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Doctoral Dissertations, 2011-2026 Economics

2012

Essays on India's Employment Guarantee

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Princeton, NJ : Princeton University

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ESSAYS ON INDIA'S EMPLOYMENT GUARANTEE

John H. Papp

A DISSERTATION

PRESENTED TO THE FACULTY

OF PRINCETON UNIVERSITY

IN CANDIDACY FOR THE DEGREE

OF DOCTOR OF PHILOSOPHY

RECOMMENDED FOR ACCEPTANCE

BY THE DEPARTMENT OF ECONOMICS

Adviser: Anne C. Case

JUNE 2012

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Abstract

This thesis studies three issues related to the labor market and distributional impacts of a large rural workfare program in India. The first chapter is co-authored with a graduate student at the Paris School of Economics, Clement Imbert. The second and third chapters are single-authored.

Chapter One uses the gradual roll-out of India's National Rural Employment Guarantee Act (NREGA), a large rural workfare program, to estimate the program's impact on wages and aggregate employment. We use a difference-in-differences strategy, which compares changes in districts that received the program earlier to districts that received it later. Our estimates reveal that following the introduction of the program, public employment increased by 0.3 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 increased by roughly 4.5%, and private sector work for low-skill workers fell by 1.7%. These effects are concentrated in the dry season, during which the majority of public works employment is provided. Our results suggest that public sector hiring crowds out private sector work and increases private sector wages. We use these estimates to compute the implied welfare gains of the program by consumption quintile. Our calculations show that the welfare gains to the poor from the equilibrium increase in private sector wages are large in absolute terms and large relative to the gains received solely by program participants. 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.

Chapter Two describes the design and implementation of a survey consisting of interviews with 2,224 adults from 705 households from 70 villages in three states in India (Rajasthan, Gujarat, and Madhya Pradesh). Data collection emphasized migration and participation in the NREGA. Over half of the adults in the sample, belonging to 80 percent of households, left the village for work at least once from summer 2009 through summer

2010. Migration was short-term and seasonal with the majority of migrants leaving during the agricultural off-season. Most migrants, even those spending six to 11 months outside the village per year, engaged in economic activities when they returned to the village suggesting important economic ties to the village. Participation in the NREGA is seasonal and common with 72% of surveyed households reporting at least one member working for the program within the past year. We document substantial unmet demand for work under the program and sharp differences in provision of work under the program across states.

Chapter Three uses the data from the survey described in Chapter Two to present evidence that the NREGA decreased migration. Thirty-five percent of the respondents report leaving the village for work during summer 2009. Of these migrants, 35% report working for the NREGA, and 88% report that they would work more for the NREGA if more work were available. Eight percent of adults report that they would have migrated had they not worked for the NREGA. We document that adults living in a state that provides more days of public works under the program, even conditional on demand for NREGA work, spend less time outside the village for work compared with other states. This cross-state difference in days outside the village is statistically significant only during the summer season during which most of the NREGA work is provided. Demand for NREGA work is high among migrants despite the fact that wages for work outside the village are roughly double the earnings for NREGA participants. Using wage differentials and survey measures of demand for NREGA work, we estimate that the average migrant incurs monetary and non-monetary costs of migration equivalent to 75% of the average earnings per day spent outside the village.

Acknowledgements

I am sincerely grateful to my adviser Anne C. Case for her generous guidance and support throughout my time at Princeton. I also thank Jeff Hammer, Alex Mas, and Tom Vogl, who served on my committee. Rohini Pande deserves special mention. Several other people were influential in the creation of this dissertation, and in my development during my graduate studies. They include David Atkin, Angus Deaton, Taryn Dinkelman, Jean Dreze, Erica Field, Mike Geruso, Faruk Gul, Reetika Khera, Per Krusell, Andrew Robinson, and Sam Schulhofer-Wohl. I am indebted to Diane Coffey and Dean Spears for volunteering their time and energy to help make the survey described in Chapter Two possible. Thanks to Ajaykumar Tannirkulam, Pramod Tiwari, and the research and survey team at the Centre for Microfinance in Chennai for their tireless work executing the survey. Participants at the Princeton RPDS/CHW workshop, Princeton Industrial Relations workshop, Princeton Public Finance workshop, the NEUDC at Yale, and the Indian Statistical Institute Conference provided valuable feedback and suggestions. Financial support from the Fellowship of Woodrow Wilson Scholars, the National Science Foundation, the Industrial Relations Section at Princeton, the Russell Sage Foundation, and the Center for Economic Policy Studies at Princeton is gratefully acknowledged. Finally, I thank my family and friends, especially Dorothy and John Papp for their support and encouragement.

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Chapter 1

Equilibrium and Distributional Effects of India's Employment Guarantee

1.1 Introduction

Workfare programs are common anti-poverty policies. Many developing countries have programs that hire workers at competitive wage rates with the goal 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, however, 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 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. Using 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 due solely to participation in the program. 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 cannot be ignored.

[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.

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 ignore potential equilibrium impacts.

As Ravallion (1987) and Basu et al. (2009) show, government hiring may crowd out private sector work and lead to a rise in equilibrium private sector wages. However, the empirical evidence on the equilibrium impacts of workfare programs is limited. Gaiha (1997) shows that agricultural wages seem to respond to government wage hikes in the Maharashtra Employment Guarantee. Murgai and Ravallion (2005) consider hypothetical effects of an India-wide employment guarantee on wages. Further, even the hypothetical studies ignore the possibility that a rise in wages may hurt net labor buyers. In a rural, developing country context, where households often participate on both sides of the labor market (Benjamin, 1992), the potential losses to labor buyers are a first order concern.

We use a modified version of the theoretical framework presented in Deaton (1989) and Porto (2006) to clarify how the equilibrium welfare effects of a workfare program are distributed across the population. 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 welfare gains due to changes

in equilibrium wages relative to gains strictly due to participation in the program.

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 2010-11 the NREGA provided 2.27 billion person-days of employment to 53 million households.[2] The program was introduced gradually throughout India starting with the poorest districts in early 2006 and extending to the entire country by mid 2008. We estimate the impact of the program on employment and wages by comparing changes in outcomes in districts that received the program between April 2006 and April 2007 to those that received it after April 2008. 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 1.4, 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.

Our primary data source for the empirical analysis is a series of cross-sectional, nationally representative household surveys conducted from 2004 to 2008 covering roughly 450,000 adults. The empirical analysis proceeds in four steps.

(Step 1: Changes in Public Works Employment) 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 suggest high levels of employment under the act, many studies have documented evidence of widespread over-reporting of employment by corrupt officials (Niehaus and Sukhtankar, 2008; Khera, 2011; Imbert and Papp, 2011). We find that public employment provision is highly seasonal with the majority of employment provided during

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

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 administrative capacity or political will to implement the program.

(Step 2: Changes in Wages) Second, we document that average daily wages of casual laborers increase by roughly 4.5% during the dry season in early districts relative to late 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 roughly twice as large (9%) in the five "star" 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 poorer districts were more likely to be selected to be early districts, late districts are unlikely to provide a perfect counterfactual for early districts. As a result, the differential changes that we document may be due to some other factor correlated with poverty. When we add pre-program district-level poverty rates and other controls interacted with a dummy for the post-treatment period, our estimate of the rise in wages increases slightly. 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.

(Step 3: Changes in Private Sector Employment) Third, we document the differential changes in aggregate employment across early and late districts. We find the introduction of the program is correlated with a 1.7% fall in the fraction of days spent doing any kind of private work (waged, self employed or domestic work) among low-skilled persons. We find no evidence of a fall in the fraction of people reporting being unemployed or out of the labor force. Program districts within star states show a much larger fall of 4.1% in the fraction of days spent doing private sector work. The size of this fall is very close to the size of the increase in public employment in the star states, which is 4.4% of private sector 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, we define private sector work to include both self-employment and domestic work. We make this choice because a majority of rural households report operating an agricultural or non-agricultural household business, and work within the household business may be categorized as either domestic work or self-employment.

(Step 4: Welfare Gains by Consumption Quintile) The fourth empirical step uses the wage and employment estimates combined with household-level data on consumption, casual labor supplied, and labor hired to compute how the welfare gains from the increase in

wages are distributed across rural households. We show that the rise in wages redistributes income from richer households to poorer households. We then use individual-level data on program wages and participation to estimate the magnitude of the direct gains from participation in the program. Our estimates 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 households in the bottom three consumption quintiles, the estimated welfare gain due to the wage change represents 20-50% of the total welfare gain from the program.

This paper contributes to four broad strands of the literature. First, it contributes to the small but growing literature examining the impact of the NREGA itself (Ravi and Engler, 2009; Sharma, 2009; Azam, 2011). 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 1.3 proposes a simple model of rural labor markets, which provides a framework for estimating the distributional effects of the program. Section 1.4 presents our data and empirical strategy, Section 1.5 presents the main empirical results, Section 1.6 uses these results to estimate the welfare gains due to the program and Section 1.7 concludes.

1.2 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 billion person-days of employment to 53 million

households.[3] The India-wide budget was Rs. 345 billion (7.64 billion USD), which represents 0.6% of GDP.

The act was gradually introduced throughout India starting with 200 of the poorest districts in February 2006, extending to 120 districts in April 2007, and to the rest of rural India in April 2008. Our empirical strategy, described in detail in Section 1.4.3, compares outcomes in districts that received the program prior to April 2008 to those that received it after.

The National Rural Employment Guarantee Act sets out guidelines detailing how the program is to be implemented in practice. Whether and how these guidelines are actually followed varies widely 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.

1.2.1 Poverty Reduction through Employment Generation

One of the chief motivations underlying the act is poverty reduction through employment generation. In this respect, the NREGA follows a long history of workfare programs in India (see Appendix 1.A). Since it is first and foremost a poverty alleviation scheme, the NREGA is often compared to 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,

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

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 actual expenditure is even lower (27% in 2008-09).[4] Wages paid for unskilled work are born entirely by the central government while states must pay 25% of the expenditure on materials, capital and skilled wages. Together, these restrictions create a strong incentive to select projects that require mainly low-wage, manual work potentially at the expense of the productivity benefits of the resulting infrastructure.

1.2.2 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.[5] 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, 46% report that they usually

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

[5]Authors' calculations based on NSS Round 66 Employment and Unemployment Survey. The Employment surveys are described in detail in Section 1.4.1.

or sometimes engage in casual labor, while only 0.1% report that they usually or sometimes work in a salaried job.[6] The similarity of these public sector jobs and casual labor jobs motivates our focus on casual wages in the empirical analysis.

1.2.3 Wages and Payment

Wage rates are set at the state level, and NREGA workers are either paid a piece-rate or a fixed daily wage. Under the piece-rate system, which is more common, workers receive payment based on the amount of work completed (e.g. volume of dirt shoveled). The resulting daily earnings are almost always below the state-set wage levels. Theft by officials also reduces the actual payment received.[7]

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 79 Rupees per day for work under the act.[8] 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).

[6] Authors' calculations based on NSS Round 66 Employment and Unemployment Survey. The Employment surveys are described in detail in Section 1.4.1.

[7] 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.

[8] Authors' calculations based on NSS Employment and Unemployment Survey Round 64. The Employment surveys are described in detail in Section 1.4.1.

1.2.4 Employment, Rationing and Awareness

A more direct way to assess whether NREGA work is more attractive than available work is to ask people. The next chapter of this dissertation as well as Dreze and Khera (2009) ask people if they would work more for the NREGA if more work were available. Both studies find high levels of unmet demand. 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 majority 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.

During agricultural year 2009-10, an estimated 19% of households reported attempting to get work under the act without success.[9]

1.2.5 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

[9]Authors' calculations using NSS Employment and Unemployment Survey Round 66. The Employment surveys are described in detail in Section 1.4.1.

rains make construction projects difficult to undertake, which is likely part of the justification. Field reports, however, document government attempts to stop works during the rainy season so that the works do not compete with the labor needs of farmers (Association for Indian Development, 2009).

1.2.6 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 fully 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.

1.2.7 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 program's impact] 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. In a study using a similar methodology and data set to the one used here, Azam (2011) documents that the phase-in of the program correlates with an increase in casual wages and public works employment. He focuses on gender differences, with female wages increasing by 8% compared with 4% for men and with public works employment increasing by more for men than women.

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.

1.3 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 distributional effects of price changes. The key difference here is that we focus on the labor market rather than the market for consumption goods, though much of the analysis is similar.

1.3.1 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 and in chapters two and three, 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. We discuss later the case in which the opportunity cost of time is below the market wage.

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

$$\begin{aligned} &\max_{c_i, L_i} u(c_i, T - L_i) \\ \text{s. t. } &c_i + W(T - L_i) = WT + y_i \end{aligned} \tag{1.1}$$

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

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

1.3.2 Equilibrium

Let aggregate labor demand be defined as the sum of the household demand functions $D(W) \equiv \sum_i D_i(W)$. Define aggregate labor supply to be the sum of the individual labor supply functions $L^s \equiv \sum_i L_i^s(W, \pi_i + WT + (W_g - W)L_i^g)$. In the subsequent analysis, we assume that both of these functions are differentiable. 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:

$$L^g + D(W) = L^s \tag{1.2}$$

1.3.3 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 differentiate the market clearing condition with respect to L_g:

$$1 + D'(W)\frac{dW}{dL_g} = \sum_i \left(\frac{dL_i^s}{dW}|_{y_i}\frac{dW}{dL_g} + \frac{dL_i^s}{dy_i}\frac{dy_i}{dL_g}\right) \tag{1.3}$$

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

$$\frac{dL_i^s}{dW}|_{y_i} = \frac{dL_i^s}{dW}|_u + \frac{dL_i^s}{dy_i}L_i^s \tag{1.4}$$

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

$$\begin{aligned}\frac{dy_i^s}{dL_g} &= \pi_i'(W)\frac{dW}{dL_g} + (W_g - W)\frac{dL_i^g}{dL_g} - \frac{dW}{dL_g}L_i^g \\ &= -D_i\frac{dW}{dL_g} + (W_g - W)\frac{dL_i^g}{dL_g} - \frac{dW}{dL_g}L_i^g \end{aligned} \tag{1.5}$$

where the second equality follows from the envelope theorem for the profit function, which implies $\pi_i'(W) = -D_i$. Plugging Equations 1.4 and 1.5 into Equation 1.3 and re-arranging yields:

$$\frac{dW}{dL^g} = \frac{1 - \sum_i \frac{dL_i^s}{dy_i}(W_g - W)\frac{dL_i^g}{dL_g}}{-D'(w) + \sum_i \left(\frac{dL_i^s}{dW}|_u + \frac{dL_i^s}{dy_i}(L_i^s - L_i^g - D_i)\right)} \tag{1.6}$$

We can compute the change in aggregate private sector employment as $\frac{dD}{dL^g} = D'(W)\frac{dW}{dL^g}$. This equation allows us to estimate the elasticity of labor demand using the ratio of the percentage change in the wage divided by the percentage change in employment. In Section 1.3.5, we discuss why this ratio might not correspond to the labor demand elasticity if employers exercise market power or if the act increases workers' productivity.

From Equation 1.6, we see that an increase in government hiring will raise wages as long as the income effect is not too large ($\sum_i L_{y_i}^s(W_g - W) < 1$). The increase will be larger if demand is less elastic (small $-D'(W)$) or if labor supply is less elastic (small $\sum_i \left(\frac{dL_i^s}{dW}|_u + L_{y_i}^s(L_i^s - L_i^g - D_i)\right)$). Note 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 1.6 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.

1.3.4 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 $e(W, u_i)$ be the expenditure function corresponding to the dual of the utility maximization problem above. 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:

$$e(W, u_i) = \pi_i(W) + WT + (W_g - W)L_i^g + z_i \tag{1.7}$$

where z_i is exogenous income. $e(W, u_i)$ is the expenditure or total income required to achieve utility level u_i and $\pi_i(W) + WT + (W_g - W)L_i^g + z_i$ is total income. For fixed z_i, when L_g changes, Equation 1.7 will no longer hold because the expenditure required to achieve the same utility will change (the left hand side) and because the household's available income will change (the right hand side). We will derive the change in z_i required to maintain the equality, and therefore maintain the same utility level, following a change in L_g. We do this by differentiating Equation 1.7 with respect to L_g:

$$\frac{de(W, u_i)}{dW}\frac{dW}{dL_g} = \pi_i'(W)\frac{dW}{dL_g} + T\frac{dW}{dL_g} + (W_g - W)\frac{dL_i^g}{dL_g} - L_i^g\frac{dW}{dL_g} + dz_i \tag{1.8}$$

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

$$\begin{aligned} -dz_i &= (L_i^s - L_i^g - D_i)W\frac{dW/W}{dL_g} + (W_g - W)dL_i^g \\ &= \textit{Net Casual Labor Earnings} \times \frac{dW/W}{dL_g} + (W_g - W)dL_i^g \end{aligned} \tag{1.9}$$

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).

1.3.5 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.

Worker Productivity

Our analysis assumes that the workfare program does not directly increase workers' productivity. As a result any increase in wages represents a pure redistribution from employers to workers. To the extent that the program increases wages by changing worker productivity, Equation 1.9 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 1.2.1 suggests that the infrastructure created by the program is unlikely to have had a large effect on worker productivity during the period that we analyze. Worker productivity, however, may have 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. Further, we will not be able to estimate the elasticity of labor demand as the ratio of the percentage change in wages and the percentage change in employment.

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 1.C.1, we derive the impact on consumption of household i. The key difference compared with Equation 1.9 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.

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. These effects may be important and are certainly interesting, however, in the interest of making progress, we ignore them in this analysis.

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 $p_i L$ 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 $F_i(\cdot) = A_i G(\cdot)$. 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 $p_i W$ and therefore set $A_i G'(D_i) = p_i W$. 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 $A_i G'(D_i) = W$. 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 $A_i G'(D_i) \in [p_i W, W]$. Details of the proofs are given in Appendix 1.C.2.

There are four main take-aways 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 other 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).

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 paying with a much lower skill premium. As discussed in Section 1.2.2 above, there is indeed significant evidence of selection in that workers who participate in the workfare

program are very unlikely to report performing in salaried work in the past year (0.1%), while 46% report usually or sometimes working in casual labor. If we think of the labor market in the model as only the market for casual labor, then the model already implicitly includes a substantial selection effect. In the empirical analysis, we allow for individual-level heterogeneity in wages by including controls for education, caste, and gender in the wage regressions.

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:

$$F'(L^*) = W(L^*) + W'(L^*)L^* \tag{1.10}$$

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 1.10. 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 1.9 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 1.9 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. Further, we will not be able to estimate the elasticity of labor demand as the ratio of the percentage change in wages and the percentage change in employment.

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.

1.4 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.

1.4.1 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 Appendix 1.D. For the calibration in Section 1.6, we use the ARIS-REDS data set, which is described in detail in Appendix 1.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 1.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.

Rural to rural inter-district migration for employment is limited. Out of all adults 18 to 60 with secondary education or less living in rural areas, only 0.1% percent report having migrated from a different rural district for employment within the past year.[10] Similarly, the number of adults 18 to 60 with secondary education or less who report having migrated for employment from rural to urban areas in the past year is 0.11% of the total population of rural adults 18 to 60 with secondary education or less.[11] 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. The next chapter documents that at least in

[10] Authors' calculations using NSS Employment and Unemployment Survey Round 64. The Employment surveys are described in detail in Section 1.4.1.

[11] Authors' calculations using NSS Employment and Unemployment Survey Round 64. The Employment surveys are described in detail in Section 1.4.1.

some areas of India, short-term trips anywhere from two weeks to six months are common. Chapter Three 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 later the extent to which this goal is accomplished in practice. The NSSO over-samples some types of households and therefore provides sampling weights.[12] 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 by this time the program had been introduced to all districts for at least two years.

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

1.4.2 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 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 1.11 presents the number of non-missing observations for each district-quarter for the employment and wage outcomes, and Appendix 1.D provides further discussion.

1.4.3 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 1.1 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 prime-age adults employed in agricultural casual labor, fraction employed in non-agricultural casual labor, fraction employed in cultivation, fraction employed in a non-agricultural business, and fraction employed in 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 1.2.5, 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 1.2.6, 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.

1.4.4 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. The district fixed effects, however, provide assurance that the results are not driven by the fact that the wage and to a lesser extent the employment panels are unbalanced.[13] 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. Our main specification, however, splits the program effect by season by replacing T_{dt} with $T_{dt} \times Dry_t$ and $T_{dt} \times Rainy_t$.

[13]Table 1.11 shows the balance of the wage and employment panels.

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 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 1.D.4 for details). Standard errors are clustered at the district level.

1.5 Results

We next present the results from the empirical framework presented in the previous section.

1.5.1 Summary Statistics

Table 1.1 presents the means of the main outcomes used in the paper by year. Table 1.2 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 1.3 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.

1.5.2 Change in Public Works Employment

Table 1.4 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 1.2 percentage points during the dry season in program districts. As expected, the increase during the rainy season is less than a quarter as large. The change for late districts is much smaller and insignificant. Table 1.4 also shows that differences in public employment provision between early and late districts persist and even widen after the program is extended to all of India by 2009-10. The lack of catch-up by late districts could reflect a learning component to implementation where districts that have the program for longer generate more employment. Alternatively, the differences could reflect differential demand for work or targeting by the government. 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. The main results, however, still hold if we include 2009-10 data.

Table 1.5 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 0.44 percentage points in other districts.

The specifications in Table 1.6 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 0.74 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 the program dummy variable represents the fraction of days spent in public works out of *all* days. Therefore, someone working five days a week would contribute only $5/7 = 0.71$. 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 their time working in private employment (including domestic work). The rise in public employment therefore represents 0.78% of the private workforce.

1.5.3 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 1.7. 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. Given the large standard errors, however, 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 1.3.5, this could be because workers do not know they will be unemployed on a given day until they have invested the time searching or traveling to find a job. As a result, they do not have the option of choosing to work for the workfare program only on days on which they would have been unemployed. Alternatively, unemployment might not fall because the rationing mechanism is such that only workers who otherwise would have had work are 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.

1.5.4 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. We next present the differential trends in casual daily earnings for workers in early compared with late districts.

Table 1.12 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 1.8 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 0.7 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 1.4.3 and in Table 1.2. The results are robust to these changes in the specification.

The rise in wages could be the result of the program hiring low wage workers, leaving only high wage workers in the workforce. 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.[14] 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 1.2.2, less than 0.1% of people who worked for the government program report also working in a salaried job in the past year. Salaried jobs are generally higher paying, regular jobs, and are considered more attractive than the work provided by the workfare program. For this reason, we may expect the program to have a limited effect on salaried wages.[15] Column Six of Table 1.8 presents the results for the main specification with deflated log salaried wages as the outcome. The coefficient on the interaction between

[14]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 1.D.4 for more details.

[15]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).

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. It does raise the concern, however, that the estimated increase in casual wages may be an underestimate if the fall in salaried wages indicates a general negative demand shock for all types of labor.

Assuming that labor markets are competitive, and that changes in the wage are due to shifts along the demand curve, we can now use our estimate of the increase in the wage of 4.5% and the fall in private sector work to compute a labor demand elasticity. The elasticity of labor demand is $\epsilon_d = \frac{1.6}{4.5} = 0.35$, which is in the same range as previous estimates from rural labor markets in India (Binswanger et al., 1984).

1.5.5 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 1.9 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. The results confirm that the field studies are correct in labeling these states as star states. In fact, there seems to be very little employment generation outside these states. Columns Two through Four show that the fall in private sector work documented for all of India is concentrated within the early districts of star states during the dry season.

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 assurance that the results are not driven by selection.

1.6 Estimating the Distributional Impact

Recall from Section 1.3 that the compensating differential for household i given by Equation 1.9 and written here for convenience is

$$-dz_i \quad = \quad \textit{Net Casual Labor Earnings}_i \times \frac{dW/W}{dL_g} + (W_g - W)dL_i^g \tag{1.11}$$

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.

1.6.1 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 1.11. For the percent increase in wages $\frac{dW/W}{dL_g}$, we use 4.5% based on the estimates in Table 1.8.

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 1.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 it is 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 1.10. 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. We are forced to assume, however, 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 earnings are reported in the third row of Table 1.10. 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 4.5% 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.

1.6.2 Direct Gains from Participation

We next turn to quantifying the second term in Equation 1.11, 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 in 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 1.10.[16]

We estimate W_g using 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. This figure is likely to underestimate the initial public-private wage gap, since private wages have moved closer to the government wage as a result of the program. The estimated wage increase following program implementation between 2004-5 and 2007-8 is 4.5%. Hence, for the calibration we set the government wage to be 20% 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 and 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

[16]The results for the main outcomes are presented by consumption quintile in Table 1.14.

participant's next best option, as it only considers productive activities.

For the calibration, we consider two extreme cases. One in which the outside option is the market wage and one in which the outside option is 50% of the market wage. The implied direct transfer $(W_g - W)\Delta L_g$ under these two assumptions is presented in rows 14 and 15 of Table 1.10. The market wage is clearly an upper bound of workers' outside option: for richer households, the direct transfer is negative, suggesting a welfare loss. In the lower bound case, on the other hand, the transfer is positive for all households, which matches the observation that all quintiles participate into the program.

1.6.3 Comparing Equilibrium and Direct Gains

Rows 16 and 17 of Table 1.10 present the total estimated gain for each consumption quintile assuming an outside option equal to 100% and 50% of the government wage. Rows 18 and 19 show the fraction of the total gain due to the equilibrium change in wages. For the three poorest quintiles, the equilibrium effect is between 20% and 50% of the total gain. Rows 20 and 21 show the gain as a fraction of total expenditure. Although richer households lose from the program, the impact as a fraction of total expenditures is less than 1%.

1.7 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 in the program.

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.

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.

1.A History of Public Works Programs in India

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

Second, the Sampoorn Grameen Rozgar Yojana (SGRY) started in 2001 with the purpose of generating employment across India and remained 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.

1.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 the labor force in agriculture. Table 1.13 shows the results. The coefficient on star states is hardly affected 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 political will were important factors in the variation in employment generation across districts. When we add square terms (third and fourth column of Table 1.13), 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 the 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.

1.C Additional Model Results and Proofs

1.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 from the impact on welfare because it also includes labor supply effects. Household consumption is given by:

$$c_i = \pi_i(W) + WL_i^s(W, y_i) + (W_g - W)L_i^g \tag{1.12}$$

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

$$\begin{aligned}\frac{dc_i}{dL^g} &= (W_g - W)\frac{dL_i^g}{dL^g} \\ &+ WL_{yi}^s(W_g - W)\frac{dL_i^g}{dL^g} \\ &+ (L_i^s - D_i - L_i^g)\frac{dW}{dL^g} \\ &+ W\Big[\frac{dL_i^s}{dW}|_u + L_{y_i}^s(L_i^s + T - L_i^g - D_i)\Big]\frac{dW}{dL^g} \end{aligned} \tag{1.13}$$

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

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

1.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_iW and therefore set $A_iG'(D_i) = p_iW$. Very productive households (high A_i) will be net labor buying households and will face a marginal value of time of W and therefore set $A_iG'(D_i) = W$. Finally, a non-trivial subset of households with A_i in the middle of the distribution will neither buy nor sell labor to the market so that $A_iG'(D_i) \in [p_iW, W]$.

Proposition 1: There exists a threshold A_e such that households are net labor buyers if and only if $A_i > A_e$

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

$$\max_{L,c} u(c, T - L) \tag{1.14}$$

$$\text{s. t. } c + \tilde{W}(T - L) = Y + \tilde{W}L \tag{1.15}$$

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

$$D_e = D(W, A_e) \tag{1.16}$$

$$L^s(W, A_eG'(D_e)) = D_e \tag{1.17}$$

Note that since $L^s_Y \leq 0$ and $D_A(\tilde{W}, A_i) \geq 0$, the pair A_e and D_e exists and is unique. A household with $A_i = A_e$ therefore supplies and demands D_e labor. Since the marginal cost of hiring labor is W while the marginal value of working in the labor market is $p_iW < W$, 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 $D(W, A_i) > L^s(W, A_iG'(D(W, A_i)))$, 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

$$D_w = D(p_i W, A_w) \tag{1.18}$$

$$L^s(p_i W, A_e G'(D_w)) = D_w \tag{1.19}$$

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

Proposition 3: For $A_i \in [A_w, A_e]$, households will be neither net suppliers nor buyers of labor.

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

1.D Data

1.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 1.11 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 1.11 we see that for thick rounds, we have observations for all district-quarters. For thin rounds, there are a number of instances in which surveying did not take place in a particular district-quarter.

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

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 1.D.4 for further discussion of the weighting.

1.D.2 District Controls

Table 1.2 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.[17] 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"

[17] `http://www.esrl.noaa.gov/psd/`

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[18] 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.

1.D.3 ARIS-REDS Household Hired Labor

For our calibration exercise in Section 1.6, we require estimates of labor hired by households, information which is not available in the NSS Employment Surveys. For this reason, we use the ARIS-REDS survey data, collected by the National Council of Applied Economic Research (Delhi) in 1999-00.[19] 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."

[18] http://www.eci.nic.in/eci_main1/index.aspx
[19] http://adfdell.pstc.brown.edu/arisreds_data/readme.txt

1.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 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.

1.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.

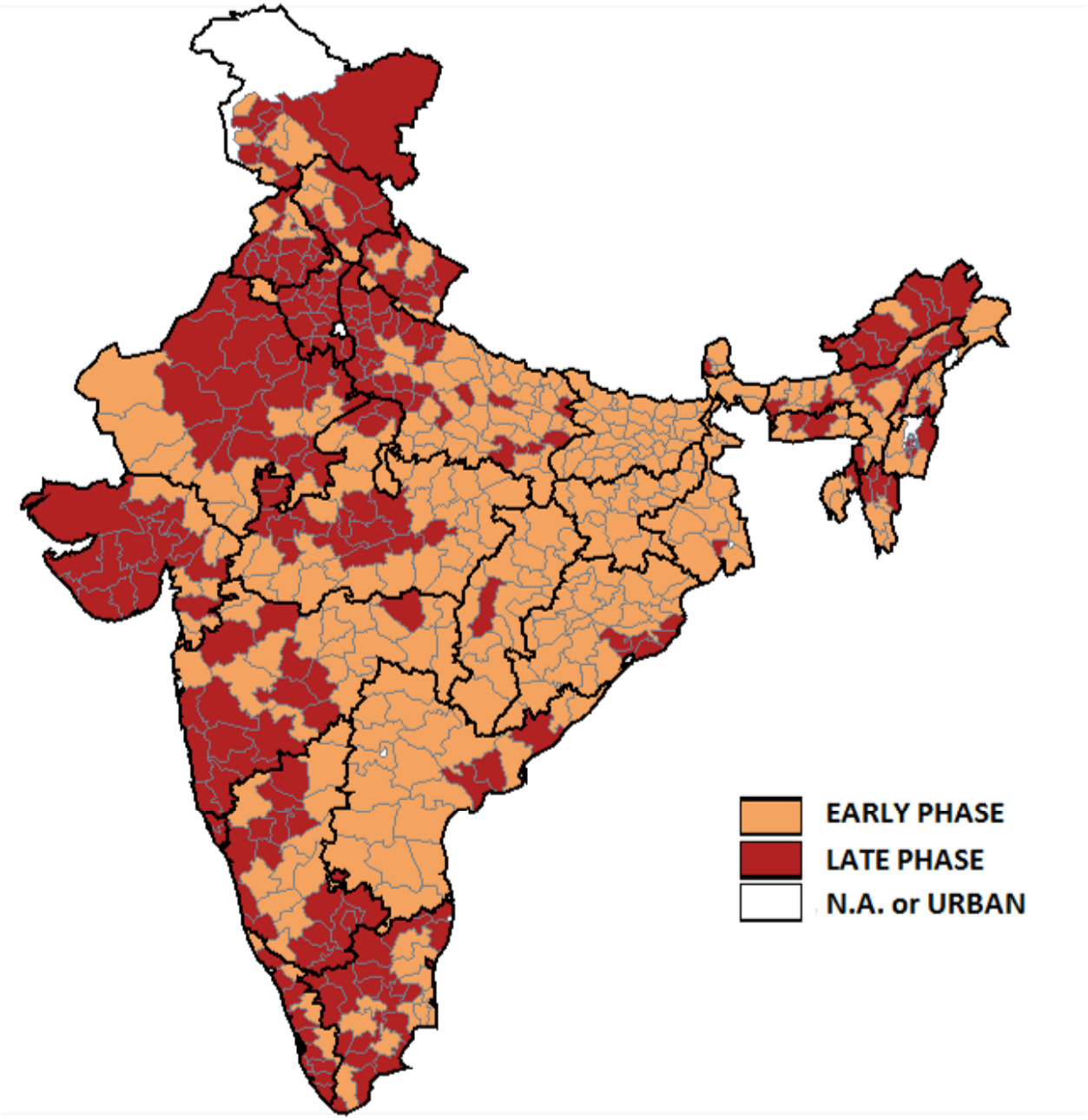

Figure 1.1: Map of Early and Late Districts

Table 1.1: Outcome Summary Statistics by Agricultural Year

	2004 (Jan to Dec)		2005 (Jan to Dec)		2007-08 (Jul to Jun)	
	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)
Public Works Employment	0.0033	0.0006	0.0034	0.0009	0.0035	0.0012
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
Surveyed persons	79,125	87,982	55,797	61,801	73,782	83,348
Surveyed persons reporting casual labor earnings in past 7 days	19,377	9,191	14,491	6,452	24,846	11,151
District-quarters with at least one person surveyed	3,907	3,907	3,718	3,718	3,940	3,940
District-quarters with at least one person working in casual labor in last 7 days	3,368	3,368	3,045	3,045	3,434	3,434

This table presents means for the main outcomes used in the paper over time. 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 60 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.

Table 1.2: 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)	Non-missing Observations
Literacy Rate	0.555	0.651	-0.096***	0.583	0.595	-0.012	2001 Census	No	493/493
Fraction SC	0.188	0.174	0.015*	0.182	0.183	-0.001	2001 Census	No	493/493
Fraction ST	0.133	0.048	0.085***	0.150	0.081	0.068***	2001 Census	No	493/493
Poverty Rate	0.320	0.209	0.111***	0.228	0.298	-0.069***	NSS	No	493/493
Population Density (per sq. km)	483	405	78***	239	540	-300.675***	2001 Census	No	493/493
Irrigated Cultivable Land per Capita (ha)	0.083	0.118	-0.035***	0.118	0.087	0.031***	2001 Census	No	493/493
Unirrigated Cultivable Land per Capita (ha)	0.175	0.173	0.002	0.239	0.149	0.091***	2001 Census	No	493/493
In South	0.197	0.278	-0.081**	0.491	0.123	0.368***		No	493/493
Female Labor Force Participation Ratio	0.379	0.367	0.012	0.502	0.323	0.179***	2001 Census	No	493/493
Male Labor Force Participation Ratio	0.634	0.629	0.005	0.662	0.621	0.042***	2001 Census	No	493/493
Fraction Ag Casual Laborers	0.197	0.162	0.035***	0.237	0.162	0.075***	NSS	No	493/493
Fraction Non-Ag Casual Labor	0.047	0.066	-0.019***	0.060	0.052	0.009**	NSS	No	493/493
Fraction Cultivators	0.274	0.253	0.021*	0.318	0.245	0.073***	NSS	No	493/493
Fraction Non-Ag Business	0.091	0.090	0.001	0.087	0.092	-0.006	NSS	No	493/493
Fraction Salaried Work	0.046	0.073	-0.027***	0.060	0.055	0.006	NSS	No	493/493
Fraction Labor Force in Agriculture	0.758	0.665	0.093***	0.776	0.701	0.075***	2001 Census	No	493/493
Annual Rainfall (mm)	3,345	3,277	68	2,737	3,553	-816.379***	IMD	Yes	5784/5916
Rain Shock Bad	0.142	0.182	-0.04**	0.183	0.147	0.037*	IMD	Yes	5784/5917
Rain Shock Good	0.270	0.165	0.105***	0.174	0.253	-0.079***	IMD	Yes	5784/5918
Election Year	0.526	0.344	0.182***	0.374	0.490	-0.116**		Yes	5916/5916
Number of Districts	286	207		143	350				

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

Table 1.3: 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***

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

Table 1.4: Public Works Difference-in-Differences 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) (1) - (3)	Dry Jan to Jun (6) (2) - (4)
(1)	Pre (1/04 to 12/05)	0.0012	0.0026	0.0008	0.0028		
		(0.0003)	(0.0005)	(0.0002)	(0.0007)		
(2)	Post (2007-08)	0.0035	0.0142	0.0009	0.004		
		(0.0006)	(0.0026)	(0.0004)	(0.0011)		
(3)	(2) - (1)	0.0024***	0.0116***	0.0001	0.0012	0.0022***	0.0104***
		(0.0007)	(0.0026)	(0.0004)	(0.0012)	(0.0008)	(0.0021)
(4)	Post (2009-10)	0.0092	0.0177	0.0062	0.0116		
		(0.0015)	(0.0032)	(0.0014)	(0.0027)		
(5)	(4) - (2)	0.0056***	0.0035	0.0052***	0.0076***	0.0004	-0.0041
		(0.0015)	(0.0032)	(0.0014)	(0.0028)	(0.0016)	(0.0026)
	Districts	286	286	207	207		

The unit of observation is a district-quarter. Each cell is a mean of the variable public works. For example, row (1), column (2) is the mean of public works for all districts that received the program prior to April 2007 (early districts) with the sample restricted to the first six months (dry season) of 2004 and 2005. Public works is an estimate of the fraction of days spent working in public works employment for adults aged 18 to 60 with secondary education or less. Table 1 and the text describe variable construction in detail. All means 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 February 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. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 1.5: Public Works Difference-in-Differences Estimates by Implementation Group

		Star States			Other States		
		Early Districts (1)	Late Districts (2)	Diff-in-Diff (3)	Early Districts (4)	Late Districts (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) - (2)	0.0064	0.0155***	-0.009	0.0041***	0.0026***	0.0012
		(0.0053)	(0.0048)	(0.0044)	(0.0012)	(0.001)	(0.001)
	Districts	83	60		172	147	

The unit of observation is a district-quarter. Each cell is a mean of the variable public works. For example, row (1), column (2) is the mean of public works for all districts within star states that received the program after April 2007 (early districts) with the sample restricted to 2004 and 2005. Public works is an estimate of the fraction of days spent working in public works employment for adults aged 18 to 60 with secondary education or less. Table 1 and the text describe variable construction in detail. Star states are identified by field reports as having implemented the administrative requirements of the act particularly well. Star states include Andhra Pradesh, Madhya Pradesh, Tamil Nadu, Rajasthan, and Chhatisgarh. 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 are adjusted for correlation of the errors at the district level. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 1.6: Public Works (First Stage)

	Public Works (1)	Public Works (2)	Public Works (3)	Public Works (4)	Public Works (5)
Program	0.00680***	0.00680***	0.00613***	0.00720***	
	(0.00140)	(0.00146)	(0.00161)	(0.00193)	
Program X Dry					0.0110***
					(0.00308)
Program X Rainy					0.00334**
					(0.00135)
Observations	5,784	5,784	5,784	5,784	5,784
R-squared	0.020	0.164	0.181	0.194	0.197
District FE	No	Yes	Yes	Yes	Yes
Quarter x Year FE	No	No	Yes	Yes	Yes
District Controls	No	No	No	Yes	Yes

Each column presents the results from a separate regression. The unit of observation is a district-quarter. The outcome public works is an estimate of the fraction of days spent working in public works employment for adults aged 18 to 60 with secondary education or less. Table 1 and the text describe variable construction in detail. All estimates are computed using weights proportional to district population. District controls are listed in Table 2. District controls that do not vary over time are interacted with a dummy for 2007-08 (post-program). 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. Program is a dummy variable equal to one for early districts during July 2007 to June 2008. The sample includes all observations from the district panel from January 2004 to December 2005 and from July 2007 to June 2008. Standard errors in parentheses are adjusted for correlation of the errors at the district level. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 1.7: 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	0.00993***	-0.0227***	0.0155***	-0.00272	0.0110***	-0.0157**	0.00675	-0.00207
	(0.00286)	(0.00641)	(0.00431)	(0.00329)	(0.00308)	(0.00691)	(0.00454)	(0.00369)
Program X Rainy	0.00234***	-0.00564	0.00721	-0.00391	0.00334**	0.00124	-0.00149	-0.00309
	(0.000857)	(0.00508)	(0.00443)	(0.00266)	(0.00135)	(0.00608)	(0.00494)	(0.00321)
Observations	5,784	5,784	5,784	5,784	5,784	5,784	5,784	5,784
District Controls	No	No	No	No	Yes	Yes	Yes	Yes

Each column presents results from a separate regression. All regressions include district and year-quarter fixed effects. The unit of observation is a district-quarter. Public, private, unemployed, and not in the labor force are estimates of the fraction of adults aged 18 to 60 working in public works, private sector work (including domestic work), unemployed or not in the labor force. Dry is a dummy variable equal to one for the first two quarters of the year. Table 1 and the text describe variable construction in detail. All estimates are computed using weights proportional to district population. Rainy is a dummy variable equal to one for the second two quarters of the year. All estimates are computed using weights proportional to district population. District controls are listed in Table 2. District controls that do not vary over time are interacted with a dummy for 2007-08 (post-program). Program is a dummy variable equal to one for early districts during July 2007 to June 2008. The sample includes all observations from the district panel from January 2004 to December 2005 and from July 2007 to June 2008. Standard errors in parentheses are adjusted for correlation of the errors at the district level. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 1.8: Main Specification Daily Earnings

	District level			Person level		
	Log Daily Casual Earnings	Log Deflated Daily Casual Earnings	Log Deflated Daily Casual Earnings	Log Deflated Daily Casual Earnings	Log Deflated Daily Casual Earnings	Log Deflated Daily Salaried Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Program X Dry	0.0440**	0.0389**	0.0550***	0.0531***	0.0437**	-0.116***
	(0.0187)	(0.0191)	(0.0196)	(0.0185)	(0.0172)	(0.0431)
Program X Rainy	0.0104	0.00355	0.0196	0.0209	0.0264	-0.0290
	(0.0186)	(0.0194)	(0.0202)	(0.0188)	(0.0173)	(0.0441)
Observations	5,576	5,576	5,576	85,508	85,449	29,323
District Controls	No	No	Yes	Yes	Yes	Yes
Worker Controls	No	No	No	No	Yes	Yes

Each column presents results from a separate regression. All regressions include district and year-quarter fixed effects. For the first three columns, the unit of observation is a district-quarter. For the last three columns, the unit observation is a person. Log daily casual earnings is the log of earnings per day worked for people who report working in casual labor. Daily salaried earnings are earnings from salaried work which tend to be higher-paying longer-term jobs. Table 1 and the text describe variable construction in detail. 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. District controls that do not vary over time are interacted with a dummy for 2007-08 (post-program). Individual controls are dummies for workers' gender, marital status, age, caste, religion and education, and the fraction of working time in the agricultural sector. Program is a dummy variable equal to one for early districts during July 2007 to June 2008. The sample includes all observations from the district panel from January 2004 to December 2005 and from July 2007 to June 2008. Standard errors in parentheses are adjusted for correlation of the errors at the district level. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 1.9: Program Effects by Implementation Group

	Public	Private	Unemployed	Not in Labor Force	Log Deflated Daily Earnings	Log Deflated Daily Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Program X Dry X Star States	0.0351***	-0.0386***	0.00465	-0.00118	0.102***	0.0871***
	(0.00825)	(0.0122)	(0.00731)	(0.00547)	(0.0278)	(0.0239)
Program X Rainy X Star States	0.00496**	-0.000744	-0.00259	-0.00162	0.0354	0.0469*
	(0.00218)	(0.00881)	(0.00737)	(0.00464)	(0.0287)	(0.0247)
Program X Dry X Other States	-0.000188	-0.00504	0.00778	-0.00256	0.0320	0.0265
	(0.00168)	(0.00694)	(0.00498)	(0.00399)	(0.0214)	(0.0191)
Program X Rainy X Other States	0.00133	0.00336	-0.000877	-0.00382	0.00944	0.0144
	(0.00125)	(0.00648)	(0.00523)	(0.00358)	(0.0217)	(0.0196)
Observations	5,784	5,784	5,784	5,784	5,576	83,474
District Controls	Yes	Yes	Yes	Yes	Yes	Yes
Worker Controls	No	No	No	No	No	Yes

Each column presents the results of a separate regression. All regressions include district and year-quarter fixed effects. For columns (1) to (4), the unit of observation is a district-quarter. For columns (5) and (6), the unit of observation is a person. Public, private, unemployed, and not in the labor force are estimates of the fraction of adults aged 18 to 60 working in public works, private sector work (including domestic work), unemployed or not in the labor force. Log daily casual earnings is the log of earnings per day worked for people who report working in casual labor. Table 1 and the text describe variable construction in detail. Dry is a dummy variable equal to one for the first two quarters of the year. Rainy is a dummy variable equal to one for the second two quarters of the year. All estimates are computed using weights proportional to district population. District controls are listed in Table 2. District controls that do not vary over time are interacted with a dummy for 2007-08 (post-program). Worker controls are listed in the notes of Table 8. Star states is a dummy variable equal to one for districts within star states. Other states is a dummy variable equal to one for districts that are not in star states. See Table 5 for a description of star states. Program is a dummy variable equal to one for early districts during July 2007 to June 2008. The sample includes all observations from the district panel from January 2004 to December 2005 and from July 2007 to June 2008. Standard errors in parentheses are adjusted for correlation of the errors at the district level. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 1.10: Calibration

		Expenditure Quintile						
		Poorest	Second	Third	Fourth	Richest	Full Sample	Construction
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Household Expenditures and Income								
(1)	Monthly Consumption Per Capita	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	3.6%	8.2%	11.3%	18.1%	39.5%	13.8%	NCAER 1999
(7)	Estimated Monthly Labor Cost per Household	102	237	324	519	1136	398	(6) x Full (3) x 5
(8)	Net Labor Earnings per Month	630	356	134	-180	-951	177	(3) - (7)
(9)	Wage change	4.5%	4.5%	4.5%	4.5%	4.5%	4.5%	Estimated
(10)	Net Income Gain from Wage Change	28.3	16.0	6.0	-8.1	-42.8	8.0	(8) x (9)
Gain from Government employment								
(11)	Increase in Days in Public Employment per HH per Month	2.05	0.79	0.74	0.52	0.34	0.89	Estimated
(12)	Average Private Sector Wage	46.7	46.7	46.7	46.7	46.7	46.7	NSS 2004-5
(13)	Average Government Wage	56.0	56.0	56.0	56.0	56.0	56.0	(12) + 20%
(14)	Direct Gain with Outside Option at Market Wage	28.5	7.5	6.9	4.7	3.1	8.2	(11) x [(13)-(12)]
(15)	Direct Gain with Outside Option at 50% of Market Wage	66.6	26.2	24.1	16.6	10.7	28.9	(11) x (13)
Total Gain								
(16)	Total Gain with Outside Option at Market Wage	56.9	23.5	12.9	-3.4	-39.7	16.2	(10) + (14)
(17)	Total Gain with Outside Option at 50% of Market Wage	94.9	42.2	30.2	8.5	-32.1	36.8	(10) + (15)
Gain from Wage Change as Fraction of Total Gain								
(18)	Assuming Outside Option is Market Wage	49.8%	68.1%	46.6%	--	--	49.1%	(10)/(16)
(19)	Assuming Outside Option is 50% of Market Wage	29.9%	37.9%	20.0%	--	--	21.6%	(10)/(17)
Total Gain as Fraction of Total Expenditures								
(20)	Assuming Outside Option is Market Wage	3.1%	1.0%	0.5%	-0.1%	-0.8%	0.7%	(16)/(2)
(21)	Assuming Outside Option is 50% of Market Wage	5.2%	1.8%	1.1%	0.3%	-0.6%	1.5%	(17)/(2)

Columns (1) to (5) correspond to different quintiles based on household per capita expenditure. Column (6) is all households. The last column indicates how each figure is obtained. Rows (1) to (5) use data from the NSS 2004-05 Employment Survey to compute averages for each quintile using survey sample weights. The fraction of casual labor costs paid by quintile (sixth row) is computed using data from the 1999-00 ARIS-REDS survey as follows. First we use monthly per capita expenditure to define quintiles. Second, by quintile, we aggregate all wages paid by the household to adult laborers. Third, we aggregate all income from casual labor supplied outside the household by all adults aged 18 to 60. The means in row (6) are obtained for each quintile by dividing total wages paid by total wage income received across all households. The wage change in row (9) is equal to the estimate of the program impact during the dry season from the specificiation in Table 8 with workers controls. The increase in days in public employment per household per month reported in row (11) is obtained from the regressions reported in Table A4.

Table 1.11: 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 with non-missing observations per district-quarter. There are 493 districts in the panel. The NSS attempts to survey an equal number of villages in each districts during each quarter. During thick rounds (2004-05, 2007-08, 2009-10), this is generally possible. During thin rounds (2005-06, 2003-04), this is less likely to be achieved. Casual wages are only available for district-quarters during which at least one respondent reports working in casual labor.

Table 1.12: 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) (1) - (3)	Dry Jan to Jun (6) (2) - (4)
(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) - (2)	0.0731***	0.0908***	0.0902***	0.0921***	-0.0171	-0.0013
		(0.0165)	(0.0165)	(0.0196)	(0.0183)	(0.0283)	(0.0267)
	Observations	286	286	207	207		

The unit of observation is a district-quarter. Each cell is a mean of the log of average casual earnings for a different sample. For example, row (1), column (2) is the mean of the log of average casual earnings for all districts that received the program prior to April 2007 (early districts) with the sample restricted to the first six months (dry season) of 2004 and 2005. Log daily casual earnings is the log of earnings per day worked for people who report working in casual labor. Table 1 and the text describe variable construction in detail. 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 are adjusted for correlation of the errors at the district level. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 1.13: Are Star State Results Driven by Demand for Public Employment?

	Public Work (1)	Public Work (2)	Public Work (3)
Star States	0.0182***	0.0212***	0.0210***
	(0.00249)	(0.00215)	(0.00277)
Poverty Rate		0.0128	0.0475**
		(0.00770)	(0.0195)
Fraction ST		-0.0150**	-0.0171
		(0.00545)	(0.0158)
Literacy Rate		-0.000125	-0.0299
		(0.0153)	(0.0564)
Irrigated Cultivable Land per Capita		-0.0123**	-0.0301
		(0.00439)	(0.0176)
Fraction Labor Force in Agriculture		-0.0179	0.103*
		(0.0204)	(0.0506)
Fraction of Labor force in Ag. Casual Work		-0.000894	-0.0572
		(0.0113)	(0.0499)
Fraction of Labor force in Non Ag. Casual Work		-0.0505**	-0.0589
		(0.0218)	(0.0666)
Log Casual Wage in 2004-5		-0.00593	-0.0285
		(0.00857)	(0.0545)
Female Labor Force Participation Ratio		0.0129	-0.0730
		(0.0101)	(0.0595)
Male Labor Force Participation Ratio		-0.0464*	0.529
		(0.0240)	(0.503)
Poverty Rate (Square)			-0.0419**
			(0.0184)
Fraction ST (Square)			-0.00163
			(0.0220)
Literacy Rate (Square)			0.0185
			(0.0554)
Irrigated Cultivable Land per Capita (Square)			0.0227
			(0.0172)
Fraction Labor Force in Agriculture (Square)			-0.101*
			(0.0516)
Fraction of Labor Force doing Ag. Casual Work (Square)			0.129
			(0.115)
Fraction of Labor Force doing Non Ag. Casual Work (Square)			-0.0429
			(0.346)
Log Casual Wage in 2004-5 (Square)			0.0272
			(0.0564)
Female Labor Force Participation Ratio (Square)			0.128
			(0.0860)
Male Labor Force Participation Ratio (Square)			-0.448
			(0.406)
Second Phase	0.00369	0.00473	0.00529
	(0.00226)	(0.00311)	(0.00327)
Observations	286	286	286
R-squared	0.126	0.162	0.198

The unit of observation is a district. The outcome is the district-level change in public works from 2004-05 to 2007-08. The sample is composed of all districts that received the program prior to April 2007. The change in public works is computed using the the estimate of public works, which 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. Control variables are described in the text. Star states is a dummy variable equal to one if a district is within a star state. Star states are identified by field reports as having implemented the administrative requirements of the act particularly well. Star states include Andhra Pradesh, Madhya Pradesh, Tamil Nadu, Rajasthan, and Chhatisgarh. Standard errors are clustered at the state level. ***, **, and * indicate significance at the 1, 5, and 10% levels.

Table 1.14: Outcomes by Quintile

	HH Public	HH Private	HH Unemployed	HH Not in Labor Force
	(1)	(2)	(3)	(4)
Dry Season				
Program X Dry X Quintile 1	0.0675***	-0.0770*	0.0523*	-0.0429*
	(0.0204)	(0.0397)	(0.0275)	(0.0233)
Program X Dry X Quintile 2	0.0260**	-0.0375	0.0318	-0.0203
	(0.0117)	(0.0285)	(0.0199)	(0.0174)
Program X Dry X Quintile 3	0.0242***	-0.00784	0.00434	-0.0207
	(0.00831)	(0.0265)	(0.0182)	(0.0168)
Program X Dry X Quintile 4	0.0171***	-0.0239	0.0124	-0.00564
	(0.00534)	(0.0240)	(0.0164)	(0.0178)
Program X Dry X Quintile 5	0.0111**	-0.0284	-0.00730	0.0245
	(0.00503)	(0.0287)	(0.0153)	(0.0257)
Rainy Season				
Program X Rainy X Quintile 1	0.0179***	0.00426	-0.0160	-0.00615
	(0.00567)	(0.0326)	(0.0291)	(0.0214)
Program X Rainy X Quintile 2	0.0159***	-0.000106	-0.00283	-0.0130
	(0.00521)	(0.0237)	(0.0197)	(0.0150)
Program X Rainy X Quintile 3	0.0136***	0.00510	0.0178	-0.0364**
	(0.00371)	(0.0225)	(0.0156)	(0.0163)
Program X Rainy X Quintile 4	0.00220	0.0358	-0.00376	-0.0343*
	(0.00539)	(0.0235)	(0.0148)	(0.0185)
Program X Rainy X Quintile 5	0.00606	0.0370	-0.0373**	-0.00577
	(0.00413)	(0.0298)	(0.0163)	(0.0278)
Observations	173,538	173,538	173,538	173,538
District x Quintile FE	Yes	Yes	Yes	Yes
Quarter x Year FE	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes
Individual Controls	No	No	No	No

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

Chapter 2

Design and Implementation of a Survey Measuring Migration and Public Works Participation in Rural India

2.1 Introduction

In this chapter, we discuss the design and implementation of a survey focused on short-term migration and participation in a public works program in rural India. We compare the sample with a large nationally representative household survey and document some key patterns in the data.

As Foster and Rosenzweig (2008) explain, research on short-term migration is limited by the nature of existing data sets (p. 3052). Data collection is hindered by the practical difficulty of finding migrants, by the complications migration poses to defining sampling frames and households, by survey respondents forgetting or overlooking short trips, and by questionnaires that simply do not ask about short-term migration. Even specialized migration surveys are surveys of migrants at destination (Foster and Rosenzweig, 2008, p. 3058), and as a result much of the data that speak to labor migration can say little about the effects of labor flows on sending economies.

We designed the survey discussed in this chapter to minimize the above difficulties. With a survey instrument targeted at understanding short-term migration, we found that over two-fifths of the adults who completed our survey lived outside of their village to work at some point in the summer of 2010. Over half left the village for work at least once from summer 2009 through summer 2010. Because these averages include all adults aged 14 to 69, they imply nearly universal migration among working young men. They also imply that a very large fraction of households, approximately 80 percent, sent at least one migrant. Trips are frequent and short: the median trip in our sample lasted 30 days, and 99 percent were less than four months.

Our data help us understand why these households chose short-term migration. Migration is seasonal, driven by the annual cycle of agricultural productivity. Repeated short-term migration during agriculturally unproductive times of the year is part of households' long-

term economic strategy. Migrants' employment relationships are short-term in nature with few migrants working for the same employer on repeated trips. The data suggest that few short-term migrants become long-term migrants. Indeed, only three percent of households said that someone had left permanently in the last five years for reasons other than marriage. Even though people rarely break away from households to live permanently in a city, some migrants spend long periods outside the village. That ten percent of the adults in the sample spend nine months a year outside of the village suggests that definitions of "permanent migration" should be carefully considered.

In addition to questions about short-term migration, the survey instrument contained detailed questions about participation in India's National Rural Employment Guarantee Act (NREGA). The National Rural Employment Guarantee Act, passed in September 2005, entitles every household in rural India to 100 days of work per year at a state-level minimum wage. Based on government administrative data, in 2010-11 the NREGA provided 2.27 billion person-days of employment to 53 million households.[1] Despite its size and importance in the lives of the poor in India, the paucity of data on participants means that even basic questions such as whether or not the act provides work to those who want it are difficult to answer (Khera, 2011).

Our results reveal that demand for work for the program significantly exceeds actual days worked, with many individuals reporting wanting to work more for the program. Further, supply of work under the program varies significantly from state to state even in villages lying right across state borders. Interestingly, the timing of demand and supply of work matches closely to the timing of short-term migration, with the majority of work provided during the summer when most short-term migrants leave for work.

This chapter provides a description of the thought-process and decisions that went into designing the survey as well as the context in which the survey was conducted. We provide

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

summary statistics and discussion for a number of key variables from the survey. The goal of the analysis is to reveal the data's strengths and limitations and communicate the contents of the data set. The discussion is therefore broad and shallow, leaving deeper analysis for other work, such as the following chapter. This material should be particularly useful for those designing their own surveys on similar topics or for those wishing to use the survey data for their own research. For the interested reader, Coffey et al. (2011) provides a more detailed discussion of migration in our sample in the context of the literature on the shift from agricultural to non-agricultural employment in developing countries.

The next section gives some background on the people who make up the survey population and where they live. Section 2.3 describes how the survey was designed and why and discusses how this survey relates to existing surveys. Section 2.4 shows how surveyed households and adults compare with the rural population in India and to adults in sampled households who we were unable to survey. Section 2.5 presents a first set of results using the data. Section 2.6 concludes with some ideas for future research.

2.2 Context and Variation across States

The survey was conducted during summer 2010 in a high out-migration area straddling the borders of three states in India (Rajasthan, Gujarat, and Madhya Pradesh). The surveyed villages were selected from four districts: Banswara in Rajasthan, Jhabua and Ratlam in Madhya Pradesh, and Dahod in Gujarat. Banswara, the capital of Banswara district, is within 300 kilometers of the major migration destinations of Ahmedabad and Baroda, and roughly 400 kilometers from Surat, also a major Gujarati city and migration destination. The study area has a high concentration of tribal people, and households are mainly patrilocal. For more detailed descriptions of the area and populations studied here, Mosse et al. (2002) and Haberfeld et al. (1999) are excellent sources.

Panel B of Table 2.1 presents summary statistics for the surveyed households. House-

holds in the study area are poor. Ninety-two percent of households in our sample have a dirt floor, 71.7% do not have electricity and only 1.4% have a television set. While nearly all of the households in the survey own land, there are few large landowners with only 2% of households holding more than ten acres and the largest holding being 28 acres.

2.2.1 Seasonality

There are three main agricultural seasons: summer (March-June), monsoon (July-October), and winter (November-February). Agriculture is predominantly rain-fed with the main growing season during the monsoon. Corn is planted during the monsoon for home consumption, and the fodder from the corn is saved for feeding animals. Almost half of households have no irrigation. Households with irrigation are able to plant crops, mainly wheat, during the winter. Crops are rarely grown during the summer.

2.2.2 Languages

The primary languages in the study area are Hindi and Vagri in Rajasthan and Madhya Pradesh and Hindi and Gujarati in Gujarat. Some adults in Madhya Pradesh and a few in Rajasthan reported speaking Bhilli and Malvi as well. These six languages are similar to each other but differ enough as to create difficulties communicating. In all states, the majority of household heads identified as Hindu and a minority (9%) identify as Adivasi, which is a tribal religion. A substantial fraction (63%) of the sample identified as belonging to a Scheduled Tribe (ST).[2]

[2]The fraction of households belonging to a group that the central government calls "ST" is likely much higher than this. Over a quarter of households said they did not know to which category they belonged.

2.2.3 Cross-state Variation

By surveying along the border of the three states, it was possible to survey villages with access to the same migration destinations but with varying state-level policies. At the state-level, Rajasthan and to a lesser extent Madhya Pradesh have been pioneers in the implementation of the NREGA, while Gujarat has lagged far behind. As we show later, these state-level differences persist right up to the border with large gaps in implementation across states in our sample.

Despite their proximity to each other, sample villages differ substantially from state to state in terms of infrastructure, government administrative capacity and language. Gujarat as a whole has better infrastructure than Rajasthan and Madhya Pradesh. Based on 2001 census data, 93% of the rural population in Gujarat lived in villages with a paved road leading to the village compared with 48% in Madhya Pradesh and 73% in Rajasthan. Ninety-six percent of the rural population in Gujarat lived in villages with regular bus service, compared with 62% in Rajasthan and 35% in Madhya Pradesh. Further, government-provided services are generally better in Gujarat. The literacy rate in Gujarat was 80% compared with 71% and 67% in Rajasthan and Madhya Pradesh respectively. These state-level differences were plainly evident in the surveyed villages. Villages in Gujarat were more likely to have well-paved roads and were connected to the electricity grid using power lines run from modern, steel electric towers, which stretched just to the state border. Many of the villages in Rajasthan and Madhya Pradesh had unpaved roads and no electricity, and even when they did have electricity, villages were connected to the grid by well-worn polls.

Panel A of Table 2.1 presents averages of variables constructed from information provided by local government officials in each surveyed village. The villages in Gujarat stand out as having higher agricultural wages as well as more cultivated land per person during the winter season during which most crops require irrigation. Villages in Madhya Pradesh are less likely to have paved approaches and are on average 0.6 kilometers further away from

a road with regular motor vehicle traffic. Out of all sample villages, only one official within a village in Gujarat reported that workers from nearby villages come to the village to work.

The three states in the sample differ substantially in their implementation of the NREGA. As described in chapter one, the NREGA requires every state to put in place a system for implementing the requirements of the act. Chief among these requirements is that each household be provided 100 days of employment on demand. Rajasthan and to a somewhat lesser extent Madhya Pradesh are considered pioneers in the implementation of the NREGA. Gujarat, despite its substantial administrative capacity, seems to have moved much slower than other states in the implementation of the act. Panel C of Table 2.1 reveals substantial variation in the fraction of adults who report working for the NREGA in the past year. Ten percent of adults in Gujarat report working for the NREGA in the past year, while roughly 40% of adults in Rajasthan and Madhya Pradesh report working. We present evidence later in this paper suggesting that a substantial part of this difference is driven by the supply of work provided by the government rather than demand by potential participants.

2.3 Survey Design and Existing Data Sources

The survey presented here is not the first to document short-term migration or participation in the NREGA. Studies outside of economics (Breman, 1996; Rogaly, 1998; Haberfeld et al., 1999; Mosse et al., 2002; Deshingkar, 2006) and an increasing number of studies within economics (Banerjee and Duflo, 2007; Badiani and Safir, 2009; Chowdhury et al., 2009; Bryan et al., 2011) suggest that labor migration from South Asian villages is common, and that an important fraction of this migration is for short lengths of time. Indeed, an estimated 80 percent of migrants in India who leave rural areas for work each year spend less than six months away from the village.[3] Short-term migration is not limited to South Asia. Goza

[3]Author's calculations using Round 64 of the NSS Employment and Unemployment Survey.

et al. (1997) describe the nature of short-term migration within Brazil.

Participation in the NREGA as well as demand for NREGA work has been documented in a number of studies (Sharma, 2009; Dreze and Khera, 2009; Institute of Applied Manpower Research, 2009). In at least one case, these NREGA surveys included questions on migration as well (Ashish and Bhatia, 2009). The National Sample Survey Office of India (NSSO) conducts a nationally representative employment survey roughly every other year, which asks questions about NREGA work and occasionally about short-term migration. The strength of the data set presented here relative to existing sources is that it combines detailed questions on both short-term migration and program participation. With the narrow focus on these two areas, we were able to tailor the survey design to improve accuracy and detail. In the sequel, we discuss the NSSO's methodology and compare summary statistics from the NSSO's surveys with the results from our sample.

2.3.1 Short-term Migration

The NSSO's Employment and Unemployment Survey asks respondents about the total time spent outside the village for work in the past year. Those who have spent a total of between 30 and 180 days in the past year working outside the village are labeled as short-term migrants. Those who spend more than 180 days outside the village are considered long-term migrants, and therefore not part of the household. As a result, only limited information is collected on those who spent 180 to 360 days outside the village.

We define short-term migration as spending at least two consecutive days and less than 11 months outside the village for work in the past year. We use a more expansive definition of the household, collecting information on all persons who spent at least thirty days in the village within the past year. Five percent of adults left the village to work for more than two consecutive nights but worked for less than 30 days outside the village in the last year. As a result, these migrants would not be counted using the NSSO's definition. On the

other extreme, 8% of adults in our sample left the village for work for between 180 days and 11 months. The NSSO survey and many other surveys would have collected only limited information on these adults as they are not classified as household members.

To our knowledge, there have been two other migration surveys done in the region that we study. One is documented in Mosse et al. (2002) and the other in Haberfeld et al. (1999). In Mosse et al. (2002), 2,588 households from 42 villages in the three states were interviewed. The villages visited were part of a Department for International Development funded program that lasted four years and targeted families who earned their livelihoods from rain-fed agriculture. In the Haberfeld et al. (1999) study, 645 households from eight villages of Dungarpur district of Rajasthan were interviewed. Our survey builds on these studies, not only because of the ten-year gap between surveys, but also because our detailed adult questionnaires allow for in-depth analysis of migrants' experiences.

The use of a short time frame to define migration, an expansive definition of a household and individual interviews with each adult allow a more complete characterization of the experiences of short-term migrants. In addition, in cases in which adults were away for work during the first survey visit, we returned a second time during the monsoon. We asked about migration for the last five years, and then collected detailed information about trips in the last four seasons up to a maximum of four trips. We believe that having this more general conversation with adult respondents about their previous migration experience helped them to start thinking about their trips, so that they could report them more accurately.

Our survey allows us to closely examine migration trips taken in the past four seasons. We collected information on location, employment type, wages, travel, and living conditions for each trip up to a maximum of four. We asked migrants which household members accompanied them on each trip, how work was found, and whether employers provided in-kind payment. For this information, our strategy of interviewing the individual migrant, as opposed to asking a household head about members' migration was very useful. Wage

information, in particular, can be sensitive, especially if migrants bring home what they save out of their wages to contribute to household expenses.

2.3.2 NREGA

Using separate adult surveys for each adult in the household likely improved the accuracy of answers to the NREGA questions. In order to obtain work under the NREGA, each household member must first register by having her name added to the household's official NREGA identity card. Registration is apparently difficult since we observed cases in which an unregistered household member worked in place of a registered person rather than going through the process of registering herself. As a result, in some cases, different household members gave conflicting reports about who actually did the work. We were able to catch at least some of these cases because we took the time to separately interview as many adults as possible as well as ask the household head about participation by all household members.

In order to assess the demand for NREGA work, we asked respondents whether they wanted more work on the NREGA and if so how much. In order to increase the reliability of the answers to these hypothetical questions, we asked why respondents either wanted more work or did not want more work. These responses could be used to better assess whether respondents would have worked more for the NREGA if more work were available. For example, a respondent that reports wanting more work and who says she did not work more because the NREGA does not pay enough or because she was sick is unlikely to have worked more if more work had been available. On the other hand, a respondent that reports wanting more work and who says she did not work more because the village leaders closed the worksite is likely to have worked more. Alternatively, a person who says she does not want to work more for the NREGA because village officials will not provide more work likely would actually work more if given the option.

Previous studies of the NREGA have documented late payment and underpayment

of wages (Dreze and Khera, 2009). We asked respondents how long after the work was completed payment was made and about total earnings as well as typical daily earnings.

2.3.3 Language

Although many households in our sample spoke the major languages of Hindi and Gujarati, some respondents were more comfortable with Vagri, Malvi, Bhilli or other local languages. These languages are closely related to Hindi and Gujarati and are often described as a blend of the two languages. They are different enough, however, to generate misunderstandings and potentially lead to inaccurate answers, especially for hypothetical questions. Although the survey itself was written in Hindi, in instances in which respondents were more comfortable with a different language and there was a surveyor who knew that language, the surveyors were instructed to use that language instead for the interview. Most of the surveyors knew Gujarati, Hindi and Vagri, so for Gujarati and Vagri respondents this was generally feasible. For the 7% of adults who did not speak Hindi, Gujarati, or Vagri, another household member or person in the village was asked to help translate to minimize misunderstandings.

2.3.4 Seasonality and Recall

Recall problems were an important challenge. Reporting on past events is challenging for most people, but here these problems were exacerbated by respondents' lack of schooling and thus disfluency in numbers and calendar time. In addition, both migration trips and NREGA work spells were generally short and at irregular intervals.

In order to help respondents recall past events, surveyors posed questions in terms of seasons rather than calendar months. Despite these efforts, few people could remember any details of trips beyond three years back. Interestingly, though, people often remembered the first time they migrated, or even the first two or three years of migration. As a result, we recorded migrants' first trip and the following three years, memory permitting.

Due to potential recall problems, we focus mainly on data based on the most recent four seasons. That is, we focus on the current season (summer 2010), the previous winter (winter 2009-10), the previous monsoon (monsoon 2009) and the previous summer (summer 2009). Based on piloting and observing many surveys in progress, we believe that the majority of respondents were able to accurately report migration and NREGA work over this time period. Still, the data should be taken as an approximation to the true days worked and is nothing close to the ideal achieved by a repeated panel survey with a recall period of a week or even thirty days. In the sequel, we refer to variables computed based on the "past year." Unless stated otherwise, these variables refer to the period summer 2009, monsoon 2009 and winter 2009-10. We use this time period rather than simply the preceding 365 days because it allows us to use the same period for all respondents. Since surveying occurred during summer 2010 and partly during monsoon 2010, simply asking questions about the past 365 days could introduce hard to predict variations in responses, since it is unlikely that respondents are able to remember when exactly within a season a trip or NREGA work spell occurred.

2.4 Sample Selection and Characteristics

As discussed in Section 2.2, the survey location was selected because previous studies in this area (Mosse et al., 2002; Haberfeld et al., 1999) reported high rates of out-migration and because the area marks the intersection of three states with varying levels of implementation of the NREGA.

The specific villages surveyed were selected based on pair-wise matching using proximity to each other, land composition (irrigated land, cultivable non-irrigated land, culturable waste[4]), population density, and caste composition taken from the 2001 census. Figure 2.1

[4]Culturable waste includes lands available for cultivation but not cultivated in the past five years.

shows the study location and selected villages. More detail on village and household selection is provided in Appendix 2.A.

2.4.1 Response Rates

For each sampled household, a household survey, answered by the household head or other knowledgeable person, as well as individual adult surveys for each person aged 14 to 69 were attempted. From here on, we define "adult" as any person between the ages of 14 and 69. Figure 2.2 shows the response rate at the household level. Out of 753 attempted household surveys, 705 were completed for a completion rate of 93.6%.

An adult was considered a member of the household if she had spent at least 30 days living with the household in the past year. Adult household members who were visitors from another household, had married into another household, or were 70 years or older were not attempted. After removing these adults, the total sample of adults in the 705 households was 2,722. Out of these adults, adult surveys were completed for 2,224 or 81.6%.

The remaining 18.4% of adults were either unavailable or declined the survey. For these adults, the household head or other knowledgable household member was asked to respond to a much smaller set of questions about the unavailable adults' migration and local work. Figure 2.3 shows the response rates at the adult level.

We use the larger group of adults for which we have at least some migration and NREGA information to make general comparisons between migrants and non-migrants and NREGA participants and non-participants as well as to judge how pervasive migration and NREGA participation is in the population. We use the group that answered the full adult survey to explore more detailed questions about the nature of migration and the NREGA. We turn next to describing how these two samples compare to one another, and how surveyed households and adults compare with the rural population of India.

2.4.2 Household Characteristics

The first column of Table 2.2 presents means for the households in our sample. The second column presents means from the rural sample of Rounds 61 and 66 of the NSS Employment and Unemployment Survey (from here on, "NSS Round 61 and 66").[5]

There is no reason our sample should match the characteristics of the rural population of India as a whole. However, it is informative to compare and contrast the two. Age, marital status, and gender are similar across the samples. Households in our sample almost universally engage in small-scale cultivation of their own land. Although cultivation is common throughout rural India, the results from the NSS survey make clear that it is far from universal with only 45 percent of households reporting a member employed in own cultivation. Salaried work is less common among households in our sample, though not by much.

A striking difference is the much higher rates of participation in the NREGA in our sample, over 70% compared with 25%. Households in our sample worked over four times as many days as households in India as a whole. Part of this difference can be accounted for by household size since households in our sample have an average of 4.4 adults compared with 3.2 for the NSS sample. The reason NREGA participation is so common in our sample is partly because Rajasthan and Madhya Pradesh are among the star implementers of the NREGA and partly because our sample is particularly poor so demand is high.

Despite the high NREGA participation rates of households across India, 20% of households surveyed by the NSS report being unable to obtain work despite attempting to get work. We show later that many adults in our sample experience the same problem, and we are able to provide more detail on what exactly trying and failing to obtain work actually

[5]We use data from both NSS rounds since Round 61 includes detailed cultivation information, while Round 66 contains detailed migration and NREGA participation information. Round 61 was conducted between July 2004 and June 2005. Round 66 was conducted between July 2009 and June 2010.

means.

We will see in the next section that adults in our sample are much more likely to participate in short-term migration. Interestingly, Table 2.2 shows that households are less likely to have had someone leave permanently within the last five years. Permanent migration is a tricky concept to define as some migrants might return after a long stay outside the village. We asked the household head about migrants who "permanently" left the household within the last five years and are currently living in another district. In the NSS survey sample, we define permanent migrants as a person who left the district one to five years ago and has not returned. Neither definition accounts for the permanent or long-term migration of entire households, which is difficult to measure with surveys in the sending location.

2.4.3 Characteristics of Surveyed Adults

The first two columns of Table 2.3 present descriptive statistics for the two main sub-samples used throughout the paper. The first column includes the 2,722 adults for whom the adult survey was attempted. The second column includes the 2,224 adults who answered the full adult module. The third column includes adults who did not answer the adult survey.

Based on age, gender, marital status and education, the full sample of adults and those who completed the adult survey appear similar. However, this is mainly because most adults were surveyed. Comparing the sample of adults who completed the adult survey directly with those who did not complete it, we find many statistically and economically significant differences. Adults who did not complete the adult survey are more likely to be male and unmarried. They are younger, and slightly better educated than those who completed the adult survey. This is almost certainly because many of those who were not interviewed were away for work at the time surveyors visited their household. Indeed, those who did not complete the adult survey were 21.6 percentage points more likely to have spent between two and 330 days outside the village for work in the last year, and 29.1 percentage points

more likely to have migrated for work in all three seasons than those who completed the adult survey. The adults that we were unable to survey were 29 percentage points less likely to have ever worked for the NREGA.

The final column in Table 2.3 presents basic summary statistics from Round 64 of the NSS Employment and Unemployment Survey, conducted between July 2007 and June 2008. As expected given that our survey region is particularly poor, our sample is significantly less educated than the national rural population. To quantify short-term migration, the NSS asks whether a household member spent between 30 and 180 days living outside of the village for work. By this definition, 2.5% of rural Indian adults were short-term migrants.

Based on Table 2.2, only 3.7% of households in our sample reported that someone had migrated out of the household permanently in the last five years compared with 8.8% of households for India as a whole. Although we did not collect data on migration of entire households, existing sources suggest permanent migration from rural areas in India is low (Munshi and Rosenzweig, 2009).

The low levels of permanent migration contrast sharply with the rate of short-term migration. We found that 18% of migrants in our sample spent six to 11 months away from the household (six percent spent nine to 11 months). This migration would not be considered "short-term" by the NSS survey, but nor can it really be considered "permanent." When migrants return to the village, a very large fraction of them return to work. Over 97% of those who had been away six months or more did work upon returning to the village, and about 93% of those who had been away for 11 months worked when they returned to the village. We take this work as a sign that even migrants who live outside the villages for many months a year have important economic ties to the villages.

2.4.4 Migrants and Non-Migrants

The first two columns of Table 2.4 split our sample of adults into those who spent between two and 330 days total outside the village as migrant workers in the past year and those who did not. The third and fourth columns perform a similar exercise for the NSS sample using the NSS definition of short-term migration of between 30 and 180 days. In both our sample and the NSS sample, short-term migrants are more likely to migrate to urban than rural areas. They mainly work in construction, followed by agriculture. Migrants tend to be younger than the average adult.

There are a few differences between short-term migrants in our sample and those in the NSS sample. In our sample, a larger fraction of short-term migrants are women. The average short-term migrant in our sample is less educated than the average short-term migrant from the NSS sample. However, in both samples, short-term migrants tend to be less educated than adults who do not engage in short-term migration. Panel B of Table 2.4 reports statistics based on the most recent trip for short-term migrants from the NSS sample and the most recent trip for short-term migrants that completed the adult survey for our survey sample. Manufacturing seems to be a common industry for short-term migrants from all of rural India, while in our sample only 3.5% of short-term migrants worked in manufacturing.

2.4.5 Public Works Participants and Non-Participants

The first two columns of Table 2.5 split our sample of adults into those who participated in the NREGA within the past year and those who did not. The third column presents sample means for adults surveyed by the NSS Round 66 survey who reported working for the NREGA within the past seven days or reported usually or occasionally working in public works employment in the past year. Within our sample and across India, roughly half of NREGA workers are women. In our sample, NREGA workers are older than the average adult and an average of eight years older than the average short-term migrant. NREGA

workers are less educated than those who do not participate in NREGA work.

2.5 Some Basic Results

Having established how our sample compares with adults in rural India as a whole, we next present a more detailed analysis of adults and households in our sample.

2.5.1 Migration and Program Participation by Age and Gender

Figure 2.4 presents the fraction of adults of each age in our sample who migrated for work in the past year, or accompanied someone who migrated for work, in the past year. These data come from various sources within the survey. For most adults, their migration data is taken from the adult survey that they answered. Information on adults who were away from the village for work at the time of the survey was reported by another household member.

Nearly 90% of males in their early 20s and over 60% of females in their early 20s migrated in the past year. The fraction of adults who migrated in the last year falls off quickly at older ages. Our data allow us to estimate age at first migration. The adult survey was administered to people 14 to 69, and asked those who had ever migrated their age at first migration for work. Among those who had ever migrated for work, the median age at first migration was 17, and the mean was 19. The tenth percentile of the distribution was 13 years, and the 90th was 29 years.

Figure 2.5 presents the fraction of adults of each age in our sample who worked for the NREGA in the past year. Compared with the short-term migrants, NREGA workers are more likely to be female and are considerably older. The pattern suggests that workers with lower potential wages in the city are the ones who stay in the village and work on the NREGA.

2.5.2 Seasonality

For the people in our sample, migration is strongly seasonal. Table 2.6 indicates that 35% of the 2,224 adults who completed the adult survey were living outside of their village for work at some point during summer 2009. Twenty-nine percent spent time outside the village during winter 2009-10, and ten percent spent time outside the village during the monsoon of 2009. When we look at the sub-sample of people who stayed in the village for at least one season, we see an even more pronounced pattern. Migration is most common in summer, followed by winter and then the monsoon.

The fifth, sixth and seventh columns of Table 2.6 suggest that the seasonality of migration is tightly linked to the seasonality of agriculture. The monsoon is the time when the most rain falls, making it possible to grow crops without irrigation. The average adult respondent spent 78 days cultivating his own land in the monsoon season of 2009 compared with 31 days in the winter and eight in the summer.

The fourth column of Table 2.6 shows that the majority of NREGA work is concentrated during the summer. Forty percent of respondents report working for the NREGA in summer 2009, while only six percent report working in the winter. Less than one percent report working during the monsoon. Interestingly, even during the summer the fraction of adults who report wanting to work for the NREGA is twice as high as the fraction who receive work. Although there is seasonality in the demand for NREGA work, the high levels of unmet demand during the winter and monsoon suggest that work is rationed by season as well as at the individual and household level.

2.5.3 Implementation of the NREGA in Practice

The results from the preceding section suggest that the NREGA is not meeting one of its goals of providing work on demand. Table 2.7 presents key summary statistics revealing how the NREGA is implemented in our study area. Out of the adults who report working during

summer 2009, 97% received full payment for the work they completed, but in many cases they had to wait a long time to get it. Only 42% say they received full payment within 30 days of work, and 22% had to wait more than 60 days. Gujarat stands out as much worse than the other states with 21% of adults still waiting to be paid one year after working when we surveyed them. The lack of payment significantly affects the earnings per day received by workers in Gujarat, who receive 20% less than those in Madhya Pradesh and Rajasthan. The lack of payment in Gujarat is particularly striking given that Gujarat's administrative capacity and infrastructure is generally considered better than the other two states.

Moving to the household-level questions in Table 2.7, the majority of households hold a program ID booklet (also called a "job card"), which is required to obtained work under the program. The ID booklet is meant to include a record of all days that the members of the household worked as well as payment received. In practice, due to corruption and lax record keeping, the booklet rarely provides an accurate picture of days worked. Gujarat is an outlier again with a smaller fraction of households having program ID booklets. The program ID booklets are meant to be free and available for all households. Fifty-five percent of households, however, report paying a fee for the booklet. Sixty-two percent of households who have a program ID booklet report that a village official is holding the booklet currently. This is concerning given that officials have been suspected of adding fake days to the booklets and pocketing the money (Dreze and Khera, 2009).

2.5.4 Migration and Household Labor Supply

By aggregating data from the adult surveys and from relatives who reported on adult family members who we were unable to survey, we found that 78% of households in the sample sent at least one migrant in the past year. Migrants lived alone during only 15% of all of the trips recorded in the last four seasons. Though people sometimes migrate with friends or distant relatives, 73% of trips reported were taken by two or more people from the same household.

About ten percent of earnings are sent back and 53% are brought back to the village.

2.5.5 Getting There and Costs of Migration

Table 2.9 presents basic summary statistics on the costs of migration based on migrants' most recent trips. Respondents' most recent destinations were on average 304 km away from their villages. About half of migrants could not tell us the distance to their place of work. Almost all knew, however, how much they had paid to travel. Missing values of distance were imputed using the relationship between transport costs and distance from the adults who were able to report both transport costs and distance.

On average, migrants paid 126 rupees to travel one way. The median transport cost was 1.03 times the average daily wage in the destination. Most migrants traveled by bus or jeep and approximately 14% took a train for at least some part of the journey. Despite these seemingly low migration costs, of those who migrated in the last four seasons, 46% said they had borrowed to pay for the cost of migrating. Of those who borrowed, 45% said they borrowed from a money lender.

Migration costs extend beyond the cost of a bus ticket. During informal conversations, respondents emphasized high fuel and food prices and lack of shelter. Indeed, 85% of those who answered the adult survey had no formal shelter while away from the village. Some cooked and slept at the construction site where they were working, but 58% simply did these things out in the open in public places.

Unfortunately, our survey cannot to speak to all of the costs of migration. We did not for instance, look at the impact of migration on migrants' health, or social standing in the village, which are emphasized in other work (Breman, 1996; Mosse et al., 2002). In the next chapter, we attempt to quantify perceived migration costs.

2.5.6 Labor Markets

The migration in our sample is largely a rural to urban phenomenon. For adults who left the village in the previous four seasons, eighty-three percent went to an urban destination during their most recent trip. Surat, Ahmedabad, and Baroda, all large cities in Gujarat, are the most common urban destinations with 61% of urban migrants reporting these cities as their destination during their most recent trip. In addition to these major destinations, migrants report traveling to 137 distinct urban locations.

The two most common types of work were construction work, done on 61% of all recorded trips, and agricultural labor, done on 17% of all recorded trips. Fourteen percent of jobs fell into the diverse "other" category. Jobs commonly listed in this category involved filling vehicles or digging for infrastructure projects.

That 17% of trips were devoted to agricultural labor suggests that the figure of 83% of trips to urban areas might be a bit high. Of the 475 trips for which agricultural labor was reported, 205 (43%) were reportedly to urban locations. It is possible that when asked where they went, some people reported the name of the nearest big city to the place they had worked. Alternatively, during informal conversations, respondents mentioned that in some cases migrants traveled to cities where employers recruited them for agricultural labor outside the city. If we were to count all trips in which the respondent reported agricultural labor as having been to rural locations, the fraction of trips to urban destinations would decrease from 83% to 76%, which still suggests a largely rural to urban movement.

In the majority of cases, migrants' relationship with employers was short-term rather than a repeated interaction over multiple trips. Panel A of Table 2.8 presents summary statistics based on responses about the most recent trip for migrants who stayed outside the village for two or more consecutive nights during summer 2010. Eighty-nine percent report doing non-agricultural work,[6] and about half worked for the same employer for the

[6]Note that this does not contradict the fact that 17% of respondents report doing agri-

whole trip. Only eight percent worked for the same employer they had worked for on their previous trip, and only about five percent arranged employment before leaving the village. Nine percent reported finding work through a labor contractor they knew from a previous trip. Only six percent migrants arranged a job before leaving.

Almost half of workers found work by going to a spot labor market (also called a "naka") in a city or town. The two next most common ways of finding a job on a trip were through a friend or relative (25% of trips) or through a labor contractor (15% of trips). Seventy-five percent of migrants were paid daily. The mean daily earnings for all workers including those who were paid at other frequencies was 123 Rupees and the median was 116 Rupees. Since migrants do not find or want to work every day, the earnings per day outside the village is 107 Rupees.

Migrant workers were typically not provided additional goods or services by their employers. Thirty percent reported being given tea on the job, and five percent reported getting lodging. Only two percent reported receiving meals, and three percent reported receiving cigarettes ("bidis").

To put migrants' earnings in perspective, Panels B and C of Table 2.8 present the mean daily earnings for those who participated in NREGA work and local non-NREGA casual work. We define "local" work to be any work that was done without sleeping more than two consecutive nights outside the village. The work could be performed in a nearby village or town as long as the respondent slept in the village at least every other night. Roughly 17% of adults reported working as local casual laborers in the last four seasons. 71% of local casual labor was done in the respondents' own village, rather than a neighboring village or town.

The average earnings of those who work in the village, either for NREGA or doing other casual labor work, are considerably lower (almost half) the average earnings of migrants.

cultural labor as some respondents report more than two types of work during a single trip.

When computing daily earnings for NREGA and local work, we restrict the sample to only trips during summer 2010 to ensure comparability of earnings. However, since we have seen that migrants and NREGA workers differ substantially from the population as a whole, some of these differences could be driven by selection.

2.6 Discussion and Future Research

The analysis and summary statistics presented in this paper scratch the surface of potential uses for the data from the survey. The detailed information on demand for NREGA work could be used to estimate long-run take-up of the program in similar populations. One rare feature of the data is that in many cases we have information on the wages in both migrant destinations and sending locations for the *same* people. This information could be used to more accurately estimate the costs of migration as it allows one to control for selection on productivity at the person-level.

The data allow a comparison of households in three different states, each with different infrastructure, language, and levels of development, but with similar distances to migration locations. In addition, we only briefly touched on the detailed migration histories. For those adults who completed the full adult survey, we asked each person's migration experience as far back as they could remember. Recall was a significant obstacle, with most adults reporting that they could not remember significant portions of their migration histories. This data could nonetheless prove valuable. Finally, the data are ideally suited to assessing to what extent the NREGA affects migrants' decision to migrate. This is the focus of the next chapter.

2.A Village Selection and Household Sampling

Initially, 102 villages were selected, but due to time and budget constraints, 32 villages were dropped. We dropped the 32 villages located furthest to the west as this made surveying in the remaining villages logistically easier. The dropped villages as well as the surveyed villages are shown in Figure 2.1. Voting lists were used to randomly sample ten households for surveying in each village.

The specific villages surveyed were selected based on pair-wise matching using proximity to each other, land composition (irrigated land, cultivable non-irrigated land, culturable waste[7]), population density, and caste composition. Data for all of these characteristics was taken from the 2001 census. A detailed description of the matching algorithm used is provided in the supplemental appendix available upon request from the author.

Voting lists are used by the state and local village governments for elections and are meant to include every adult aged 18 and older in the village. In practice, this may not be true as voting lists are updated irregularly. However, during piloting, complete household listings in three villages were completed to compare with the voting lists, one in each state. In all cases, each household enumerated in the full census had at least one adult member listed in the voting list. During the household listing, we defined a household as a group of persons living under the same roof for at least thirty days in the past year, sharing food from a common source when together, and contributing or sharing in a common resource pool. In some cases, the definition of household in the voter list did not correspond to our definition. In particular, the voter list definition was more expansive often including for example three brothers who lived separately as one household. During surveying, in cases in which the voter list and our household definitions differed, we surveyed only the economic household with the eldest household head. For this reason, more established, older households are likely

[7]Culturable waste includes lands available for cultivation but not cultivated in the past five years.

to be over-represented in the final sample.

In cases in which we were unable to survey any members of a household, we randomly selected an additional household from the voting list. For 65 of the 70 sample villages, we were able to survey ten households. In three villages, we mistakenly surveyed a total of an extra seven households. In two villages, local officials requested that we halt surveying after we had completed nine household surveys. As a result, the full sample size is 705 households from 70 villages. In one village, local officials declined to complete the village questionnaire, so that we have village-level information for only 69 of the 70 villages.

2.B Key Variable Construction

Earnings per Day Worked for Migrants The survey instrument included questions about the frequency of payment and the typical amount per pay period. In most cases (74%), respondents were paid daily and in these cases we used the typical daily payment as earnings per day worked. We also asked respondents how many days per week they typically worked. Respondents worked on average six days per week and the median respondent worked six days. For respondents who were paid weekly, fortnightly, or monthly, we used the reported payment adjusted by the typical number of days per week worked. For example, a migrant paid 800 Rupees weekly and working six days per week earns $800/6 = 133$ Rupees per day worked. For migrants that were paid irregularly or in one lump sum at the end of work, we used the total earnings from the trip divided by the number of days worked. For migrants with missing values of days worked per week, we assumed they worked six days.

Surveyors were instructed to check whether daily earnings, total earnings, trip length, and days worked per week made sense together. If they did not, they were instructed to ask the respondent for an explanation and write it down. For example, in one case, total earnings from a trip was abnormally high because the respondent was paid for work performed on a different trip. In cases in which the surveyor comments indicated that the reported variables

did not accurately measure the earnings per day worked of the respondent, we either adjusted the daily earnings or set the daily earnings to missing.

Finally, five percent of respondents received payment in-kind for their work, being paid in wheat for example. We leave these daily earnings observations as missing.

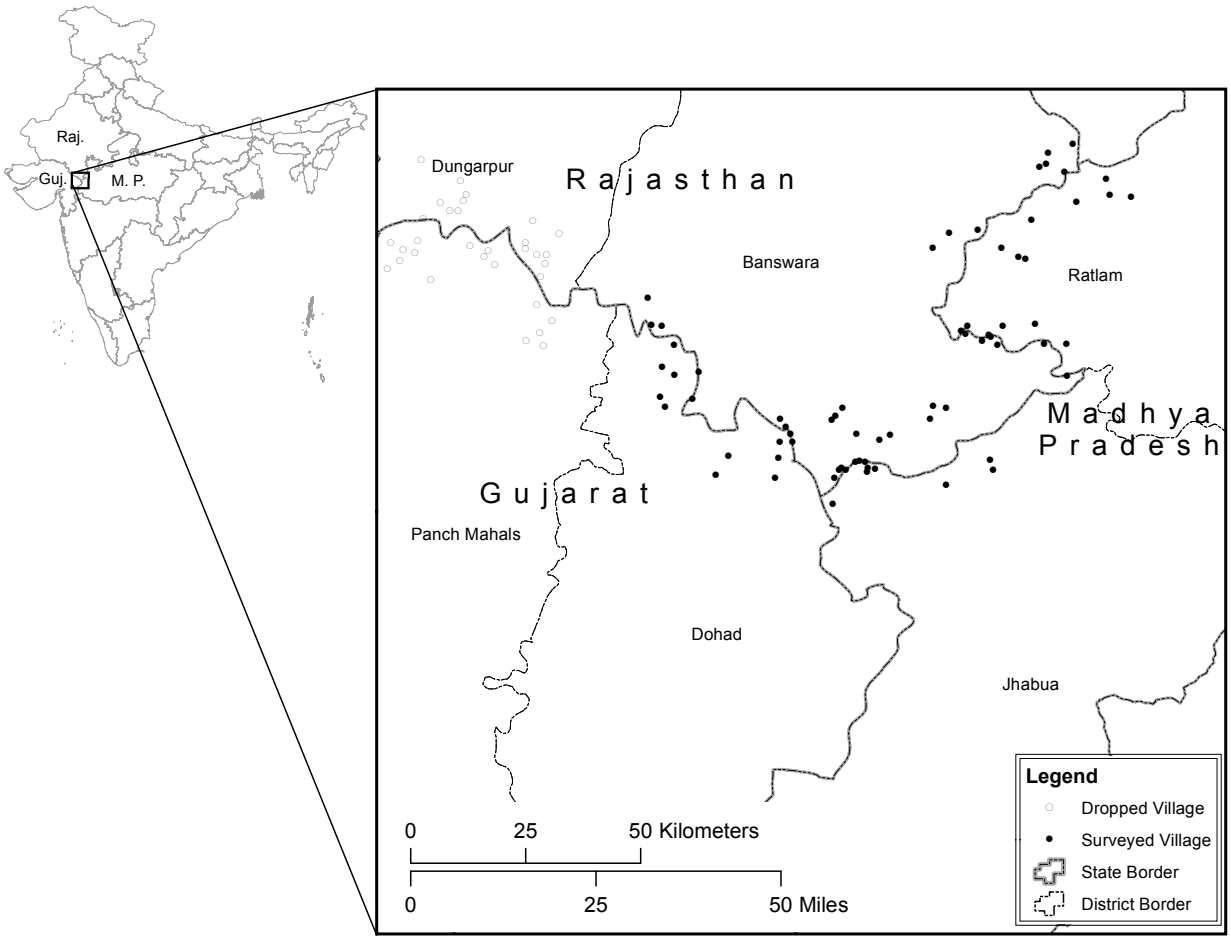

Figure 2.1: Map of Survey Villages

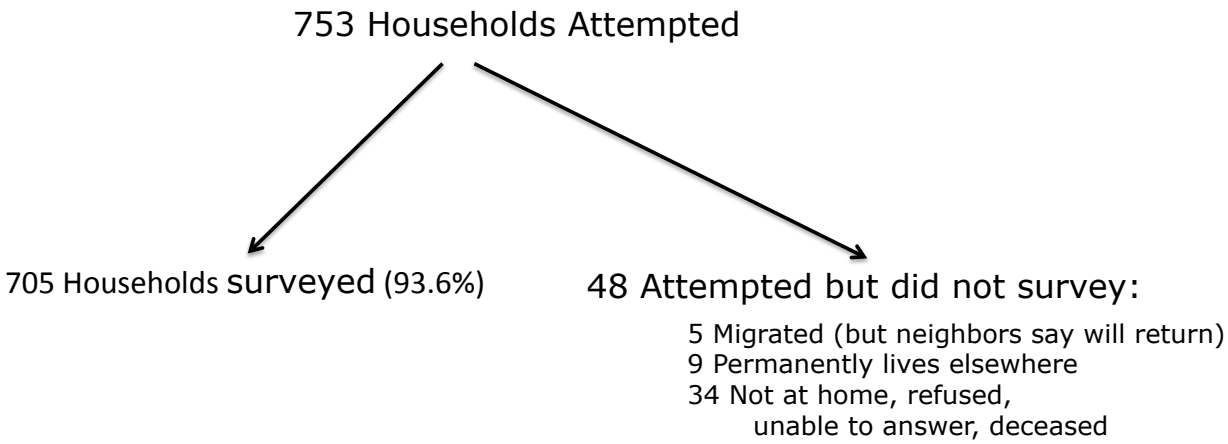

Figure 2.2: Household Survey Response Rates

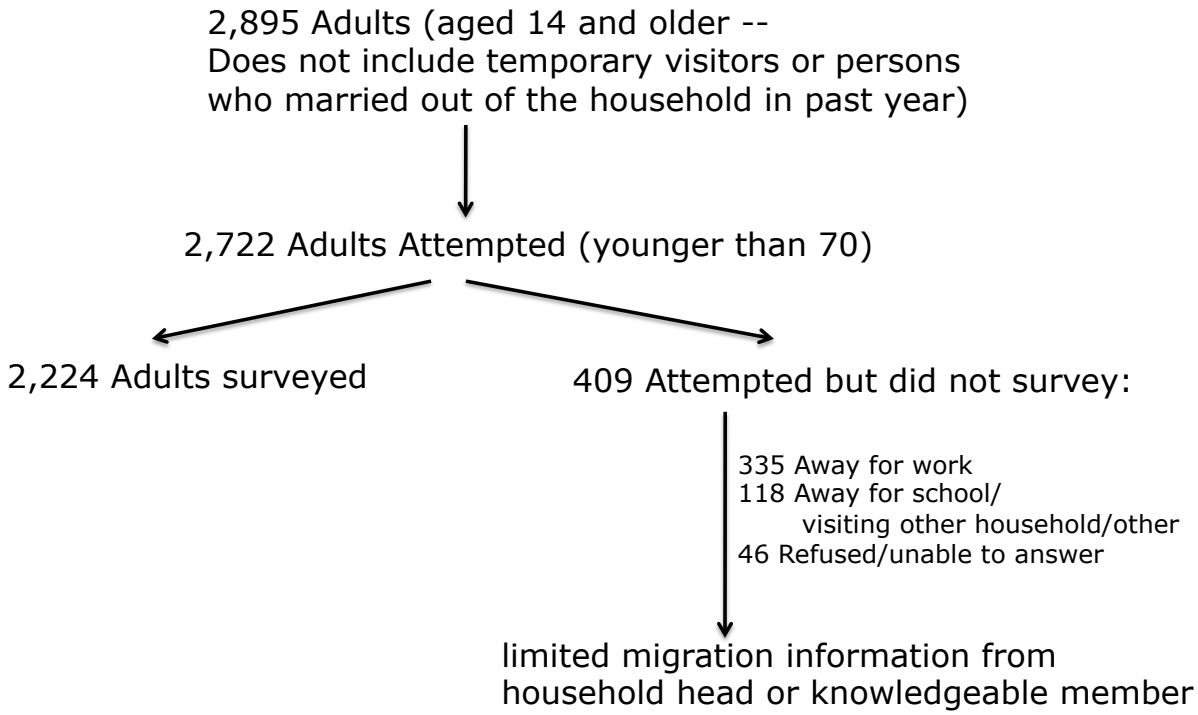

Figure 2.3: Adult Survey Response Rates



Figure 2.4: Migration by Age and Gender

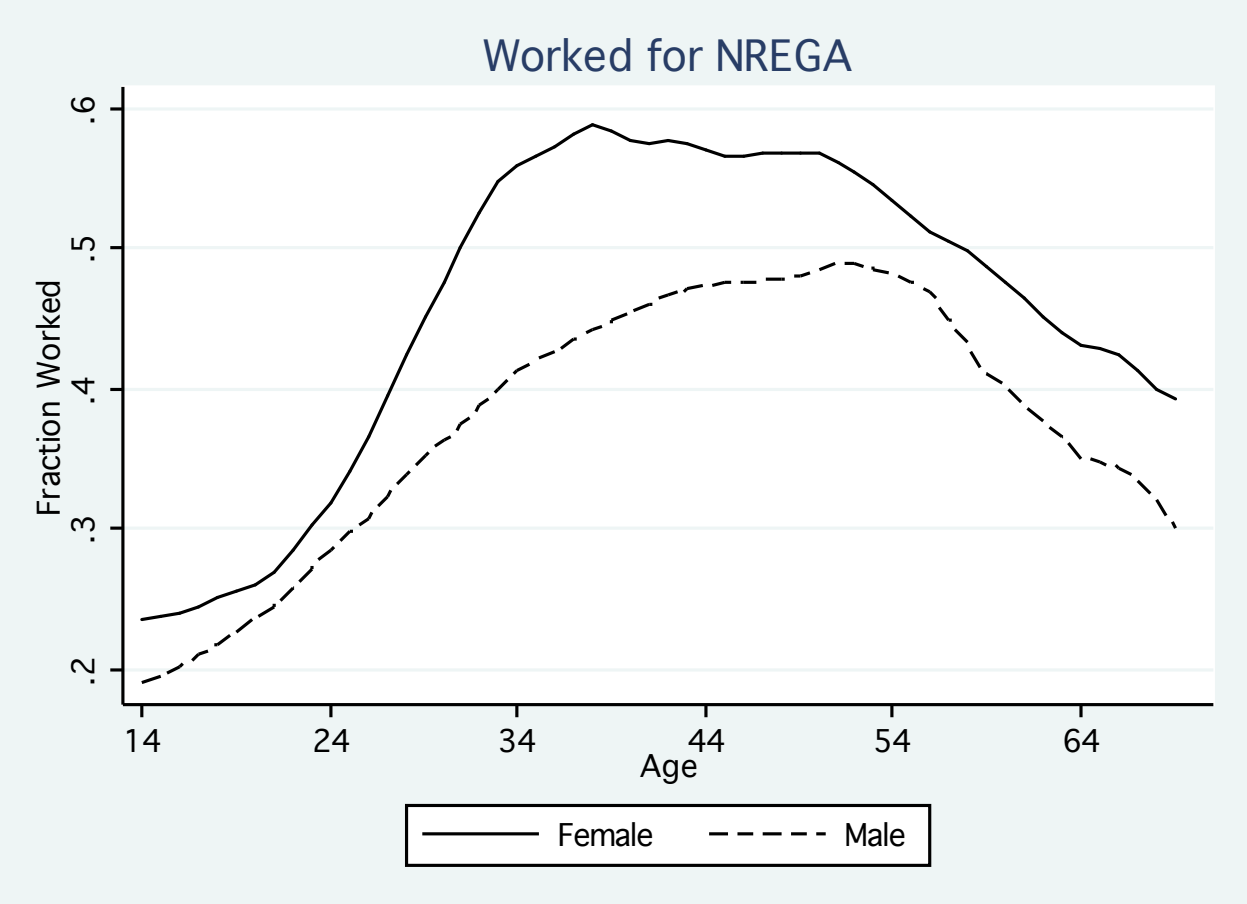

Figure 2.5: NREGA Participation by Age and Gender

Table 2.1: Summary Statistics of Sample Villages

	Gujarat	Madhya Pradesh	Rajasthan	Full Sample
	(1)	(2)	(3)	(4)
Panel A: Village-Level				
Population (median)	636	499	680	600
Population (average)	860	791	1128	972
Agricultural Wage (Male, Monsoon 2009)	97.0	70.0	71.6	74.7
Agricultural Wage (Female, Monsoon 2009)	93.0	65.8	69.6	71.7
Paved Approach	0.900	0.583	0.886	0.783
Distance to Road with Car Traffic (Km)	0.300	0.917	0.343	0.536
Distance to Railway Station (Km)	47.2	44.7	56.9	51.3
Workers from Nearby Villages Work in Village (Y/N)	0.100	0.000	0.000	0.014
Months NREGA Worksite Open per Year	4.80	5.54	5.06	5.19
Observations	10	24	35	69
Panel B: Household-Level				
Owns Cultivable Land	1.000	0.992	0.994	0.994
Cultivable Land (Acres)	2.47	2.93	2.94	2.87
Has Irrigated Land	0.500	0.536	0.516	0.521
Acres Cultivated (Summer 2010)	0.126	0.062	0.059	0.069
Acres Cultivated (Winter 2009-10)	0.917	0.838	0.991	0.926
Acres Cultivated (Monsoon 2009)	2.32	2.51	2.67	2.56
Cultivates Rice	0.480	0.571	0.637	0.591
Cultivates Wheat	0.490	0.437	0.496	0.474
Cultivates Maize	0.970	0.972	0.983	0.977
Cultivates Dal	0.390	0.460	0.337	0.389
Cultivates Cotton (for sale)	0.010	0.643	0.382	0.423
Cultivates any other crop for sale	0.270	0.512	0.388	0.416
Monsoon 2009 Yields Below Normal	0.869	0.663	0.769	0.746
Monsoon 2009 Yields Above Normal	0.040	0.037	0.000	0.019
Winter 2009-10 Yields Below Normal	0.849	0.579	0.721	0.689
Winter 2009-10 Yields Above Normal	0.094	0.357	0.274	0.279
Has Well for Irrigation or Drinking	0.570	0.496	0.444	0.481
Has Cell-phone	0.490	0.302	0.308	0.331
Has Electricity	0.540	0.298	0.219	0.293
Has Television	0.030	0.016	0.009	0.014
Dirt Floor	0.880	0.960	0.897	0.917
Household Head is Hindu	0.760	0.806	0.903	0.848
Household Head is Adivasi	0.160	0.099	0.066	0.091
Observations	100	252	353	705
Panel C: Adult-Level				
Speaks Vagri	0.023	0.572	0.946	0.665
Speaks Gujarati	0.998	0.088	0.080	0.230
Speaks Hindi	0.254	0.390	0.345	0.346
Speaks Malvi	0.000	0.084	0.024	0.042
Speaks Bhilli	0.000	0.229	0.012	0.087
Speaks Other Language	0.002	0.051	0.010	0.023
Worked for NREGA in Past Year	0.105	0.375	0.469	0.377
Observations	437	965	1,320	2,722

The unit of observation is a village, household, and adult in Panels A, B, and C respectively. Variables in Panel A are based on responses from a village official. The first three columns present means with the sample restricted to observations within each of the three surveyed states. The fourth column presents means computed using all observations. Local officials in one village in Madhya Pradesh declined to complete the village questionnaire. As a result, we have data for only 24 rather than 25 villages in the second column of Panel A and 69 out of 70 villages in the fourth column.

Table 2.2: Household Characteristics

	Own Survey (All Households) (1)	NSS Survey (All Households) (2)	NSS Round (3)
Agriculture			
Owns Cultivable Land	0.994	0.570	2004-05
Cultivable Land (Acres)	2.9	1.7	2004-05
Household Income Sources			
Own Cultivation	0.967	0.451	2009-10
Non-agricultural Business	0.033	0.197	2009-10
Salaried Work	0.071	0.118	2009-10
Migrant Labor	0.747	--	
Agricultural Labor	0.102	0.316	2009-10
NREGA Work	0.714	--	
Non-agricultural Labor	0.128	0.181	2009-10
Demographics			
Children Aged 13 or Less	2.8	1.4	2009-10
Adults	4.4	3.2	2009-10
Household Size	7.2	4.6	2009-10
Household Head			
Scheduled Tribe	0.626	0.109	2009-10
Hindu	0.848	0.844	2009-10
Adivasi	0.091	--	
NREGA			
Household participated in past year	0.718	0.249	2009-10
Number of days worked	41.6	9.03	2009-10
Tried but did not receive work	--	0.198	2009-10
Permament Migrant (outside district)			
All	0.091	0.112	2004-05
Reason other than marriage	0.037	0.088	2004-05
Observations	705	79,297	2004-05
		59,129	2009-10

The unit of observation is a household. The first column includes means computed using all households in the survey presented in the paper. The second column includes means computed using NSS Survey Round 2004-05 or NSS Survey Round 2009-10. NSS Survey Round 2004-05 refers to the rural sample of round 61 of the NSS Employment survey conducted from July 2004 to June 2005. NSS Survey Round 2009-10 refers to the rural sample of round 66 of the NSS Employment survey conducted from July 2009 to June 2010. Estimates from both NSS surveys are computed using survey sampling weights. "--" denotes not available. For the NSS sample, permanent migrants are defined as persons who left the district between one and five years ago and have not returned. Permanent migrants in our survey sample are defined as persons who "permanently" left the household within the last five years and are currently in another district. The row labeled "Tried but did not receive work" reports the fraction of households that report attempting to get work under NREGA but not receiving work.

Table 2.3: Summary Statistics and Comparison with National Rural Population

	Own Survey				NSS Survey 2007-08
	All Adults	Full Adult Survey Completed	Adult Survey not Completed	Difference (3) - (2)	All Adults
	(1)	(2)	(3)	(4)	(5)
Female	0.511	0.525	0.448	-0.077	0.497
	(0.0056)	(0.0166)	(0.0067)	(0.019)	(0.001)
Married	0.704	0.729	0.594	-0.134	0.692
	(0.0091)	(0.021)	(0.0105)	(0.0233)	(0.0017)
Illiterate	0.666	0.683	0.590	-0.093	0.385
	(0.0185)	(0.0325)	(0.0189)	(0.0302)	(0.0028)
Primary School	0.248	0.238	0.291	0.053	0.439
	(0.0105)	(0.0247)	(0.0118)	(0.0272)	(0.0024)
Secondary Education	0.118	0.112	0.143	0.030	0.097
	(0.0126)	(0.0223)	(0.0121)	(0.0186)	(0.0012)
Tertiary Education	0.016	0.016	0.014	-0.002	0.080
	(0.0042)	(0.0061)	(0.0049)	(0.0079)	(0.0015)
Age	32.8	34.1	27.0	-7.11	34.4
	(0.248)	(0.484)	(0.301)	(0.592)	(0.0452)
Spent 2-330 days away for work	0.464	0.422	0.649	0.226	--
	(0.0194)	(0.0383)	(0.0187)	(0.0385)	
Migrated for Work all Three Seasons	0.139	0.080	0.402	0.322	--
	(0.0135)	(0.0437)	(0.0101)	(0.0447)	
Ever Worked for NREGA	0.528	0.581	0.291	-0.290	--
	(0.0253)	(0.0354)	(0.0259)	(0.0332)	
Spent 30-180 days away for work	0.313	0.311	0.321	0.010	0.025
	(0.0158)	(0.0331)	(0.0165)	(0.034)	(0.0009)
Adults	2,722	2,224	498		252,711

The unit of observation is an adult. Standard errors computed assuming correlation of errors at the village level in parentheses. The first four columns present means based on subsets of the adults aged 14 to 69 from the main data set discussed in the paper. The first column includes the full sample of persons aged 14 to 69 for whom the adult survey was attempted. The second column includes all persons aged 14 to 69 for which the full adult survey was completed. The third column includes all persons aged 14 to 69 for which the full adult survey was not completed. The fourth column presents the difference between the third and second columns. The fifth column presents means computed using all adults aged 14 to 69 in the rural sample of the NSS Employment and Unemployment survey Round 64 conducted between July 2007 and June 2008. Means from the NSS survey are constructed using sampling weights. "--" denotes not available.

Table 2.4: Characteristics of Persons Who Did and Did Not Migrate Short-term

	Own Survey Migrated Short-term (1)	Own Survey Did Not Migrate Short-term (2)	NSS Survey 2007-08 Migrated Short-term (3)	NSS Survey 2007-08 Did Not Migrate Short-term (4)
Panel A: All Adults				
Female	0.421	0.588	0.145	0.506
Married	0.725	0.687	0.701	0.691
Age	28.1	36.9	30.4	34.5
Illiterate	0.661	0.671	0.388	0.385
Some School or Literate	0.422	0.367	0.494	0.437
Secondary	0.021	0.030	0.063	0.098
Tertiary	0.007	0.023	0.055	0.080
Adults	1,257	1,465	15,265	237,446
Panel B: Migrants Only				
Industry				
Construction	0.619		0.419	
Agriculture	0.162		0.231	
Manufacturing	0.035		0.169	
Other	0.184		0.181	
Destination - Urban	0.832		0.677	
Adults with Industry and Destination Data	1,116		15,265	

The unit of observation is an adult. All samples are restricted to adults aged 14 to 69. The first and second columns present means for all adults for which we attempted a full adult survey. The first column includes means for adults who migrated short-term within the past year. The second column includes means for adults who did not migrate short-term. The third and fourth columns include means for adults from the rural sample of the NSS Employment and Unemployment survey Round 64 conducted between July 2007 and June 2008. The third column restricts the sample to those who did migrate short-term within the past year. The fourth column restricts the sampe to those who did not migrate short-term. Means from the NSS survey are constructed using sampling weights. Short-term migrants are persons who spent between 30 and 180 days away from the village for work in the past year in the NSS survey and between 2 and 330 days in our survey. "Destination - Urban" is determined based on the most recent trip for persons in our sample and based on longest trip for persons in the NSS Sample. The first column of Panel B presents means for all short-term migrants who completed the full adult survey.

Table 2.5: NREGA Participants and Non-Participants

	Own Survey Worked for NREGA (1)	Own Survey Did Not Worked for NREGA (2)	NSS Survey 2009-10 Worked for NREGA (3)
Female	0.558	0.483	0.476
Married	0.794	0.650	0.812
Age	36.6	30.5	36.6
Illiterate	0.756	0.612	0.528
Some School or Literate	0.302	0.447	0.389
Secondary	0.006	0.038	0.056
Tertiary	0.009	0.020	0.027
Observations	1,027	1,695	5,346

The unit of observation is an adult. The sample is restricted to adults aged 14 to 69. The first and second columns include means for all adults for which we attempted a full adult survey. The first column reports means for adults who worked for the NREGA within the past year. The second column reports means for all other adults. The third column reports means from round 66 of the NSS Employment and Unemployment Survey for persons who either reported working in public works employment in the past seven days or report usually or occasionally working in public works employment in the past year. Round 66 was conducted between July 2009 and June 2010. Means from the NSS survey are constructed using sampling weights. We do not include a fourth column with adults from the NSS sample who did not work for the NREGA within the past year because it impossible to determine that an adult did not work for the NREGA within the past year based on the NSS questions.

Table 2.6: Agricultural Seasonality, Short-term Migration, and Program Participation

	Left Village for Work						
	Full Sample	Stayed in village at least one season	Wanted NREGA Work	Worked for NREGA	Cultivated Own Land More than 2 Weeks	Days Spent Cultivating Own Land	Fraction of Total Rainfall
Season	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Winter 2009-10	0.29	0.23	0.75	0.06	0.55	30.7	0.00
	(0.016)	(0.014)	(0.011)	(0.009)	(0.022)	(1.76)	
Monsoon 2009	0.10	0.02	0.54	0.00	0.86	77.6	0.93
	(0.011)	(0.003)	(0.019)	(0.002)	(0.011)	(1.92)	
Summer 2009	0.35	0.29	0.82	0.40	0.20	8.40	0.06
	(0.019)	(0.019)	(0.011)	(0.025)	(0.015)	(0.48)	
Observations	2,224	2,046	2,224	2,224	2,224	2,224	--

The unit of observation is an adult. The sample is restricted to adults aged 14 to 69 who completed the full adult survey. The first column presents the fraction of adults who left the village for work during each season. The second column presents the same fraction with the sample restricted to individuals that spent at least one full season during the 2009-10 agricultural year in the village. The third column presents the fraction of adults who report wanting to work for the NREGA during each season. The fourth column presents the fraction of adults who report working for the NREGA during each season. The fifth column presents the fraction of individuals who spent at least two weeks cultivating their own land in the village. The sixth column presents the average days spent cultivating land across all adults in the sample. The seventh column presents the fraction of total rainfall occuring during each season based on averages of readings taken from metereological stations throughout each of the five study districts from 2006 to 2010. Raw data is avaiable from IMD website. Standard errors computed assuming correlation of errors at the village level in parentheses. See text for definition of seasons.

Table 2.7: NREGA Implementation

	Gujarat	Madhya Pradesh	Rajasthan	Full Sample
	(1)	(2)	(3)	(4)
Panel A: Adult-Level				
Worked for NREGA Summer 2009	0.100	0.391	0.497	0.402
Full Payment within 30 Days of Work	0.364	0.471	0.401	0.422
Full Payment within 60 Days of Work	0.455	0.765	0.810	0.782
Full Payment within 90 Days of Work	0.576	0.887	0.916	0.894
Full Payment Ever	0.788	0.945	0.984	0.964
Earnings per Day Worked Summer 2009	52.2	64.1	65.2	64.4
Adults	330	749	1145	2,224
Panel B: Household-Level				
Household has Program ID Booklet	0.750	0.940	0.980	0.933
Paid a Fee for Program ID Booklet	0.410	0.452	0.658	0.549
Village Official Holding ID Booklet	0.680	0.643	0.581	0.617
Made Request for Work	0.230	0.246	0.406	0.323
Request Led to Receipt of Work	0.348	0.581	0.923	0.771
Why Not Request Work? (Made Request for Work = 0)				
Not Aware of Application Process	0.701	0.532	0.534	0.560
Will not Receive Work Anyway	0.078	0.016	0.034	0.034
Not Interested in NREGA Work	0.039	0.016	0.005	0.015
Work Available without Request	0.065	0.458	0.471	0.400
Households	100	252	353	705

The unit of observation is an adult and household for Panels A and B respectively. The sample for Panel A is restricted to adults aged 14 to 69 who completed the full adult survey. The sample for Panel B is comprised of all surveyed households. The first three columns present means with the sample restricted to adults living in each of the three surveyed states. The fourth presents means using full sample. With the exception of the last four rows, the means for variables on indented lines are computed using the sample restricted to adults or households for which the previous non-indented variable is equal to one. For example, the mean for "Full Payment within 30 Days of Work" is restricted to those adults who worked for the NREGA during summer 2009. "Full Payment within X Days of Work" is equal to one if the individual received full payment for NREGA work within X days of completing the work. "Made Request for Work" is equal to one if the household head reported that any member of the household made a written or other request for NREGA work. "Request Led to Receipt of Work" is equal to one if any member received work based on the request. See appendix for description of the "Why Not Request Work?" variables.

Table 2.8: Job and Employer Characteristics

Panel A: Work during Trips Outside the Village	
Worked for Same Employer Full Trip	0.470
Found Work at Spot Labor Market	0.488
Worked for Same Employer during Previous Trip	0.080
Arranged Job before Leaving	0.056
Job Through Labor Contractor from Previous Trip	0.093
Received Payment Daily	0.748
Salaried Work	0.006
Casual Work/Self-Employed	0.994
Did Not Find Work	0.001
Non-agricultural Work	0.888
Mean Earnings per Day Worked	123
Median Earnings per Day Worked	120
Mean Earnings per Day Out of Village	107
Median Earnings per Day Out of Village	100
Observations	913
Panel B: NREGA Work	
Mean Earnings per Day Worked	62.8
Median Earnings per Day Worked	60.0
Observations	594
Panel C: Local Non-NREGA Casual Work	
Mean Earnings per Day Worked	74.6
Median Earnings per Day Worked	60.0
Mean Earnings per Day Worked or Looking for Work	72.4
Median Earnings per Day Worked or Looking for Work	60.0
Observations	178

The unit of observation is an adult. All samples are restricted to adults aged 14 to 69 who completed the full adult survey. Panel A presents means for variables with the sample restricted to adults who stayed away from the village for work for two or more nights during summer 2010. Means are computed based on the most recent trip for each adult. Panels B and C includes means of variables for work performed during summer 2010. Each observation corresponds to a different person. Panel C is restricted to casual work which is informal, temporary work in which workers are often paid daily. See appendix for description of variable construction.

Table 2.9: Migration Costs

Travel Costs	
Average Distance (Km)	304
Median Distance (Km)	275
Median Transportation Cost as Fraction of Wage	1.03
Shelter	
No Formal Shelter	0.846
Rented	0.020
Employer Provided, Other	0.134
Transportation	
Bus or Jeep	0.960
Train	0.139
Other	0.012
Observations	1,116

The unit of observation is an adult. The sample is restricted to adults aged 14 to 69 who completed the full adult survey and who stayed away from the village for work for two or more nights in the past year. Variables are computed based on the most recent trip for each adult. Missing values of distance are imputed using relationship between transporation costs and distance for trips with non-missing values of distance and transportation cost.

Chapter 3

Short-term Migration and India's Employment Guarantee

3.1 Introduction

Workfare programs are common anti-poverty policies. Many developing countries have programs that hire workers at competitive wage rates with the goal of increasing the income of the poor.[1] One justification for workfare programs is that the seasonality of agricultural work allows a workfare program operating during the off-season to create productive assets without decreasing private sector employment or crowding out other income earning opportunities (Ravallion, 1987). Indeed, a substantial literature documents high rates of unemployment in rural labor markets (Rudra, 1982; Binswanger and Rosenzweig, 1984; Dreze and Mukherjee, 1989; Datt, 1996).

This paper uses newly collected data from a high out-migration area of India to present evidence that an off-season workfare program has a significant impact on private sector work through its impact on short-term migration. As a result, the net income effects of the program are much less than the wage offered by the program and possibly even negative, though this does not rule out substantial welfare gains for participants.

We present five pieces of evidence suggesting that a substantial number of workfare participants would have migrated for work if not for the program. First, eight percent of surveyed adults report that had they not received work under the program, they would have migrated for work. Second, a substantial fraction of migrants report working for the workfare program suggesting that at the margin, work under the program is at least as attractive as short-term migration. Third, 88% of short-term migrants report that if provided more days under the program they would work more. Fourth, we document a robust negative correlation between migration and employment under the workfare program. Finally, we show that adults living in a state that provides more days of government work, even conditional

[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.

on demand for government work, spend less time outside the village for work compared with other states. This cross-state difference in days spent outside the village for work is statistically significant only during the summer season during which most of the government work is provided.

It is perhaps surprising that migration appears to be so strongly affected by the workfare program, especially given that earnings outside of the village are nearly twice the level of workfare daily earnings. The gap in earnings could simply reflect differential productivity between migrants and participants in the government program, but the wage differential persists even for adults who report both working for the government program and migrating. The large wage differentials combined with high demand for work under the workfare program suggest substantial migration costs.

We investigate migration costs formally by modeling short-term migration using a framework similar to Benjamin (1992)'s. Short-term migration provides a higher monetary return than local work but requires a fixed cost. Workers also incur a flow cost for each day spent away. Using this framework and the survey data, we are able to draw some conclusions about the flow costs of migration. Specifically, we compare the daily earnings of migrants who report wanting to work more for the workfare program with the government wage provided by the program. Since these individuals have already paid the fixed costs of migration, this difference is informative of the minimum marginal costs migrants incur along the intensive margin. For example, a migrant who earns 80 rupees per day in the city but reports a willingness to work for 50 rupees in the village faces a per day cost of migration of at least 30 rupees. We find that for the average migrant, the flow costs of migration are 75% of daily earnings in the city.

The findings of this study suggest that the potential spill-over effects to urban areas from a rural workfare program could be large, especially in areas with high levels of short-term migration. More generally, many policies and programs in developing countries are rural-

only. The present study presents one channel through which the effects of these rural policies may spill over to urban areas. The results suggest that even workfare programs operating during the off-season can have a significant impact on private sector employment, and that for many workers, even the off-season opportunity cost of time is considerably greater than zero.

The following section describes the workfare program and the study area in more detail. Section 3.4 presents the data set used throughout the paper. Sections 3.5 and 3.6 present the main results on the interaction between the government program and migration. Section 3.7 uses information on the earnings of migrants to put bounds on migration costs. Section 3.8 concludes.

3.2 Context

The rural workfare program studied in this paper is India's National Rural Employment Guarantee Act (NREGA). The act, passed in September 2005, entitles every household in rural India to 100 days of work per year at a state-level minimum wage. Based on government administrative data, in 2010-11 the NREGA provided 2.27 billion person-days of employment to 53 million households.[2] Chapter One describes the act and its implementation in detail. Here, we highlight the details relevant for the current study.

Work under the act is short-term, often on the order of a few weeks per adult. Households with at least one member employed under the act during 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] Within the study area as well as throughout India, work under the program is rationed. During agricultural year 2009-10, an estimated 19% of households re-

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

[3]Author's calculations based on NSS Round 66 Employment and Unemployment Survey. Chapter one describes the employment surveys in detail.

ported attempting to get work under the act without success.[4] The rationing rule that local officials use varies from village to village and is unfortunately unknown to us. Conversations with survey respondents suggested that in some cases local officials provided work preferentially to certain households, while in other cases a certain number of household members from each household were allowed to work for the act. In all cases, work was primarily available during the summer, and most respondents were actively recruited for work by village officials rather than applying for work.

We study the interaction of the act with short-term migration. Conventional models of rural to urban migration treat migration as a long-term decision (Harris and Todaro, 1970). Yet considerable evidence outside of economics (Haberfeld et al., 1999; Mosse et al., 2002; Smita, 2008; Deshingkar, 2006) and an increasing number of studies within economics (Banerjee and Duflo, 2007; Badiani and Safir, 2009; Chowdhury et al., 2009) suggest that a significant fraction of migration is short-term. In India, an estimated 60% of migrants who leave rural areas for work each year spend less than six months away from the village.[5] In most of the cases studied, short-term migration is seasonal, either driven by seasonality in demand in destinations, or seasonality in local opportunities. Some authors have recommended government policies encouraging short-term migration in areas with acute seasonal variation in earnings opportunities (Khandker, 2012). Other studies and papers have suggested that the NREGA may be impacting migration (Jacob, 2008; Ashish and Bhatia, 2009).

Migration in the study sample is discussed in detail in Chapter Two. We note here that trips are short with median length of the most recent trip reported by migrants of 30 days. As found in other studies, migration is concentrated during the agricultural off-season. Jobs are generally low-skill with short-term employer-employee relationships. On their most

[4]Author's calculations based on NSS Round 66 Employment and Unemployment Survey. Chapter one describes the employment surveys in detail.

[5]Author's calculations based on Round 64 of the NSS Employment Survey conducted from July 2007 to June 2008.

recent trip, forty-eight percent of adults found work at a spot labor market, 62% worked in construction, 75% were paid daily, and only 8% worked with the same employer as on a previous trip.

Our analysis uses data collected from a sample of households living within villages in the states Gujarat, Rajasthan, and Madhya Pradesh. The villages were selected to be on the border of the three states. The survey location was selected because previous studies in the area reported high rates of out-migration and poverty (Mosse et al., 2002), and because surveying along the border of the three states provided variation in state-level policies. The survey instrument and data are described in detail in Chapter Two.

3.3 Model of Out-of-Village Work

The model of short-term migration serves two purposes. First, it helps us identify individuals who likely reduced their time spent working outside the village as a result of the NREGA. Second, the model provides a framework for estimating the variable costs of migration.

3.3.1 Set-up

Consider a potential migrant living in a rural area. She has a time endowment of one which is split between work within the village t_v and work outside of the village $1 - t_v$. In-village earnings take the form $f(t_v)$ with $f(\cdot)$ increasing and concave and $f'(0) >> 0$. Leaving the village requires a fixed cost c_f and a variable cost c_v per unit of time spent outside the village. While outside the village, migrants earn w_u per day away. Time spent within the village t_v solves:

$$\max_{t_v} f(t_v) - c_f 1_{\{t_v<1\}} + (w_u - c_v)(1 - t_v) \tag{3.1}$$

$$\text{such that } t_v \in [0, 1] \tag{3.2}$$

For any interior solution $t_v < 1$, the optimal period of time spent in the village is $t_v^* = t_{min}$ where $f'(t_{min}) = w_u - c_v$. As a result, leaving the village for work is optimal if and only if:

$$f(t_v^*) - c_F + (w_u - c_v)(1 - t_v^*) > f(1) \tag{3.3}$$

Figure 3.1 depicts the case in which this inequality holds so that it is optimal to migrate.

3.3.2 Introducing Government Work

Next, we consider what happens when a fixed (small) amount of government work within the village t_g is provided at wage w_g. If $w_g > w_u - c_v$, migrants will spend $t_v^* = t_{min} + t_g$ days within the village and will work for the government program. Even if $w_g < w_u - c_v$, migrants may still work more for the program if:

$$f(1 - t_g) + w_g t_g > f(t_v^*) - c_F + (w_u - c_v)(1 - t_v^*) + f(1) \tag{3.4}$$

in which case migrants will stop migrating completely and stay back in the village to work for the program. Figure 3.2 presents the case in which t_g is small enough that the individual remains a migrant. We have three groups of individuals:

1. Individuals with $w_g > w_u - c_v$ and $f(1-t_g)+w_g t_g < f(t_v^*)-c_F+(w_u-c_v)(1-t_v^*)+f(1)$ will work for the government program and continue to migrate but will reduce the days spent migrating one-for-one with days spent working for the program.

2. Individuals with $f(1 - t_g) + w_g t_g > f(t_v^*) - c_F + (w_u - c_v)(1 - t_v^*) + f(1)$ will work for the government program and stop migrating completely.

3. Individuals with $w_g < w_u - c_v$ and $f(1-t_g)+w_g t_g < f(t_v^*)-c_F+(w_u-c_v)(1-t_v^*)+f(1)$ will choose not to work for the government program.

Our contribution is estimating the size of each of these groups and estimating the distribution of migration costs within the population of migrants based on the method described next.

3.3.3 Estimating Variable Migration Costs

Suppose that variable migration costs within the population of current migrants are distributed according to $N(\mu_c, \sigma_c)$. And suppose for each individual i, we observe potential earnings per day outside the village (w_u^i), earnings per day of government work (w_g^i) and a dummy variable for whether the individual would work more for the government program if provided work ($WANT_i$). The likelihood conditional on the parameters μ_c, σ_c is

$$L(\{w_g^i, w_u^i, WANT_i\}|\mu_c, \sigma_c) = \sum_{WANT_i=1} \log\left(\Phi(\frac{w_u^i - w_g^i - \mu_c}{\sigma_c})\right) + \sum_{WANT_i=0} \log\left(1 - \Phi(\frac{w_u^i - w_g^i - \mu_c}{\sigma_c})\right) \quad (3.5)$$

Section 3.7 discusses the resulting likelihood estimates of μ_c and σ_c.

3.3.4 Discussion

One possibility that the model abstracts from is that individuals may be able to participate in government work without reducing time spent outside the village or in-village earnings. For example, to the extent that agricultural production within the village requires intense periods of work followed by short periods of time spent waiting for the next stage in the agricultural process, individuals may be able to work for the government without sacrificing any agricultural output. Although plausible, in the current application this is unlikely to be relevant since the vast majority of work is provided during the agricultural off-season.

Another point worth noting is that the model assumes individuals' utility functions are linear in earnings and that there is no leisure choice. More generally, one could think of $f(t_v)$ as capturing utility from time spent in the village after the individual has optimally

chosen in-village work and leisure given a time constraint of t_v, and one could interpret $(w_u - c_v)(T - t_v) - c_f 1_{\{t_v<T\}}$ as capturing utility from time spent outside the village. The variable cost c_v would then include the value of leisure outside the village.

3.4 Data

The survey consists of household, adult and village modules. The sample includes 705 households living in 70 villages in the states Gujarat, Rajasthan and Madhya Pradesh. The household module was completed by the household head or other knowledgable member. One-on-one interviews were attempted with each adult aged 14 to 69 in each household. In 69 of the 70 villages, a local village official answered questions about village-level services, amenities and labor market conditions.

The analysis in this paper focuses entirely on those adults who completed the full one-on-one interviews. Table 3.1 presents means of key variables for the subset of adults who answered the one-on-one interviews as well as all adults in surveyed households. Out of 2,722 adults aged 14-69, we were able to complete interviews with 2,224 (81.7%).

The fourth column of the table presents the difference in means between adults who completed the one-on-one interview and those who did not. The 498 adults that we were unable to survey are different from adults that were interviewed along a number of characteristics. Perhaps most strikingly, 40% of the adults that we were unable to survey were away from the village for work during all three seasons of the year compared with eight percent for the adults that we did interview. It should therefore be kept in mind when interpreting the results that migrants that spend most of the year away from the village are underrepresented in our sample.

To assess how the adults in our sample compare with the rural population in India, the fifth column of Table 3.1 presents means from the rural sample of the nationally representative NSS Employment and Unemployment Survey. Literacy rates are substantially lower

in the study sample compared with India as a whole, reflecting the fact that the study area is a particularly poor area of rural India. The NSS asks only one question about short-term migration, which is whether an individual spent between 30 and 180 days away from the village for work within the past year. Based on this measure, adults in our sample are 28 percentage points more likely to migrate short-term than adults in India as a whole. Part of this difference may be due to the fact that our survey instrument was specifically designed to pick up short-term migration, though most of the difference is more likely due to the fact that the sample is drawn from a high out-migration area.

Table 3.2 presents means of variables for different subgroups. Adults who reported migrating within the past year (Column One) are much younger, less educated, and more likely to be male than respondents on average. NREGA participants (Column Two) are slightly older, much less educated and more likely to be female than the average respondent. NREGA participants who report not wanting to work for the NREGA (Column Three) are more educated and slightly more likely to be female than the average respondent. The measures of household wealth such as land holdings, whether the household owns a cell phone, and whether the household has an electricity connection do not differ substantially across groups.

3.4.1 Seasonality

The lives of the people in the study area are closely linked to the agricultural cycle. As a result, it was crucial that the survey instrument collect data for each individual over the course of at least one full agricultural year. Since migration, cultivation, and participation in the NREGA are all highly seasonal, the survey instrument included questions about each activity separately for summer 2010, winter 2009-10, monsoon 2009, and summer 2009. Most respondents were surveyed between mid summer 2010 and early monsoon 2010, so that in many cases, summer 2010 was not yet complete at the survey date. As a result, when we

refer to a variable computed over the past year, it corresponds to summer 2009, monsoon 2009, and winter 2009-10. Respondents were much more familiar with seasons than calendar months, and there is not an exact mapping from months to seasons. Summer is roughly mid-March through mid-July. The monsoon season is mid-July through mid-November, and winter is mid-November through mid-March.

Table 3.3 presents some key variables by season. NREGA work is concentrated during the summer months. Migration is concentrated during the summer and winter.

3.4.2 Measuring Demand for NREGA Work

An important variable for the following analysis is whether an individual wanted to work more for the NREGA during a particular season. Specifically, the question, translated to English is, "if more NREGA work were available during [season] would you work more?" for individuals who had worked for the NREGA. For individuals who did not work for the NREGA, we asked "did you want to work for the NREGA during [season]?" One should be skeptical that the answer to these questions truly indicates a person's willingness to work. Later, we show that the correlations between the response to the resulting measure of demand and respondent characteristics are sensible.

Table 3.3 presents the fraction of respondents who report wanting more NREGA work by season and the answers to the questions about why. The table confirms the seasonal pattern in NREGA participation as well as the significant unmet demand for NREGA work.

3.4.3 Measuring Earnings

In order to assess the costs of migration, we require reliable measures of the wage that NREGA participants and migrants earn. Given the short-term nature of most migrant jobs, the same migrant might work for multiple employers for different wages within the same season. For this reason, the survey instrument included questions about earnings, wages,

and jobs for each trip within the past four seasons up to a maximum of four trips. Some migrants still might hold multiple jobs and therefore earn different wages within the same trip, but daily earnings and wages are more likely to be constant within the same migration trip than within the same season. In total, this yields wage observations for 2,749 trips taken by 1,125 adults.

So that we do not overweight migrants who took more frequent, shorter trips relative to migrants who took less frequent, longer trips, we calculate the average wage for each migrant for each season that the migrant was away. Appendix 3.A describes the construction of the earnings measures in detail. Table 3.4 presents summary statistics. The table reveals that during summer 2009, out of 768 migrants, we have non-missing earnings for only 593 (77%). This is because for some adults who took more than four trips, we did not record information for any of the trips taken during summer 2009. For these adults and all adults with non-missing summer 2010 earnings, we construct predicted earnings for summer 2009 by projecting summer 2009 earnings onto summer 2010 earnings and dummies for whether the person was engaged in migrant agricultural labor during summer 2009 and summer 2010. The mean for the resulting earnings per day away is provided in Row 6 of Table 3.4.

The second half of Table 3.4 presents the measures of daily earnings for NREGA work. Importantly, some respondents report never having been paid. Out of the 895 adults who worked for the NREGA during summer 2009, 32 (3.6%) report not having been paid in full at the time of the survey. Assuming a wage of zero for those who were not paid yields a wage of 64.4 rupees per day compared with 67 for only those who were paid. For the following analysis, we will need a measure of daily earnings on NREGA that non-NREGA participants would expect to receive. We predict NREGA earnings during summer 2009 for non-participants with a linear regression using summer 2010 NREGA daily earnings, a gender dummy, age, age squared, and dummies for highest education achieved, and state. Interestingly, none of the predictors except summer 2010 NREGA daily earnings are statis-

tically significant, suggesting that the NREGA wage does not vary with productivity. In contrast, gender and age are good predictors of migration earnings.

3.5 Government Employment and Migration

As discussed in Section 3.3, we are interested in three subgroups. Individuals who both migrated and worked for the NREGA or want work, individuals who would have migrated had they not received NREGA work, and individuals who neither worked for the NREGA nor want to work for the NREGA.

From the first column of Table 3.5 we see that during summer 2009, 40% of adults worked for the NREGA (first row). Thirty percent of the NREGA participants or 12% of all adults migrated as well (third row). Based on the model, we would expect that these individuals substituted away from migration towards NREGA work. However, this is only a lower bound on the impact of the NREGA on migration. An additional 8.4% of adults report that they would have migrated had they not worked for the NREGA (fourth row). These results suggest that at its current levels, NREGA work reduced migration for 20% of adults or roughly half of migrants.

As shown previously, there is significant unmet demand for NREGA work with a large fraction of respondents reporting that they would work more for the NREGA if more work were available. Demand is particularly high among migrants. During summer 2009, 88% of migrants report that they would have worked more during summer 2009 if more NREGA work had been available. This suggests that an expansion of the NREGA in the study area would significantly impact migration.

In fact, migrants are actually more likely than non-migrants to want NREGA work. Table 3.6 presents estimates from regressions of the form:

$$WantWork_{it} = \beta_0 + \beta_1 Migrant_i + \beta_2 Migrate_{it} + \gamma X_{it} + \epsilon_{it} \tag{3.6}$$

where $WantWork_{it}$ is a dummy variable equal to one if individual i reported that they would work more for the NREGA if more work were available in season t (t is summer 2009, monsoon 2009, or winter 2009-10), $Migrant_i$ is a dummy variable equal to one if individual i left the village for work within the past year, $Migrate_{it}$ is a dummy variable equal to one if individual i left the village for work during season t, and X_{it} is a set of individual controls. Reassuringly, the signs on the controls square well with what one might expect. More educated adults are less likely to demand NREGA work. There is less demand for NREGA work during the monsoon and winter seasons, and adults with salaried jobs are much less likely to report wanting to work for the NREGA. Interestingly, there appears to be little difference in demand between men and women. The coefficients on $Migrant_i$ and $Migrate_{it}$ are both economically and statistically significant. An individual who left the village in a given season is 15 percentage points more likely to report a willingness to work more for the NREGA.

We next document correlations between time spent outside the village and time spent working for the NREGA during summer 2009. Table 3.7 reports estimates from regressions of the form:

$$DaysOutside_i = \beta_0 + \beta_1 WorkedNREGA_i + \beta_2 NREGADays_i + \gamma X_i + \epsilon_{it} \tag{3.7}$$

where $DaysOutside_i$ is the number of days individual i spent outside the village for work during summer 2009, $WorkedNREGA_i$ is a dummy variable equal to one if individual i worked for the NREGA during summer 2009, $NREGADays_i$ is the number of days that individual i worked for the NREGA during summer 2009, and X_i are controls. Across specifications, we observe a robust negative correlation between days worked for the NREGA and days spent outside the village, with the coefficient on $NREGADays_i$ indicating that a one day increase in days of NREGA work corresponds to a fall of between 0.19 to 0.27

days of work outside the village. It is worth noting that if every adult who worked for the NREGA and either migrated or said they would have migrated had they not worked on the NREGA reduced their time spent outside the village one for one with NREGA work, then we would expect that an increase of an additional day of NREGA work would correspond with a fall in days spent outside of the village of roughly 0.20.

Estimates from Equation 3.7 only pick up the correlation between work outside the village and NREGA work. For example, we would expect a negative correlation in the case in which the NREGA has no impact on migration, and some people choose to migrate and the people who stay back in the village choose to work on the NREGA because there is nothing else to do. If this is the case, we would expect migrants to not want to work for the NREGA, when in fact, as we showed previously, we see that migrants are more likely than non-migrants to report wanting to work for the NREGA. The third column of Table 3.7 explicitly includes controls for demand for NREGA work with little impact on the estimated coefficient on days of NREGA actually worked.

Still, this does not rule out that the rationing rule used by village officials is such that, conditional on demand, individuals who received NREGA work are less likely to migrate. In the fourth column of Table 3.7 we add controls for whether the respondent left the village for work during monsoon 2009 or winter 2009 and days spent outside the village. Many migrants leave the village during winter and there is little NREGA work available at this time. To the extent that the negative correlation between NREGA work and days outside the village is driven by migrants who want NREGA work being less likely to receive work, adding controls for migration in other seasons and especially during winter should decrease the coefficient on NREGA work. The coefficient on days of NREGA work drops to 0.190, suggesting this may be part of the story though not all of it.

3.6 Cross-State Comparisons

As detailed in Chapter Two, the sample villages were selected in part because they were located at the intersection of the three states of Rajasthan, Madhya Pradesh, and Gujarat. Chapter Two documented the substantial differences in implementation of the NREGA across the border, with Rajasthan providing more days of work even conditional on demand. In this section, we compare days worked on the NREGA and days spent outside the village in Rajasthan compared with the other two states Gujarat and Madhya Pradesh. We estimate regressions of the form

$$Y_i = Raj_i + X_i + \epsilon i \tag{3.8}$$

where Y_i is an outcome for adult i, Raj_i is a dummy variable equal to one if the adult lives in Rajasthan and X_i are controls. The vector X_i includes controls for gender, education, age, and a dummy variable for whether the adult reported being willing to work more for the NREGA.

We begin with the days worked for the NREGA as the outcome and consider each season separately. The first column of Table 3.8 presents the results from regressing days worked for the NREGA during summer 2009 on just a dummy variable for whether the adult lives in Rajasthan. The coefficient on the Rajasthan dummy suggests that on average, adults in Rajasthan worked almost nine additional days or more than twice the number of days of the average adult in the MP and Gujarat villages. The second column adds the controls with little impact on the estimated coefficient. Columns Three through Six repeat the same analysis using NREGA days worked during winter and monsoon. We see little difference across states in days worked during these seasons mainly because days worked is close to zero everywhere.

Table 3.9 repeats the same analysis with days spent outside the village for work as the

dependent variable. Estimates from the first two columns suggest that during summer 2009, adults in the Rajasthan villages spent almost seven fewer days on average working outside the village. Assuming that the villages in Gujarat and Madhya Pradesh provide a valid counterfactual for the village in Rajasthan, these estimates suggest that one day of additional NREGA work reduces migration by approximately 0.75 days. As detailed in Chapter Two, however, there are many important differences among adults living in Rajasthan, Madhya Pradesh and Gujarat. As a result, these differences in migration could be entirely due to preexisting differences among the states unrelated to the NREGA.

As a test, Columns Three through Four of Table 3.9 present the results using days worked outside the village during monsoon and winter as the dependent variable. To the extent that the difference in migration during summer 2009 is driven by differences in migration unrelated to the NREGA, we might expect that these differences would also appear during the monsoon and winter when there is no differential in NREGA work across the states. In fact, the estimates in Columns Three through Six show that the differences across states during these seasons are much smaller and statistically insignificant.

These results, taken together with the results from the previous section, suggest that government work is an attractive alternative to migration for the adults in our sample and that the NREGA has had a significant impact on migration to date. This is perhaps surprising given that the wage per day of work outside the village is roughly twice that for NREGA workers suggesting that the costs of migration are high. We next investigate this issue formally.

3.7 Variable Migration Costs Estimates

Table 3.10 presents the earnings per day of work outside the village and per day spent outside the village for migrants as well as earnings per day worked for the NREGA. The construction of these variables is described in detail in Section 3.4.3. The first column restricts the sample

to individuals who worked for the NREGA and migrated during summer 2009. The second column extends the sample to all adults who left the village during summer 2009. The third column and fourth column split the sample of migrants into those who report wanting more NREGA work and those who report not wanting more NREGA work. The differential between daily earnings outside the village and NREGA earnings is over 40% higher for migrants who do not want NREGA work.

Next, we estimate the distribution of variable migration costs using the framework set out in Section 3.3.3. Table 3.11 presents the results. The mean migration cost per day away is 77.7 rupees which is 75% of the average daily earnings per day away from the village. The standard deviation of migration costs is 40 rupees, though this should be interpreted with caution since any measurement error in NREGA earnings or earnings per day away from the village will increase this estimated standard deviation.

3.8 Discussion

The previous analysis suggests that a substantial fraction of adults either chose NREGA work over short-term migration or would have done so if more NREGA work were available. Because short-term migrants are not firmly attached to urban labor markets, their decision to migrate is easily influenced by rural (or urban) anti-poverty programs. In the case of a rural workfare program, which provides only a short period of relatively high wage work, short-term migrants can easily stay back in the village for a few more days and migrate later.

Even in an area with severe seasonality in locally available work, a seasonal workfare program still has a significant impact on private sector work. In pure income terms, it appears that workers may actually be sacrificing income to work for the program. Given the large estimated migration costs, this is not necessarily an undesirable outcome. If reducing migration is not a policy goal, however, cash transfers or credit subsidies might be preferable anti-poverty policies to workfare programs.

3.A Construction of Key Variables

Earnings per Day Worked for Migrants The survey instrument included questions about the frequency of payment and the typical amount per pay period. In most cases (74%), respondents were paid daily and in these cases we used the typical daily payment as earnings per day worked. We also asked respondents how many days per week they typically worked. Respondents worked on average six days per week and the median respondent worked six days. For respondents who were paid weekly, fortnightly, or monthly, we used the reported payment adjusted by the typical number of days per week worked. For example, a migrant paid 800 rupees weekly and working six days per week earns $800/6 = 133$ rupees per day worked. For migrants that were paid irregularly or in one lump sum at the end of work, we used the total earnings from the trip divided by the number of days worked. For migrants with missing values of days worked per week, we assumed they worked six days.

Surveyors were instructed to check whether daily earnings, total earnings, trip length, and days worked per week made sense together. If they did not, they were instructed to ask the respondent for an explanation and write it down. For example, in one case, total earnings from a trip was abnormally high because the respondent was paid for work performed on a different trip. In cases in which the surveyor comments indicated that the reported variables did not accurately measure the earnings per day worked of the respondent, we either adjusted the daily earnings or set the daily earnings to missing.

Finally, five percent of respondents received payment in-kind for their work, being paid in wheat for example. We leave these daily earnings observations as missing.

Earnings per Day Away for Migrants For respondents with non-missing total earnings (62%), earnings per day away was computed using total earnings divided by days away. For respondents with missing total earnings, we used earnings per day worked adjusted upwards

using days worked per week away.

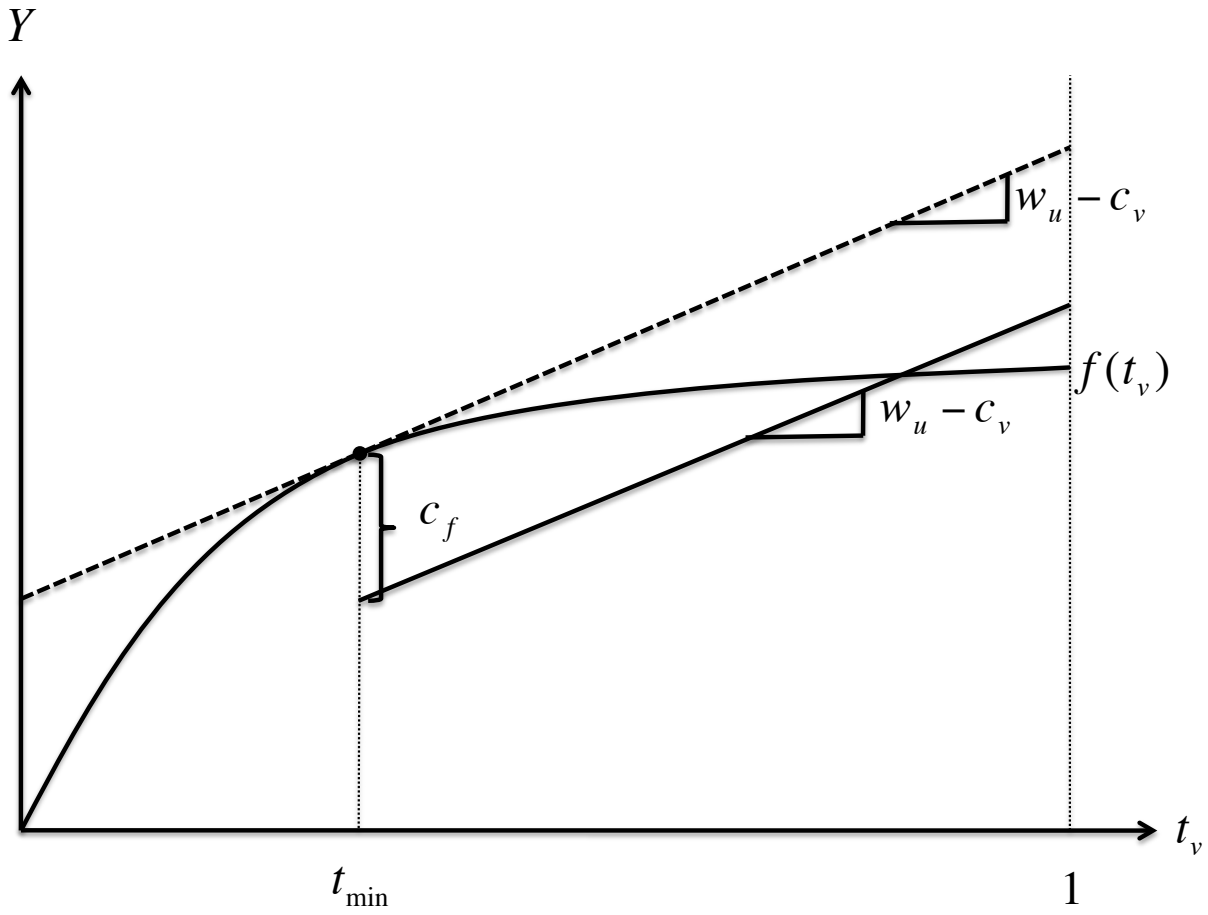

Figure 3.1: Current Migrant

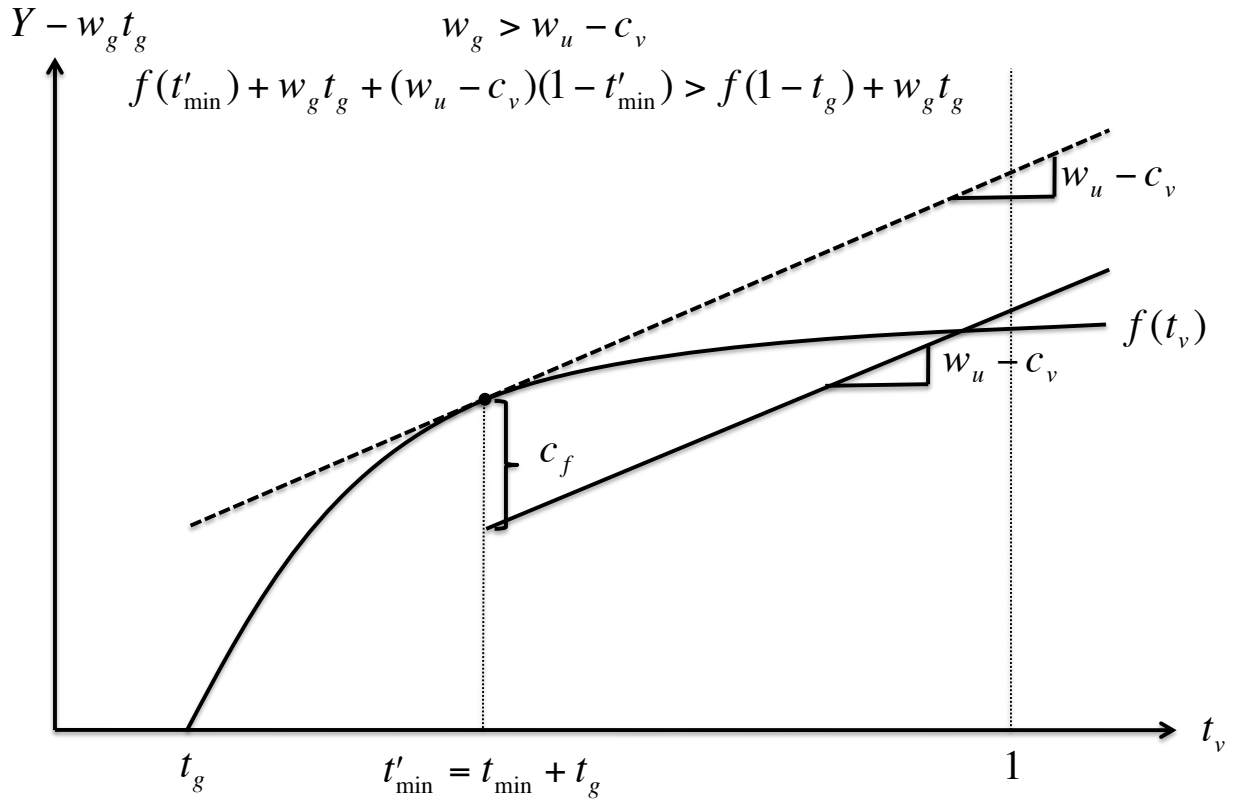

Figure 3.2: Introduce Government Employment

Table 3.1: Selection of Adult Sample

	Own Survey				NSS Survey 2007-08
	All Adults	Full Adult Survey Completed	Adult Survey not Completed	Difference (3) - (2)	All Adults
	(1)	(2)	(3)	(4)	(5)
Female	0.511	0.525	0.448	-0.077	0.497
	(0.0056)	(0.0166)	(0.0067)	(0.019)	(0.001)
Married	0.704	0.729	0.594	-0.134	0.692
	(0.0091)	(0.021)	(0.0105)	(0.0233)	(0.0017)
Illiterate	0.666	0.683	0.590	-0.093	0.385
	(0.0185)	(0.0325)	(0.0189)	(0.0302)	(0.0028)
Primary School	0.248	0.238	0.291	0.053	0.439
	(0.0105)	(0.0247)	(0.0118)	(0.0272)	(0.0024)
Secondary Education	0.118	0.112	0.143	0.030	0.097
	(0.0126)	(0.0223)	(0.0121)	(0.0186)	(0.0012)
Tertiary Education	0.016	0.016	0.014	-0.002	0.080
	(0.0042)	(0.0061)	(0.0049)	(0.0079)	(0.0015)
Age	32.8	34.1	27.0	-7.11	34.4
	(0.248)	(0.484)	(0.301)	(0.592)	(0.0452)
Spent 2-330 days away for work	0.464	0.422	0.649	0.226	--
	(0.0194)	(0.0383)	(0.0187)	(0.0385)	
Migrated for Work all Three Seasons	0.139	0.080	0.402	0.322	--
	(0.0135)	(0.0437)	(0.0101)	(0.0447)	
Ever Worked for NREGA	0.528	0.581	0.291	-0.290	--
	(0.0253)	(0.0354)	(0.0259)	(0.0332)	
Spent 30-180 days away for work	0.313	0.311	0.321	0.010	0.025
	(0.0158)	(0.0331)	(0.0165)	(0.034)	(0.0009)
Adults	2,722	2,224	498		252,711

The unit of observation is an adult. Standard errors computed assuming correlation of errors at the village level in parentheses. The first four columns present means based on subsets of the adults aged 14 to 69 from the main data set discussed in the paper. The first column includes the full sample of persons aged 14 to 69 for whom the adult survey was attempted. The second column includes all persons aged 14 to 69 for which the full adult survey was completed. The third column includes all persons aged 14 to 69 for which the full adult survey was not completed. The fourth column presents the difference between the third and second columns. The fifth column presents means computed using all adults aged 14 to 69 in the rural sample of the NSS Employment and Unemployment survey Round 64 conducted between July 2007 and June 2008. Means from the NSS survey are constructed using sampling weights. "--" denotes not available.

Table 3.2: Summary Statistics by Subgroup

	Migrants	NREGA Participants	Did Not Want NREGA Work	Full Sample
	(1)	(2)	(3)	(4)
Adult-level Variables				
Female	0.436	0.560	0.508	0.525
Married	0.732	0.788	0.594	0.729
Age	28.6	36.2	35.9	34.1
Illiterate	0.660	0.758	0.551	0.683
Primary School	0.288	0.209	0.252	0.238
Secondary Education or Above	0.126	0.084	0.225	0.129
Speaks Gujarati	0.232	0.092	0.262	0.211
Speaks Hindi	0.356	0.335	0.437	0.346
Household-level Variables				
Owns Cultivable Land	0.998	0.996	0.997	0.998
Cultivable Land (Acres)	2.91	3.05	3.39	3.03
Has Irrigated Land	0.489	0.575	0.542	0.538
Has Well for Irrigation or Drinking	0.462	0.524	0.489	0.495
Has Cell-phone	0.387	0.358	0.421	0.385
Has Electricity	0.290	0.298	0.350	0.318
Has Television	0.005	0.014	0.015	0.018
Dirt Floor	0.919	0.911	0.885	0.900
Household Head is Hindu	0.841	0.880	0.830	0.849
Household Head is Adivasi	0.093	0.065	0.108	0.089
Number of Adults	5.04	4.79	5.06	4.96
Number of Children	3.13	2.94	2.75	2.95
Adults	1,125	1,133	325	2,224

The unit of observation is an adult. Each cell is a sample average. The first column is restricted to adults who left the village for work during the past year (summer 2009, monsoon 2009 or winter 2009-10). The second column is restricted to adults who participated in NREGA during the past year. The third column is restricted to adults who reported not wanting NREGA work during the entire past year. The fourth column includes all adults who completed the full adult survey.

Table 3.3: Reasons for Not Working for NREGA or Not Wanting NREGA Work

	Summer 2009 (1)	Monsoon 2009 (2)	Winter 2009-10 (3)	Summer 2010 (4)
Panel A: Full Sample				
Worked for NREGA	0.402	0.004	0.064	0.367
NREGA Days Worked	11.2	0.106	1.13	9.34
NREGA Days Worked Conditional on Working	28.1	26.1	18.0	25.5
Want More NREGA Work	0.800	0.537	0.752	0.804
Total Days of NREGA Work Desired	43.9	21.5	42.7	44.8
Adults	2,224	2,224	2,224	2,224
Panel B: Subsample of Adults Who Want More Work				
Why Did You Not Work More?				
Family Worked Maximum 100 days	0.036	0.003	0.007	0.028
Works Finished/No Work Available	0.556	0.817	0.745	0.517
No Program ID Card/Name Not on ID Card	0.035	0.044	0.036	0.037
Officials Would not Provide More Work	0.058	0.009	0.033	0.067
Other	0.306	0.203	0.226	0.349
Adults	1,779	1,194	1,673	1,789
Panel C: Subsample of Adults Who Do Not Want More Work				
Why Did You Not Want to Work More?				
Working Outside the Village	0.171	0.047	0.123	0.170
Other Work in Village	0.126	0.669	0.245	0.154
Sick/injured/unable to work	0.101	0.045	0.087	0.106
Studying	0.236	0.169	0.307	0.205
NREGA Does Not Pay Enough	0.043	0.014	0.038	0.044
No Need for Work/Do Not Want to Do Manual Work	0.036	0.015	0.022	0.037
Other	0.436	0.152	0.334	0.478
Adults	445	1,030	551	435

The unit of observation is an adult. Each column restricts the sample to responses for a particular season. Panel A includes all adults who completed the adult survey. Panel B restricts the sample to adults who report wanting to work more for the NREGA during the season specified in the column heading. Panel C restricts the same to adults who report not wanting to work more for the NREGA during the season specified in the column heading.

Table 3.4: Wage Summary Statistics

	Summer 2009 (1)	Monsoon 2009 (2)	Winter 2009-10 (3)	Summer 2010 (4)
(1) Adults Who Migrated	768	218	646	927
(2) Adults with Non-missing Earnings	593	175	594	859
(3) Mean Earnings per Day Worked	118.1	126.8	123.4	123.9
(4) Mean Earnings per Day Away	101.9	107.3	109.0	111.2
(5) Adults with Predicted Earnings	968			
(6) Mean Predicted Earnings per Day Away	102.4			
(7) Adults Who Worked for NREGA	895	9	143	817
(8) Adults Who Had Received Full Payment at Survey Time	863	9	126	447
(9) Adults with Non-missing Earnings	816	9	135	592
(10) Mean Earnings per Day of *Paid* Worked	67.0	83.0	74.6	70.3
(11) Mean Earnings per Day Worked	64.4			
(12) Adults with Predicted NREGA Earnings	2,224			
(13) Mean Predicted NREGA Earnings	63.4			

The unit of observation is an adult. See the text and appendix for details on construction of the earnings measures. Each column restricts the sample to responses for a particular season.

Table 3.5: Migration and NREGA Work

	Summer 2009 (1)	Monsoon 2009 (2)	Winter 2009-10 (3)	Summer 2010 (4)
Panel A: Full Sample				
(1) Worked for NREGA	0.402	0.004	0.064	0.367
(2) Migrated	0.345	0.098	0.290	0.417
(3) Worked for NREGA and Migrated	0.120	0.000	0.017	0.115
(4) Would Have Migrated If No NREGA Work	0.084	0.002	0.017	0.065
(5) Migrated and Would Work More for NREGA	0.302	0.071	0.254	0.358
(6) Days Outside Village for Work	20.5	4.87	15.2	24.7
(7) Adults	2,224	2,224	2,224	2,224
Panel B: Subsample of Adults Who Migrated				
(8) Worked for NREGA	0.348	0.000	0.057	0.276
(9) Would work more for NREGA	0.875	0.725	0.873	0.860
(10) Days worked on NREGA	7.962	0.000	1.053	5.867
(11) Adults	768	218	646	927
Panel C: Subsample of Adults Who Migrated or Would Have Migrated				
(12) Worked for NREGA	0.475	0.022	0.108	0.374
(13) Would work more for NREGA	0.897	0.731	0.880	0.875
(14) Days worked on NREGA	12.6	0.498	2.05	9.13
(15) Adults	955	223	683	1,072
Panel D: Subsample of Adults Who Worked for the NREGA				
(16) Would Have Migrated if No NREGA Work	0.209	0.556	0.259	0.177
(17) Migrated	0.298	0.000	0.259	0.313
(18) Days Outside Village for Work	15.3	0.000	9.97	16.0
(19) Adults	895	9	143	817

The unit of observation is an adult. Each column restricts the sample to a particular season. Panel B restricts the sample to adults who left the village for work during the season specificed in the column heading. Panel C restricts the sample to adults who left the village for work or report that they would have left the village for work if that had not worked for the NREGA during the season specified in the column heading. Panel D restricts the sample to adults who worked for the NREGA during the season specificed in the column heading.

Table 3.6: Correlates of NREGA Demand

	Want NREGA Work (1)	Want NREGA Work (2)	Want NREGA Work (3)
Female	-0.0275*	0.0000369	0.00216
	(0.0161)	(0.0165)	(0.0165)
Primary School or Literate	-0.0335	-0.0237	-0.0237
	(0.0215)	(0.0212)	(0.0212)
Secondary or Above	-0.171***	-0.154***	-0.151***
	(0.0295)	(0.0285)	(0.0283)
Monsoon 2009	-0.263***	-0.263***	-0.250***
	(0.0183)	(0.0183)	(0.0169)
Winter 2009-10	-0.0477***	-0.0477***	-0.0447***
	(0.00703)	(0.00703)	(0.00668)
Age	0.0350***	0.0347***	0.0343***
	(0.00348)	(0.00330)	(0.00330)
Age Squared	-0.000481***	-0.000452***	-0.000448***
	(0.0000446)	(0.0000425)	(0.0000425)
Salaried Job	-0.330***	-0.297***	-0.296***
	(0.0653)	(0.0640)	(0.0639)
Migrant (Any Season)		0.121***	0.0961***
		(0.0193)	(0.0216)
Migrated (Current Season)			0.0542**
			(0.0234)
Constant	0.326***	0.217***	0.214***
	(0.0674)	(0.0653)	(0.0651)
Observations	6,669	6,669	6,669

The unit of observation is an adult by season. Standard errors computed assuming correlation of errors within villages. The dependent variable is a dummy variable for whether the individual reports willingness to work more days for the NREGA during a given season if work were available. ***, ** and * indicate significance at the 1, 5 and 10 percent level.

Table 3.7: Days Spent Outside Village Summer 2009

	Days Outside Village (1)	Days Outside Village (2)	Days Outside Village (3)	Days Outside Village (4)
Worked for NREGA (summer 2009)	-8.085***	-0.678	-0.476	1.977
	(1.909)	(2.735)	(2.805)	(2.150)
NREGA Days Worked (summer 2009)		-0.267***	-0.270***	-0.190***
		(0.0675)	(0.0694)	(0.0520)
Want More NREGA Work (summer 2009)			-1.368	-1.727
			(2.432)	(1.543)
NREGA Days Wanted (summer 2009)			0.137***	0.0785***
			(0.0370)	(0.0249)
Migrated Monsoon 2009				12.14**
				(5.336)
Days Migrated Monsoon 2009				0.107
				(0.0799)
Migrated Winter 2009				8.413**
				(3.247)
Days Migrated Winter 2009				0.423***
				(0.0522)
Education and Demographic Controls	Yes	Yes	Yes	Yes
Observations	2,217	2,198	2,188	2,186

The unit of observation is an adult. Each column presents results from a regression of days spent outside the village during summer 2009 on a set of explanatory variables. All regressions include a dummy for gender, whether the adult has primary school or is literate, a dummy for whether the adult has secondary schooling or above, age, age squared, and a constant. Cluster standard errors are computed assuming correlation of errors within villages. Observations differ across columns due to missing explanatory variables. ***, ** and * indicate significance at the 1, 5 and 10 percent level.

Table 3.8: Cross-state Comparison Days Spent Working for NREGA

	Summer 2009		Monsoon 2009		Winter 2009-10	
	NREGA Days (1)	NREGA Days (2)	NREGA Days (3)	NREGA Days (4)	NREGA Days (5)	NREGA Days (6)
Rajasthan	8.874***	9.029***	0.157	0.151	-0.340	-0.358
	(1.200)	(1.150)	(0.135)	(0.135)	(0.325)	(0.322)
Female		2.906***		-0.126		0.417**
		(0.812)		(0.103)		(0.183)
Primary Education		0.482		0.0649		-0.290
		(0.830)		(0.0491)		(0.274)
Secondary or Above		1.585		-0.136		-0.407
		(1.287)		(0.0826)		(0.355)
Age		1.081***		0.00939		0.0800*
		(0.167)		(0.0156)		(0.0404)
Age-Squared		-0.0110***		-0.000158		-0.00103**
		(0.00200)		(0.000208)		(0.000517)
Want More NREGA Work		10.58***		0.0647		0.942***
		(1.013)		(0.0624)		(0.207)
Mean Outcome for Gujarat and MP	6.631	6.631	0.0250	0.0250	1.310	1.310
	(0.875)	(0.875)	(0.0195)	(0.0195)	(0.254)	(0.254)
Observations	2,205	2,204	2,224	2,223	2,221	2,220

The unit of observation is an adult. Each column presents results from a regression of days spent working on the NREGA during a particular season on a set of explanatory variables. Rajasthan is a dummy for whether the adult lives within a village in Rajasthan. Standard errors are computed assuming correlation of errors within villages. Observations differ across columns due to missing dependent and explanatory variables. All regressions include a constant. ***, ** and * indicate significance at the 1, 5 and 10 percent level.

Table 3.9: Cross-state Comparison Days Spent Working Outside the Village

	Summer 2009		Monsoon 2009		Winter 2009-10	
	Days Outside Village (1)	Days Outside Village (2)	Days Outside Village (3)	Days Outside Village (4)	Days Outside Village (5)	Days Outside Village (6)
Rajasthan	-6.717**	-7.330**	-0.506	-0.637	-2.163	-2.623
	(3.086)	(2.828)	(1.296)	(1.295)	(2.230)	(2.144)
Female		-13.33***		-4.449***		-8.472***
		(1.415)		(0.651)		(1.010)
Primary Education		-6.487**		-1.000		-2.741
		(2.471)		(1.324)		(1.937)
Secondary or Above		-15.92***		-3.762**		-8.723***
		(3.300)		(1.633)		(3.002)
Age		0.745***		0.0788		0.473*
		(0.250)		(0.120)		(0.258)
Age-Squared		-0.0184***		-0.00348**		-0.0124***
		(0.00329)		(0.00169)		(0.00348)
Want More NREGA Work		3.002		-1.734		2.032
		(2.399)		(1.975)		(2.360)
Mean Outcome for Gujarat and MP	23.93	23.93	5.128	5.128	16.35	16.35
	(2.302)	(2.302)	(0.981)	(0.981)	(1.597)	(1.597)
Observations	2,218	2,217	2,221	2,220	2,222	2,221

The unit of observation is an adult. Each column presents results from a regression of days spent working outside the village during a particular season on a set of explanatory variables. Rajasthan is a dummy for whether the adult lives within a village in Rajasthan. Standard errors are computed assuming correlation of errors within villages. Observations differ across columns due to missing dependent and explanatory variables. ***, ** and * indicate significance at the 1, 5 and 10 percent level.

Table 3.10: Wage Differentials (Summer 2009)

	Migrated and Worked for NREGA	Migrated	Migrated and Want More NREGA Work	Migrated and Do not Want More NREGA Work
	(1)	(2)	(3)	(4)
(1) Earnings per Day Outside Village	99.9	102.5	100.5	116.4
	(3.42)	(2.55)	(2.41)	(7.82)
(2) Earnings per Day Worked Outside Village	116.6	119.6	117.3	135.8
	(3.7)	(2.75)	(2.61)	(8.45)
(3) Earnings per Day of NREGA Work	64.1	63.2	63.1	63.4
	(1.97)	(0.93)	(0.96)	(1.26)
(4) Difference (1) - (3)	35.8	39.3	37.4	52.9
	(3.62)	(2.48)	(2.42)	(7.68)
Observations	251	718	627	91

The unit of observation is an adult. The first row presents the mean earnings per day outside the village during summer 2009 for different subsets of all migrants. For adults with missing earnings, earnings from migration trips taken during summer 2010 are used to predict earnings in summer 2009. The second row presents the mean of earnings per day worked for NREGA during summer 2009. For adults who did not work for NREGA or have missing earnings, earnings are predicted using summer 2010 NREGA earnings and a set of person-level characteristics. Standard errors computed assuming correlation of errors within villages in parentheses.

Table 3.11: Migration Cost Estimates

	Parameter Estimate
(1) Mean Migration Cost	77.7 [74.2, 82.0]
(2) Standard Deviation of Migration Costs	39.6 [34.2, 43.9]
(3) Mean Earnings per Day Outside Village	102.5
(4) Observations	718

The unit of observation is an adult. The first and second rows present estimates of the mean and standard deviation of the distribution of migration costs per day spent outside the village for the sample of adults who left the village during summer 2009. Confidence intervals are computed by bootstrapping assuming errors are correlated within villages.

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