct transfer $(W_g - W)\Delta L_g$ under these two assumptions is presented in rows 14 and 15 of Table 10.

6.3 Comparing Equilibrium and Direct Gains

Rows 17 and 18 of Table 10 present the total estimated gain for each consumption quintile assuming an outside option equal to 30% and 80% of the government wage. Rows 19 and 20 show the fraction of the total gain due to the equilibrium change in wages. For the three poorest quintiles, the equilibrium effect is between 16% and 54% of the total gain. Rows 21 and 22 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 small.

[15]The results for the main outcomes are presented by consumption quintile in Table A.4.

7 Conclusion

This paper provides some of the first evidence on the equilibrium impacts of workfare programs in a developing country context. These programs are commonly introduced with the goal of reducing poverty. While past empirical work focused on quantifying the direct income gains to participants from these programs, we estimate the equilibrium wage and employment effects as well. Our data allow us to estimate how these wage gains are distributed across the population.

Our results suggest that the welfare gains and losses from the rise in equilibrium wages are of the same magnitude as the direct income gains from participation. Further, the gains from the rise in wages disproportionately accrue to the poor. As a result, when evaluating the relative attractiveness of a workfare program compared with anti-poverty programs such as a cash transfer, it is important to consider potential equilibrium effects as well.

As an illustration, let us consider households from the lowest consumption quintile, which earn on average 732 Rupees from casual labor per month, and hire themselves casual labor for 102 Rupees per month. To them, the employment guarantee provides two more days of public employment per month. Direct gains from program participation are equal to two days of earnings on public works, minus the opportunity cost: their valuation could range from the wage premium to private work (14 Rupees) to the full public wage (121 Rupees). The equilibrium rise in wages implies that for holding private casual labor supply constant, households earn 4.5% more. This indirect gain is 28 Rupees per month, which lies between the bounds of direct gains from participation in the program.

Like many social programs in developing countries, workfare programs involve a transfer to the rural poor funded by (mostly urban) tax payer money. We show that through their effect on labor markets, they also 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 why the implementation of the Indian employment guarantee has been poor in some states (Bihar, Jharkhand, West Bengal) despite the large potential demand for public employment.

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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 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, 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].

In order to substantiate the claim that higher levels of public employment in star states is not due entirely to higher demand for public employment, we regress the change in public works employment in early districts from 2004-05 to 2007-08 on a dummy for whether a district is in a star state and a set of controls likely to be correlated with demand. The controls are deflated casual wages from the 2004-05 NSS Employment Survey, share of irrigated land, fraction of scheduled tribes from the 2001 census, literacy rate, poverty rate, female and male labor force participation, fraction of agricultural and non agricultural casual laborers, and fraction of labor force in agriculture. Table A.3 shows the results. The coefficient on star states is hardly effected by the addition of the controls. Further, the predictors of demand explain surprisingly little of the variation in actual provision across districts. The dummy for whether a district is in a star state alone explains more variation than all of the predictors of demand.

These results are supportive evidence that supply-side factors such as administrative capacity and/or political will were important factors in the variation in employment generation across districts. When we add square terms (third and fourth column of Table A.3), the overall conclusion does not change. However, we find some evidence of a "hump shaped" relation between poverty and public employment. The marginal effect of poverty ratio becomes negative at the 85th poverty percentile of phase 1 and 2 districts. This suggests that both the richest districts, which have presumably lower demand, and the ultra-poor districts, which are constrained by administrative capacity, have lower employment generation.

C Theoretical Appendix

C.1 Impact on Household Consumption

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

$$c_i = \pi_i(W) + WL_i^s(W, y_i) + (W_g - W)L_i^g \tag{7}$$

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

$$\begin{aligned}\frac{dc_i}{dL^g} &= (W_g - W)\frac{dL_i^g}{dL^g} \\ &+ WL_{yi}^s(W_g - W)\frac{dL_i^g}{dL^g} \\ &+ (L_i^s - D_i - L_i^g)\frac{dW}{dL^g} \\ &+ W\Big[\frac{dL_i^s}{dW}|_u + L_{y_i}^s(L_i^s + T - L_i^g - D_i)\Big]\frac{dW}{dL^g}\end{aligned} \tag{8}$$

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 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.2 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 $p_i L$ 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 $F_i(\cdot) = A_i G(\cdot)$ 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 $p_i W$ and therefore set $A_i G'(D_i) = p_i W$. 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 $A_i G'(D_i) = W$. 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 $A_i G'(D_i) \in [p_i W, W]$.

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

$$\max_{L,c} u(c, T - L) \tag{9}$$

$$\text{s. t. } c + \tilde{W}(T - L) = Y + \tilde{W} L \tag{10}$$

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

$$D_e = D(W, A_e) \tag{11}$$

$$L^s(W, A_e G'(D_e)) = D_e \tag{12}$$

Note that since $L_Y^s \leq 0$ 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

$$D_w = D(p_i W, A_w) \tag{13}$$

$$L^s(p_i W, A_e G'(D_w)) = D_w \tag{14}$$

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 $D(p_i W, A_i) >$

$L^s(pW, A_iG'(D(p_iW, A_i)))$, so that the household will be a net labor supplier. Net labor suppliers will always face an effective marginal wage of p_iW. For a household with $A_i > A_w$, we will have $D(p_iW, A_i) < L^s(pW, A_iG'(D(p_iW, A_i)))$, so that the household will not be 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 $D = L^s(A_iG'(D), A_iG(D))$. Note that for $A_i \in [A_w, A_e]$, the labor supply and demand will satisfy $A_iG'(D) \in [p_iW, W]$.

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 year 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-quater.

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 5% 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.

Variables: Our main outcomes are district-level 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: non-government work, public works, not in the labor force, and unemployed. For each district-quarter we then aggregate the person-level estimates using survey sampling weights to construct employment estimates at the district-quarter level. During the analysis, we weight each district using weights proportional to total rural population.

Our wage measures are computed as follows. 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 then aggregate these estimates to the district-level using survey sampling weights. In order to be able to use workers controls, we also perform the wage analysis at the individual level. We re-weight individual observations so that the sum of all weights within a district-quarter is the same as for the district-level regressions. See Appendix Section D.4 for further discussion of the weighting.

D.2 District Controls

Table 2 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 "unirrigated 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. Unfortunately we could not obtain information on 2003 rainfall from the IMD website. This is why we also use University of Delaware Air Temperature & Precipitation data provided by the NOAA/OAR/ESRL PSD, Boulder, Colorado, USA.[16] Cort Willmott & Kenji Matsuura 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 until 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 2003-2010 dataset, we constructed three control variables. “Rainfall annual” is simply the sum of precipitations over the last 12 months in mm. “Rainshock good” (“rainshock bad”) are dummies that take the value one if “rainfall annual” is above (below) the 80th (20th) percentile of the distribution of “rainfall annual” in the district since 1975.

Elections "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[17] and from the State Election Commissions of all states.

South “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

[16] http://www.esrl.noaa.gov/psd/
[17] http://www.eci.nic.in/eci_main1/index.aspx

Research (Delhi) in 1999-00.[18] The ARIS-REDS survey covers a nationally representative rural sample of Indian households, with detailed information both 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 quintile, we then sum casual labor costs, which we divide by total income from casual labor over the whole population to obtain the "fraction of casual labor costs paid by quintile." Note that we do not use sample weights for this aggregation. We plan to incorporate sampling weights after obtaining IRB approval.

D.4 Weighting

When constructing estimates at the district-level, we use survey sample weights to aggregate variables to the district-quarter level as outlined in National Sample Survey Office [2010]. Using survey sample weights ensures that the outcome for each district-quarter is an unbiased estimate of the average of the outcome for the population. When performing the district-level regressions, we weight observations within each district using weights proportional to the rural population of the district as estimated from the NSS Employment Surveys. These weights are time-invariant, so that our results are not driven by changing weights over time. 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. When we move to the individual-level regressions, we re-weight so that the sum of all weights within a district-quarter is the same as for the district-level regressions. This re-weighting ensures that the individual-level results are not driven by a change in weighting. 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

[18] `http://adfdell.pstc.brown.edu/arisreds_data/readme.txt`

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 used in the district-level regressions.

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.

Figure 1: Map of Early and Late Districts

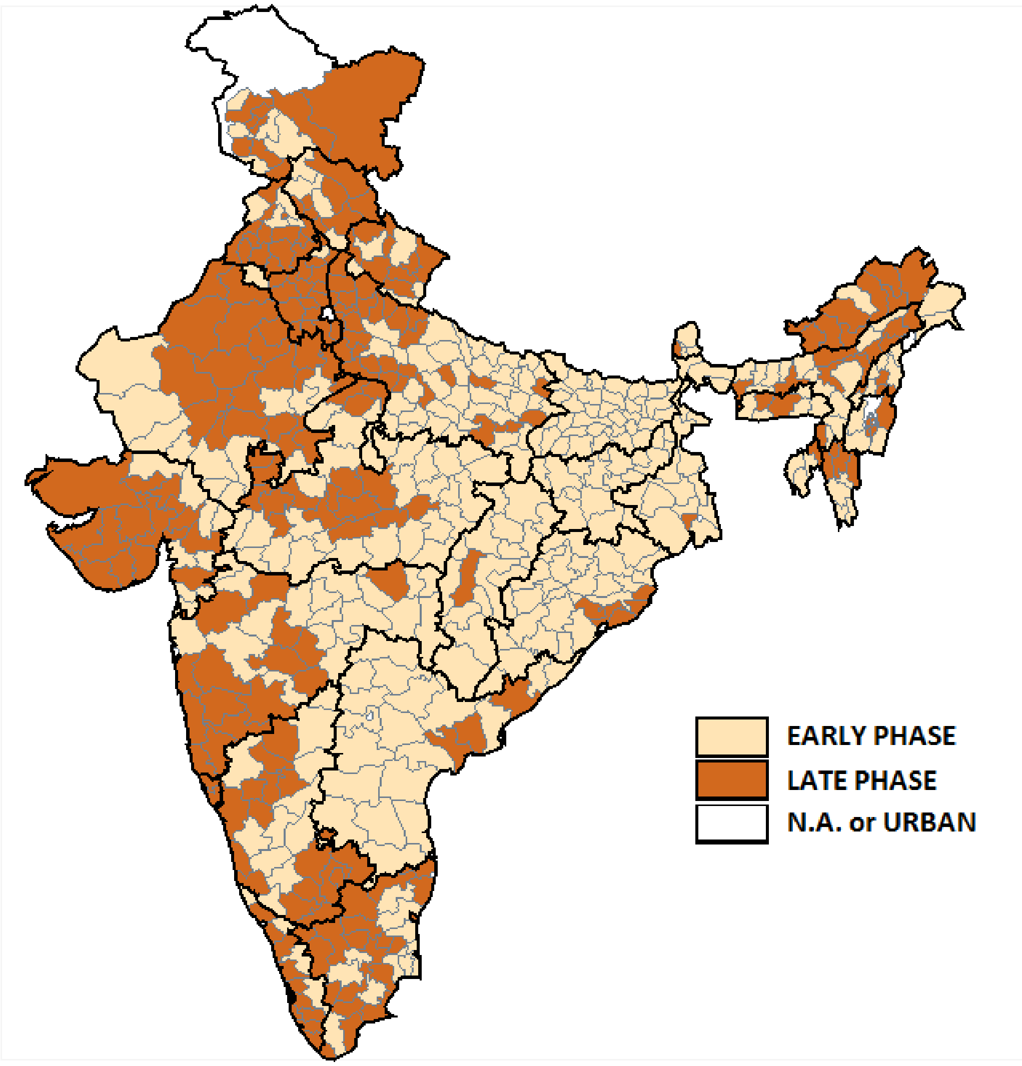

Table 1: Outcome Summary Statistics by Agricultural Year

Private Sector Work	0.8505	0.9433	0.8524	0.9451	0.8619	0.9469
Cultivator	0.3495	0.1931	0.3442	0.1859	0.3577	0.1626
Non-Ag Self-employed	0.1416	0.0311	0.1446	0.0367	0.1264	0.0276
Casual Labor	0.2504	0.1034	0.2545	0.1057	0.2803	0.1122
Salaried Work	0.0683	0.0149	0.0774	0.0158	0.0697	0.0156
Domestic Work	0.0101	0.3318	0.0075	0.3221	0.0081	0.3783
Other Work	0.0240	0.0128	0.0198	0.0120	0.0156	0.0076
Unemployed	0.0776	0.0303	0.0733	0.0280	0.0733	0.0243
Not in Labor Force	0.0686	0.0258	0.0709	0.0260	0.0613	0.0276
Log daily Casual Earnings	3.93	3.58	3.97	3.61	4.20	3.87
Deflated Daily Casual Earnings	2.72	2.37	2.72	2.37	2.78	2.46

Employment outcomes are constructed as follows. Respondents were asked what they did on each of the past seven days. Using answers to this question, the fraction of days spent in a given activity is computed for each individual. The wage estimates are computed as follows. Respondents were asked what they did on each of the past seven days and the earnings from those activities. For those who spent time in casual labor, the total earnings are divided by the number of days worked. Individual employment and wage estimates are aggregated to the district-quarter level using survey sampling weights. Estimates at the year level are computed by aggregating the district-quarter estimates using district population weights. The sample is restricted to persons aged 18 to 59 with secondary education or less. Not in the Labor Force includes persons who earned income from non-labor sources, sick or disabled persons, persons in school and persons who reported having work but did not to work. Deflated earnings are deflated using the monthly, state-level price index for agricultural labourers from the Indian Labour Bureau. Years correspond to agricultural years (e.g. 2004-05 is July 2004 to June 2005).

Table 2: District Controls Summary Statistics

Table 2
District Controls Summary Statistics

	Early (1)	Late (2)	Difference (1) - (2) (3)	Star States (4)	Other States (5)	Difference (4) - (5) (6)	Source (7)	Time-varying? (8)
Literacy Rate	0.555	0.651	-0.096***	0.583	0.595	-0.012	2001 Census	No
Fraction SC	0.188	0.174	0.015*	0.182	0.183	-0.001	2001 Census	No
Fraction ST	0.133	0.048	0.085***	0.150	0.081	0.068***	2001 Census	No
Poverty Rate	0.320	0.209	0.111***	0.228	0.298	-0.069***	NSS	No
Population Density (per sq. km)	483	405	78***	239	540	-300.675***	2001 Census	No
Irrigated Cultivable Land per Capita (ha)	0.083	0.118	-0.035***	0.118	0.087	0.031***	2001 Census	No
Unirrigated Cultivable Land per Capita (ha)	0.175	0.173	0.002	0.239	0.149	0.091***	2001 Census	No
In South	0.197	0.278	-0.081**	0.491	0.123	0.368***		No
Female Labor Force Participation Ratio	0.379	0.367	0.012	0.502	0.323	0.179***	2001 Census	No
Male Labor Force Participation Ratio	0.634	0.629	0.005	0.662	0.621	0.042***	2001 Census	No
Fraction Ag Casual Laborers	0.197	0.162	0.035***	0.237	0.162	0.075***	NSS	No
Fraction Non-Ag Casual Labor	0.047	0.066	-0.019***	0.060	0.052	0.009**	NSS	No
Fraction Cultivators	0.274	0.253	0.021*	0.318	0.245	0.073***	NSS	No
Fraction Non-Ag Business	0.091	0.090	0.001	0.087	0.092	-0.006	NSS	No
Fraction Salaried Work	0.046	0.073	-0.027***	0.060	0.055	0.006	NSS	No
Fraction Labor Force in Agriculture	0.758	0.665	0.093***	0.776	0.701	0.075***	2001 Census	No
Annual Rainfall (mm)	3,345	3,277	68	2,737	3,553	-816.379***	IMD	Yes
Rain Shock Bad	0.142	0.182	-0.04**	0.183	0.147	0.037*	IMD	Yes
Rain Shock Good	0.270	0.165	0.105***	0.174	0.253	-0.079***	IMD	Yes
Election Year	0.526	0.344	0.182***	0.374	0.490	-0.116**		Yes
Number of Districts	286	207		143	350			

With the exception of the poverty rate, controls constructed using NSS use data from Rounds 60, 61, 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 usual activity for adults 18 to 59 only. Literacy and labor force participation are restricted to persons over the age of six. Rain shock good and bad are dummy variables indicating whether annual rainfall for a district is above the 80th percentile or below the 20th percentile for that district. Election Year is a dummy variable indicating that State or local (Panchayat) elections are to be held in the proceeding year. Star states rank high for implementation of the program. Star states include Andhra Pradesh, Madhya Pradesh, Tamil Nadu, Rajasthan, and Chhatisgarh. Lagging states fall at the bottom for implementation of the program. Lagging states include Bihar, Jharkhand, Karnataka and Maharashtra. Middle states include Gujarat, Orissa, West Bengal, and Uttar Pradesh.

Table 3: Summary Statistics of Outcomes in 2004, 2005 for Early and Late Districts

Table 3
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	0.848	0.018***	0.850	0.864	-0.014***
Cultivator	0.373	0.337	0.036***	0.364	0.357	0.007
Non-Ag Self-employed	0.142	0.143	-0.001	0.122	0.151	-0.028***
Casual Labor	0.259	0.250	0.009	0.267	0.251	0.016**
Salaried Work	0.055	0.091	-0.036***	0.070	0.068	0.002
Domestic Work	0.010	0.007	0.003***	0.006	0.010	-0.004***
Unemployed	0.070	0.081	-0.012***	0.086	0.070	0.016***
Not in Labor Force	0.062	0.069	-0.007***	0.061	0.066	-0.005*
Log Daily Casual Earnings	3.848	4.094	-0.246***	3.902	3.957	-0.054***
Deflated Daily Casual Earnings	2.630	2.854	-0.224***	2.665	2.734	-0.069***

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	0.940	0.937	0.003	0.914	0.948	-0.034***
Cultivator	0.183	0.213	-0.03***	0.256	0.170	0.086***
Non-Ag Self-employed	0.040	0.038	0.003	0.059	0.031	0.027***
Casual Labor	0.108	0.105	0.003	0.152	0.089	0.063***
Salaried Work	0.013	0.019	-0.006***	0.019	0.013	0.005**
Domestic Work	0.292	0.296	-0.004	0.241	0.314	-0.073***
Unemployed	0.030	0.035	-0.005**	0.052	0.024	0.027***
Not in Labor Force	0.029	0.027	0.002	0.031	0.027	0.004**
Log daily Casual Earnings	3.515	3.681	-0.166***	3.495	3.614	-0.12***
Deflated Daily Casual Earnings	2.296	2.439	-0.143***	2.256	2.392	-0.136***

The sample is restricted to persons aged 18 to 59 with secondary education or less. Employment and wage outcomes are constructed as follow. Respondents were asked what they did on each of the past seven days and the earnings from those activities. Using answers to this question, the fraction of days spent in a given activity is computed for each individual. For wage earnners, total earnings are divided by the number of days worked. Individual employment and wage estimates are aggregated to the district-quarter level using survey sampling weights. "Not in the Labor Force" includes persons who did domestic work, who earned income from non-labor sources, sick or disabled persons, persons in school and persons who reported having work but did not to work. Earnings are deflated using price index for agricultural labourers (Indian Labour Bureau).

Table 4: Public Works Difference in Differences Estimates

		Rainy Jul to Dec (1)	Dry Jan to Jun (2)	Rainy Jul to Dec (3)	Dry Jan to Jun (4)	Rainy Jul to Dec (5)	Dry Jan to Jun (6)
(1)	Pre (1/04 to 12/05)	0.0012 (0.0003)	0.0026 (0.0005)	0.0008 (0.0002)	0.0028 (0.0007)		
(2)	Post (2007-08)	0.0035 (0.0006)	0.0142 (0.0026)	0.0009 (0.0004)	0.004 (0.0011)		
(3)	(2) - (1)	0.0024*** (0.0007)	0.0116*** (0.0026)	0.0001 (0.0004)	0.0012 (0.0012)	0.0022*** (0.0008)	0.0104*** (0.0021)
(4)	Post (2009-10)	0.0092 (0.0015)	0.0177 (0.0032)	0.0062 (0.0014)	0.0116 (0.0027)		
(5)	(4) - (1)	0.008*** (0.0015)	0.0151*** (0.0032)	0.0054*** (0.0014)	0.0088*** (0.0028)	0.0026* (0.0016)	0.0062** (0.0026)
	Observations	286	286	207	207		

Each observation corresponds to a district-quarter. The outcome Public Works employment is computed as follows. Respondents were asked what they did on each of the past seven days. Using answers to this question, the fraction of days spent in public works is computed for each individual. These individual estimates are then aggregated to the district-quarter level using survey sampling weights. All estimates are computed using weights proportional to district population. 2007-08 and 2009-10 correspond to agricultural years (July to June). The public works program was introduced in early districts between April 2006 and April 2007. The program was introduced to late districts in April 2008. Standard errors in parentheses adjusted for correlation of the errors at the district level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 5: Public Works Difference-in-Differences Estimates by Implementation Group

Table 5
Public Works Difference-in-Differences Estimates by Implementation Group

		Star States			Other States		
		Early (1)	Late (2)	Diff-in-Diff (3)	Early (4)	Late (5)	Diff-in-Diff (6)
(1)	Pre (1/04 to 12/05)	0.0037	0.0021		0.0012	0.0017	
		(0.0009)	(0.0005)		(0.0002)	(0.0005)	
(2)	Post (2007-08)	0.024	0.0065		0.0026	0.0008	
		(0.0045)	(0.0017)		(0.0004)	(0.0003)	
(3)	(2) - (1)	0.0203***	0.0044***	0.0159***	0.0014***	-0.0009	0.0025***
		(0.0045)	(0.0016)	(0.0034)	(0.0005)	(0.0006)	(0.0007)
(4)	Post (2009-10)	0.0304	0.0219		0.0067	0.0034	
		(0.0052)	(0.0049)		(0.0012)	(0.0009)	
(5)	(4) - (1)	0.0267***	0.0198***	0.0068	0.0055***	0.0018***	0.0037***
		(0.0053)	(0.0048)	(0.0044)	(0.0012)	(0.001)	(0.001)
	Districts	83	60		172	147	

See Table 2 for definitions of star, middle and lagging states. 2007-08 and 2009-10 correspond to agricultural years (July to June). The public works program was introduced in early districts between April 2006 and April 2007. The program was introduced to late districts in April 2008. Standard errors in parentheses adjusted for correlation of the errors at the district level. The outcome Public Works employment is computed as follows. Respondents were asked what they did on each of the past seven days. Using answers to this question, the fraction of days spent in public works is computed for each individual. These individual estimates are then aggregated to the district-quarter level using survey sampling weights. All estimates are computed using weights proportional to district population. * p < .10, ** p < .05, *** p < .01.

Table 6: Public Works (First Stage)

Table 6
Public Works (First Stage)

	Public Work (1)	Public Work (2)	Public Work (3)	Public Work (4)	Public Work (5)
Program	0.00691***	0.00692***	0.00627***	0.00741***	
	(0.00143)	(0.00148)	(0.00163)	(0.00197)	
Program X Dry					0.0113***
					(0.00313)
Program X Rainy					0.00351**
					(0.00139)
Observations	5,784	5,784	5,784	5,740	5,740
R-squared	0.020	0.164	0.180	0.194	0.197
District FE	No	Yes	Yes	Yes	Yes
Quarter*Year FE	No	No	Yes	Yes	Yes
District Controls	No	No	No	Yes	Yes

Each observation corresponds to a district-quarter. The outcome Public Works employment is computed as follows. Respondents were asked what they did on each of the past seven days. Using answers to this question, the fraction of days spent in public works is computed for each individual. These individual estimates are then aggregated to the district-quarter level using survey sampling weights. All estimates are computed using weights proportional to district population. Controls are listed in table 2. Controls that do not vary over time are interacted with a dummy for 2007-08 (post-program). Dry is a dummy variable equal to one for the first two quarters of the year. Rainy is equal to one for the second two quarters of the year. * p < .10, ** p < .05, *** p < .01.

Table 7: Main Specification Time Allocation

Table 7
Main Specification Time Allocation

	Public	Private	Unemployed	Not in Labor Force	Public	Private	Unemployed	Not in Labor Force
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Program X Dry	0.0101***	-0.0221***	0.0153***	-0.00332	0.0113***	-0.0148**	0.00670	-0.00323
	(0.00290)	(0.00644)	(0.00437)	(0.00329)	(0.00313)	(0.00698)	(0.00465)	(0.00370)
Program X Rainy	0.00246***	-0.00485	0.00766*	-0.00527*	0.00351**	0.00257	-0.00139	-0.00468
	(0.000882)	(0.00517)	(0.00450)	(0.00275)	(0.00139)	(0.00621)	(0.00507)	(0.00331)
Observations	5,784	5,784	5,784	5,784	5,740	5,740	5,740	5,740
District Controls	No	No	No	No	Yes	Yes	Yes	Yes

All regressions include district and year x quarter fixed effects. Observations are at the district-quarter level. The employment outcomes are computed as follows. Respondents were asked what they did on each of the past seven days. Using answers to this question, the fraction of days spent in a given activity is computed for each individual. Individual employment estimates are aggregated to the district-quarter level using survey sampling weights. 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. Controls are listed in Table 2. Controls that do not vary over time are interacted with a dummy for 2007-08 (post-program). * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 8: Main Specification Daily Earnings

Table 8
Main Specification Daily Earnings

	Daily Casual Earnings	District level Deflated Daily Casual Earnings	Deflated Daily Casual Earnings	Deflated Daily Casual Earnings	Individual level Deflated Daily Casual Earnings	Deflated Salaried Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Program X Dry	0.0449**	0.0398**	0.0563***	0.0561***	0.0468***	-0.136**
	(0.0188)	(0.0192)	(0.0200)	(0.0188)	(0.0173)	(0.0533)
Program X Rainy	0.00688	0.0000462	0.0180	0.0182	0.0264	-0.0307
	(0.0192)	(0.0200)	(0.0208)	(0.0195)	(0.0180)	(0.0546)
Observations	5,574	5,574	5,535	83,532	83,474	16,979
District Controls	No	No	Yes	Yes	Yes	Yes
Worker Controls	No	No	No	No	Yes	Yes

All regressions include district and year-quarter fixed effects. Observations are at the district-quarter level. The wage outcomes are computed as follows. Respondents were asked what they did on each of the past seven days and the earnings from those activities. For those who spent time in casual labor, the total earnings are divided by the number of days worked. Individual employment and wage estimates are aggregated to the district-quarter level using survey sampling weights. All estimates are computed using weights proportional to district population. Deflated earnings are deflated using the monthly, state-level price index for agricultural labourers from the Indian Labour Bureau. 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 2. Controls that do not vary over time are interacted with a dummy for 2007-08 (post-program). Individual controls are dummies for workers' marital status, age (by brackets of ten years), caste (ST, SC, OBC), religion (Hinduism, Islam, Buddhism, Jainism, Sikhism, Christianism, Zoroastrism) and education (Illiterate, Below Primary, Primary, Middle, Secondary), and the fraction of working time in the agricultural sector. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 9: Program Effects by Implementation Group

Table 9
Program Effects by Implementation Group

	Public	Private	Unemployed	Not in Labor Force	Deflated Daily Earnings	Deflated Daily Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Program X Dry X Star States	0.0353***	-0.0366***	0.00464	-0.00339	0.106***	0.0871***
	(0.00835)	(0.0121)	(0.00751)	(0.00533)	(0.0265)	(0.0239)
Program X Rainy X Star States	0.00507**	0.000978	-0.00247	-0.00357	0.0356	0.0469*
	(0.00222)	(0.00890)	(0.00758)	(0.00459)	(0.0271)	(0.0247)
Program X Dry X Other States	0.0000669	-0.00457	0.00772	-0.00322	0.0318	0.0265
	(0.00174)	(0.00708)	(0.00509)	(0.00406)	(0.0203)	(0.0191)
Program X Rainy X Other States	0.00158	0.00440	-0.000789	-0.00519	0.00717	0.0144
	(0.00129)	(0.00664)	(0.00537)	(0.00372)	(0.0210)	(0.0196)
Observations	5,740	5,740	5,740	5,740	83,532	83,474
District Controls	Yes	Yes	Yes	Yes	Yes	Yes
Worker Controls	No	No	No	No	No	Yes

All regressions include district and year-quarter fixed effects. Observations are at the district-quarter level. The sample is restricted to persons aged 18 to 59 with secondary education or less. Employment and wage outcomes are constructed as follow. Respondents were asked what they did on each of the past seven days and the earnings from those activities. Using answers to this question, the fraction of days spent in a given activity is computed for each individual. For wage earnners, total earnings are divided by the number of days worked. Individual employment and wage estimates are aggregated to the district-quarter level using survey sampling weights. 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. Controls are listed in Table 2. Controls that do not vary over time are interacted with a dummy for 2007-08 (post-program). See the text for the definition of "Star" States. * p < .10, ** p < .05, *** p < .01.

Table 10: Calibration

		Consumption Quintile						
		Poorest	Second	Third	Fourth	Richest	Average	Construction
Household Expenditures and Income								
1	Monthly Consumption Per Capita	316.5	465.6	599.1	794.2	1446	519.7	NSS 2004-5
2	Total Monthly Consumption	1823	2340	2688	3163	4977	2430	NSS 2004-5
3	Total Earnings per Month for Adults doing Casual Labor	732	593	458	338	185	575	NSS 2004-5
4	Casual Earnings as Fraction of Household Consumption	0.40	0.25	0.17	0.11	0.04	0.24	NSS 2004-5
5	Average Earnings per Day Worked by Adults	43.72	47.59	51.14	56.36	64.54	46.66	NSS 2004-5
Gain from Wage Change								
6	Fraction of Casual Labor Costs Paid by Quintile	0.04	0.08	0.11	0.18	0.40	0.14	NCAER 1999
7	Estimated Monthly Labor Cost per Household	102	237	324	519	1136	398	(6) x Average(3) x 5
8	Net Labor Earnings per Month	630	356	134	-180	-951	177	(3) - (7)
9	Wage change	0.045	0.045	0.045	0.045	0.045	0.045	Estimated
10	Net Gain from Wage Change	28.33	16.00	6.02	-8.12	-42.79	7.96	(8) x (9)
Direct Gain from Government Employment								
11	Increase in Days in Public Employment per Household per M	2.05	0.79	0.74	0.52	0.34	0.89	Estimated
12	Average Private Sector Wage	43.72	47.59	51.14	56.36	64.54	46.66	NSS 2004-5
13	Government Wage	53.66	53.66	53.66	53.66	53.66	53.66	Private + 15%
14	Direct Gain assuming Outside Option is Predicted Wage	20.41	4.80	1.85	-1.40	-3.67	6.21	(11) x [(13)-(12)]
15	Direct Gain assuming Outside Option is Zero	110.17	42.44	39.50	27.91	18.12	47.63	(11) x (13)
Total Gain								
16	Total Gain assuming Outside Option is Predicted Wage	48.73	20.80	7.87	-9.52	-46.46	14.17	(10) + (14)
17	Total Gain assuming Outside Option is Zero	138.50	58.43	45.52	19.79	-24.67	55.59	(10) + (15)
Gain from Wage Change as Fraction of Total								
19	Assuming Outside Option is Predicted Wage	0.58	0.77	0.76	--	--	0.56	(10)/(16)
20	Assuming Outside Option is Zero	0.20	0.27	0.13	--	--	0.14	(10)/(17)
Total Gain as Fraction of Total Expenditures								
21	Assuming Outside Option is Predicted Wage	0.03	0.01	0.00	0.00	-0.01	0.01	(10)/(2)
22	Assuming Outside Option is Zero	0.08	0.02	0.02	0.01	0.00	0.02	(10)/(2)

The last columns indicates how each figure is obtained. "NSS 2004-5" are averages of each variable per quintile of monthly per capita expenditure, using sample weights. The fraction of casual labor costs paid by quintile is computed using data from the NCAER-REDS survey of 1999. First we use monthly per capita expenditure to define quintiles. Second, by quintile, we aggregate all wages paid by the household to adult laborers, for agricultural and non agricultural work. Third, we aggregate all income from agricultural and non agricultural casual labor supplied outside the household by all adults aged 18 to 60. The outcome is obtained for each quintile by dividing total wages paid with total wage income received across all households. "Wage change" is equal to the estimate of program impact in the dry season from individual level regressions with workers controls described in the text. "Increase in Days in public employment per household per month" is obtained by household level regressions of the total fraction of days workerd per month on public works on program dummy interacted with a dry season dummy and a dummy for each quintile, using sample weights.

Table A.1: Balance of District Panel

Table A1
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	438	447
2006-07	--	--	--	--
2007-08	493	493	491	493
2008-09	--	--	--	--
2009-10	493	493	492	493
Casual Wages				
2003-04	--	--	471	470
2004-05	475	477	475	479
2005-06	397	412	412	416
2006-07	--	--	--	--
2007-08	477	479	482	480
2008-09	--	--	--	--
2009-10	472	472	471	476

Each cell shows the number of districts 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 acheived. Casual wages are only available for districts in which at least one person reports working in casual labor.

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

Table A2
Log Deflated Casual Earnings Difference-in-Difference Estimates

		Early Districts		Late Districts		Diff-in-Diff	
		Rainy Jul to Dec (1)	Dry Jan to Jun (2)	Rainy Jul to Dec (3)	Dry Jan to Jun (4)	Rainy Jul to Dec (5)	Dry Jan to Jun (6)
(1)	Pre (1/04 to 12/05)	2.5348	2.5596	2.731	2.7722		
		(0.0157)	(0.0148)	(0.0277)	(0.0251)		
(2)	Post (2007-08)	2.6197	2.6993	2.8188	2.8653		
		(0.0153)	(0.0142)	(0.0276)	(0.0266)		
(3)	(2) - (1)	0.0848***	0.1397***	0.0878***	0.0931***	-0.003	0.0466*
		(0.0141)	(0.0132)	(0.0159)	(0.0142)	(0.0276)	(0.0252)
(4)	Post (2009-10)	2.6928	2.79	2.909	2.9574		
		(0.0164)	(0.0167)	(0.0273)	(0.0303)		
(5)	(4) - (1)	0.158***	0.2305***	0.178***	0.1852***	-0.0201	0.0452*
		(0.0165)	(0.0165)	(0.0196)	(0.0183)	(0.0283)	(0.0267)
	Observations	286	286	207	207		

Each observation corresponds to a district-quarter. The wage outcomes are computed as follows. Respondents were asked what they did on each of the past seven days and the earnings from those activities. For those who spent time in casual labor, the total earnings are divided by the number of days worked. Individual employment and wage estimates are aggregated to the district-quarter level using survey sampling weights. All estimates are computed using weights proportional to district population. Deflated earnings are deflated using the monthly, state-level price index for agricultural labourers from the Indian Labour Bureau. 2007-08 and 2009-10 correspond to agricultural years (July to June). The public works program was introduced in early districts between April 2006 and April 2007. The program was introduced to late districts in April 2008. Standard errors in parentheses adjusted for correlation of

Table A.3: Are Star State Results Driven by Demand for Public Employment?

Table A3
District Hetereogeneity in Government Work

	Public Work (1)	Public Work (2)	Public Work (3)
Star States	0.0360***	0.0410***	0.0410***
	(0.00413)	(0.00320)	(0.00425)
Poverty Rate		0.0250*	0.0879**
		(0.0132)	(0.0323)
Fraction ST		-0.0197*	-0.0292
		(0.0108)	(0.0273)
Literacy Rate		0.0124	-0.0332
		(0.0263)	(0.0941)
Irrigated Cultivable Land per Capita		-0.0218**	-0.0563*
		(0.00846)	(0.0319)
Fraction Labor Force in Agriculture		-0.0281	0.186*
		(0.0385)	(0.0923)
Fraction of Labor force in Ag. Casual Work		0.00231	-0.121
		(0.0217)	(0.0924)
Fraction of Labor force in Non Ag. Casual Work		-0.0647	-0.102
		(0.0411)	(0.133)
Log Casual Wage in 2004-5		-0.0229	-0.0652
		(0.0195)	(0.116)
Female Labor Force Participation Ratio		0.0116	-0.150
		(0.0164)	(0.125)
Male Labor Force Participation Ratio		-0.0754*	0.986
		(0.0398)	(1.043)
Poverty Rate (Square)			-0.0740**
			(0.0312)
Fraction ST (Square)			0.00432
			(0.0393)
Literacy Rate (Square)			0.0255
			(0.0948)
Irrigated Cultivable Land per Capita (Square)			0.0432
			(0.0316)
Fraction Labor Force in Agriculture (Square)			-0.181*
			(0.0944)
Fraction of Labor Force doing Ag. Casual Work (Square)			0.282
			(0.223)
Fraction of Labor Force doing Non Ag. Casual Work (Square)			0.0244
			(0.766)
Log Casual Wage in 2004-5 (Square)			0.0526
			(0.112)
Female Labor Force Participation Ratio (Square)			0.238
			(0.182)
Male Labor Force Participation Ratio (Square)			-0.824
			(0.839)
Phase==2	0.00535	0.00764	0.00871
	(0.00414)	(0.00532)	(0.00549)
Observations	571	569	569
R-squared	0.105	0.132	0.161

Each observation corresponds to a district-quarter. The sample is composed of all Early Phase districts in the agricultural year 2007-8 (NSS Round 64). The outcome Public Works employment is computed as follows. Respondents were asked what they did on each of the past seven days. Using answers to this question, the fraction of days spent in public works is computed for each individual. These individual estimates are then aggregated to the district-quarter level using survey sampling weights. See the text for the definition of "Star" States. Standard errors are clustered at the state level. * p < .10, ** p < .05, *** p < .01.

Table A.4: Outcomes by Quintile

Table A3
District Hetereogeneity in Government Work

	Public Work (1)	Public Work (2)	Public Work (3)
Star States	0.0360***	0.0410***	0.0410***
	(0.00413)	(0.00320)	(0.00425)
Poverty Rate		0.0250*	0.0879**
		(0.0132)	(0.0323)
Fraction ST		-0.0197*	-0.0292
		(0.0108)	(0.0273)
Literacy Rate		0.0124	-0.0332
		(0.0263)	(0.0941)
Irrigated Cultivable Land per Capita		-0.0218**	-0.0563*
		(0.00846)	(0.0319)
Fraction Labor Force in Agriculture		-0.0281	0.186*
		(0.0385)	(0.0923)
Fraction of Labor force in Ag. Casual Work		0.00231	-0.121
		(0.0217)	(0.0924)
Fraction of Labor force in Non Ag. Casual Work		-0.0647	-0.102
		(0.0411)	(0.133)
Log Casual Wage in 2004-5		-0.0229	-0.0652
		(0.0195)	(0.116)
Female Labor Force Participation Ratio		0.0116	-0.150
		(0.0164)	(0.125)
Male Labor Force Participation Ratio		-0.0754*	0.986
		(0.0398)	(1.043)
Poverty Rate (Square)			-0.0740**
			(0.0312)
Fraction ST (Square)			0.00432
			(0.0393)
Literacy Rate (Square)			0.0255
			(0.0948)
Irrigated Cultivable Land per Capita (Square)			0.0432
			(0.0316)
Fraction Labor Force in Agriculture (Square)			-0.181*
			(0.0944)
Fraction of Labor Force doing Ag. Casual Work (Square)			0.282
			(0.223)
Fraction of Labor Force doing Non Ag. Casual Work (Square)			0.0244
			(0.766)
Log Casual Wage in 2004-5 (Square)			0.0526
			(0.112)
Female Labor Force Participation Ratio (Square)			0.238
			(0.182)
Male Labor Force Participation Ratio (Square)			-0.824
			(0.839)
Phase==2	0.00535	0.00764	0.00871
	(0.00414)	(0.00532)	(0.00549)
Observations	571	569	569
R-squared	0.105	0.132	0.161

Each observation corresponds to a district-quarter. The sample is composed of all Early Phase districts in the agricultural year 2007-8 (NSS Round 64). The outcome Public Works employment is computed as follows. Respondents were asked what they did on each of the past seven days. Using answers to this question, the fraction of days spent in public works is computed for each individual. These individual estimates are then aggregated to the district-quarter level using survey sampling weights. See the text for the definition of "Star" States. Standard errors are clustered at the state level. * p < .10, ** p < .05, *** p < .01.