Why Guarantee Employment? Evidence from a Large Indian Public-Works Program

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Abstract

Public-works programs in developing countries have recently attracted a lot of renewed attention as anti-poverty government initiatives. In this paper, I analyze the labor-market impacts of the largest public-works program in the world, the Indian National Rural Employment Guarantee Scheme (NREGS). The scheme provides a legal guarantee of 100 days of public-sector employment per year to all rural households, and allows workers to decide if and when to take up the program. In a household time-allocation model, I first show that this flexibility allows households to use the program both as an alternative form of employment and as a safety net after bad economic shocks. Empirically, I reconstruct the algorithm the government used to assign districts to implementation phases and then use a regression-discontinuity design to estimate the program effects. The results suggest that the overall labor-market impacts of NREGS on employment and casual wages are small, but that take-up is higher after bad rainfall shocks. These empirical patterns are consistent with NREGS functioning as a safety net, but not with the program providing a general alternative form of employment. The availability of a safety net affects household time-allocation decisions even in the absence of a shock, however, with men moving out of the private casual sector and into alternative occupations like self-employment. These empirical patterns imply that the overall low take-up of the program does not mean that it is ineffective in altering the situation of the poor.

JEL: H53, I38, J22, J23, J38

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1 Introduction

Public-works programs are popular policy tools to help households cope with economic shocks in countries around the world. But while the interest in such schemes has declined heavily in many developed countries, recent years have seen a resurgence of such initiatives in developing countries[1]: the World Bank, for example, funded public-works programs in 24 countries between 2007 and 2009. In contrast to earlier schemes, many of the recent programs emphasize more long-term anti-poverty and safety net goals rather than viewing jobs in government projects simply as a way of reducing temporary unemployment. Public-works programs now often work as a predictable safety net that households know they have access to when they experience a negative economic shock, but can also provide an additional source of income for underemployed workers unable to find a job. Both of these functions have the potential to reduce poverty by ensuring a larger and less variable stream of income for the poor.

How well public-works programs can fulfill such anti-poverty goals in practice is still a debated question, however, and as of now there is little empirical evidence on the causal labor-market impacts of public-works programs in developing countries.[2] Estimating the impacts of public-works programs on labor-market outcomes is often challenging because many programs are rolled out non-randomly and without a comparable control group, making a causal analysis difficult. The experience from developed countries suggests that such government initiatives often prove unable to raise workers' human capital and are in danger of crowding out private-sector jobs.[3] Additionally, concerns about the implementation quality of government initiatives due to problems with corruption and rationing potentially limit the economic benefits for workers in developing countries.

This paper analyzes the labor-market impacts of the largest public-works program in the

[1] See e.g. Lal et al. 2010. For an overview of public-works programs in developing countries see e.g. Zimmermann (forthcoming). Subbarao et al. (2013) provide an extensive account of public-works programs and recent developments in middle- and low-income countries.

[2] See e.g. Basu (forthcoming), Besley and Coate (1992), Datt and Ravallion (1994) for some examples of theoretical and empirical analyses. Most of the existing empirical literature on the topic lacks a credible causal identification strategy, however.

[3] For an overview of public-works programs in developed countries see e.g. Kluve (2010). In developing countries, public-works program could have lower private-sector impacts if demand for temporary public employment tends to be high at times when there are few other jobs available (Subbarao 1997).

world, one of the few instances where the program effects can be rigorously evaluated. India's National Rural Employment Guarantee Scheme (NREGS) provides a legal guarantee of up to 100 days of public-sector employment annually for each rural household (about 70 percent of the Indian population) that can be taken up at any point during the year. This feature makes NREGS one of the most ambitious anti-poverty schemes in developing countries, which is also reflected in high annual expenditures on the scheme of typically around 1 percent of Indian GDP. The program aims to work as an additional source of income for underemployed workers in rural labor markets and as a safety net for the rural poor after bad economic shocks.

NREGS was phased in across India in three steps between 2006 and 2008 in a highly non-random manner that prioritized economically underdeveloped districts. This feature makes the use of empirical strategies like a difference-in-difference approach unattractive since the parallel trend assumption can be shown to be violated. Instead, I rely on a regression-discontinuity design to estimate the causal impact of the employment guarantee scheme on labor-market outcomes. To carry out this research design, I uncover the algorithm the government used to assign districts to treatment phases and reconstruct the algorithm values that can then be used as a running variable.

To provide some intuition for the expected empirical impacts of NREGS, I set up a household time-allocation model: households choose to allocate their time between a private-sector job and self-employment, where the latter is assumed to be the generally preferred but riskier occupation. Once NREGS is introduced, it is allowed to function both as a third form of employment and as a safety net for self-employed households after a bad economic shock. The model implies, among a number of other testable predictions, that the safety-net function of the employment guarantee scheme affects a household's optimal time allocation even when no negative shock occurs. The availability of a safety net makes self-employment a less risky occupation than before, which indirectly subsidizes such activities and reduces the need for households to work in the safer casual private sector.

The empirical results support the model's predictions about NREGS functioning as a safety net, whereas there is little evidence for the program providing an alternative form of employment. NREGS has very limited overall labor market impacts for both men and women: the program does not lead to a substantial increase in public-sector or total employment and does not raise private-sector casual wages. Consistent with the safety-net function, there is evidence that take-up of the program increases after bad rainfall shocks, and that men leave the private sector even in the absence of a negative shock. Despite this evidence that NREGS provides some insurance to rural households, I do not find any large positive impacts on other outcomes of interest such as household expenditures, however.

Overall, my paper suggests that we need a more comprehensive understanding of household options and optimal behavior to correctly evaluate the labor-market impacts of public-works programs than is often done in the existing literature and policy debate. One of the main components that make recent public-works programs in developing countries different from earlier initiatives is that they are often conceptualized as long-standing schemes rather than as short-term interventions. This means that households know that they will have access to public employment after bad economic shocks even before the shock actually occurs, and can therefore re-optimize their time allocation to reflect this reduction in risk. These indirect effects of the program may have substantial welfare implications that are typically not captured in the debate on the net benefits of public-works programs, and may mean that programs are not ineffective in altering the living situation of the poor despite low actual take-up. In the case of India, the employment guarantee scheme potentially indirectly subsidizes self-employment, which may have large long-term impacts for rural households.

My paper therefore extends the general literature on public-works programs in developing countries. The empirical results are consistent with some existing research that finds that the insurance function of such government schemes often seems to dominate the direct income benefits, although this literature in general does not consider the time-allocation impacts that should arise even in the absence of a shock (see e.g. Dev 1995, Subbarao 1997, Subbarao et al. 2013). Some other papers document changes in time allocation very similar to the results I find in this paper, but do not link these patterns to a broader conceptual framework or to the safety-net function of public-works programs (see e.g. Berhane et al. 2011, Gilligan et al. 2008). In general, the existing literature is dominated by propensity-score matching and difference-in-difference estimators and has traditionally focused very heavily on targeting and take-up of public-works programs rather than on their broader labor-market impacts.

My paper also contributes to the active literature on the Indian employment guarantee scheme in two respects: the existing recent papers on the labor-market impacts of the program use difference-in-difference approaches and concentrate on showing that there are important heterogeneous treatment effects with respect to variables such as seasonality and implementation quality (Azam 2012, Berg et al. 2012, and Imbert and Papp 2013). Given the non-random rollout of the scheme, the often substantial effects reported in these papers could be due to the violation of the parallel trend assumption, however.[4] My regression-discontinuity analysis, which does not require the parallel trend assumption, clarifies that the overall direct impacts of NREGS seem to be small, although it confirms the conclusion of the other papers that the overall sample masks important heterogeneous treatment effects. Additionally, I show both theoretically and empirically that it is important to analyze substitution effects between different forms of non-public employment to fully capture the labor-market impacts of the employment guarantee scheme, which is not done in the existing literature. This also complements existing research on how rural labor markets in India are affected by unanticipated productivity shocks as in Jayachandran (2006).

The rest of this paper is structured as follows: section 2 provides some background information on the characteristics of NREGS. Section 3 sets up a theoretical model of a household's time optimization problem that generates a number of predictions about the labor market impacts of NREGS. Section 4 describes the rollout of the program and how it can be used in a regression discontinuity framework, while section 5 discusses the data and the empirical specifications. Section 6 presents the main results and some extensions. Section 7 concludes.

[4]Appendix figure C.17 plots the trends in private employment for the baseline data, for example, and shows that the parallel trend assumption does not hold.

2 Background

2.1 Program Characteristics

The National Rural Employment Guarantee Scheme (NREGS) is one of the most ambitious government development programs in the world.[5] It is based on the National Rural Employment Guarantee Act (NREGA) that legally guarantees each rural household up to 100 days of public-sector work a year at the minimum wage. There are no formal eligibility rules other than that the household lives in a rural area and that their members are prepared to do manual work at the minimum wage. Households can apply for work at any time of the year, and men and women are paid equally. At least one third of the NREGS workforce in a village is required to be female.

NREGS projects are supposed to advance local development primarily through drought-proofing, flood prevention and irrigation measures, and need to be carried out without the help of contractors or machines. Paid wages are the state minimum wage for agricultural laborers, although NREGA specifies a floor minimum wage.[6] At the introduction of the scheme, this floor wage was 60 rupees per day, but it has been raised over time. In most states wages are paid on a piece-rate basis where the rates are supposed to be adjusted such that a typical worker working for 8 hours will earn the minimum wage. Wages must be paid within 15 days of the day the work was performed, and are supposed to be given out on a weekly basis.

2.2 Implementation and Effectiveness of the Program

How well the ambitious features of NREGS work in reality has been of large interest to researchers, NGOs and the press right from the beginning of the scheme. Qualitative and descriptive research suggests that NREGS is implemented well enough to generate substantial

[5]The program was renamed the Mahatma Gandhi National Rural Employment Guarantee Scheme in 2009. The original name is still widely used especially in the academic literature on the program, however. For more details on the scheme see e.g. Dey et al. (2006), Government of India (2009), and Ministry of Rural Development (2010).

[6]In practice, most states have minimum wages that are higher than the national floor wage, so that the NREGS wage is state-specific.

benefits for the poor, for example during the agricultural off-season and after idiosyncratic shocks, and has improved women's access to jobs with reasonable wages and working conditions. At the same time, however, such studies also stress widespread practical limitations and violations of the provisions in the National Rural Employment Guarantee Act: muster rolls are often faulty and include ghost workers, wages are often paid with long delays and may not conform to the state minimum wage. Additionally, many local governments seem to lack the technical expertise to propose useful local projects. Big landowners have also repeatedly complained about labor shortage and demanded NREGS work be banned during the peak agricultural season (Centre for Science and Environment 2008, Institute of Applied Manpower 2007, Khera 2009, Khera and Nayak 2009, NCAER-PIF 2009, Samarthan Centre for Development Support 2007).

Varying levels of NREGS implementation quality are also documented in a number of economics papers that typically focus on individual Indian states: Johnson (2009a) looks at the impact of rainfall shocks on the take-up of NREGS in the Indian state Andhra Pradesh, and finds that participation in public-works projects increases when rainfall is lower than expected, so that NREGS seems to provide a safety net for rural households. Deininger and Liu (2013) find that NREGS increases nutritional intake and household assets in the same state, whereas the analysis in Johnson (2009b) shows that the working of NREGS in Andhra Pradesh does not seem to be strongly affected by the parties in power at the local level.

But while these papers suggest that NREGS works well in Andhra Pradesh, other research documents that this is not the case in all parts of India: Niehaus and Sukhtankar (2013, forthcoming) analyze the existence and characteristics of corruption in the implementation of NREGS in the Indian state Orissa, and find that an increase in the minimum wage was not passed through to workers. Dutta et al. (2012) use nationally representative data from 2009/10 to study the effectiveness of reaching the target population. They find that demand for NREGS often far outstrips supply and that the rationing of projects is especially common in poorer states. Despite these shortcomings, Klonner and Oldiges (2012) find some evidence of substantial reductions in poverty using a difference-in-difference approach. NREGS has also been credited with reducing rural-urban migration and improving children's education outcomes (Afridi et al. 2012, Ravi et al. 2012).

Some existing economics papers also analyze the impact of the program on rural labor markets. Imbert and Papp (2013) use a difference-in-difference approach to look at the program's impact on wages and employment, comparing early-NREGS districts to the districts that had not yet received the program in 2007/08 and therefore function as control districts. They find that NREGS increases employment by 0.3 days per prime-aged adult and private-sector wages by 4.5 percent, with the impacts concentrated during the agricultural off-season. Azam (2012) also uses a difference-in-difference approach, and finds that public-sector employment increases by 2.5 percent while wages for males and females increase by 1 and 8 percent, respectively. In a variation of the difference-in-difference design, Berg et al. (2012) analyze the impact of NREGS on agricultural wages by using monthly information on agricultural wages from 2000 to 2011. The results in the paper suggest that agricultural wages have increased by about 5 percent in districts with a high implementation quality, but that it takes between 6 and 11 months after program roll-out for these wage effects to be realized.

The difference-in-difference strategy requires these papers to make the parallel-trend assumption that labor market outcome trends would have been similar in early and late NREGS districts in the absence of the program. Given the non-random rollout of the program according to poverty criteria this is a strong assumption, however, which could substantially affect their results.[7] Appendix figure C.17 shows, for example, that the parallel trend assumption is violated for private casual employment in the baseline data, and the assumption also fails for other labor-market outcomes. The regression discontinuity approach used in this paper, on the other hand, does not require such an assumption and therefore provides a cleaner empirical identification of the impacts of NREGS.

Additionally, these papers do not consider potential substitution effects between various categories of non-public employment, which could arise if the introduction of NREGS induces

[7] Imbert and Papp (2013) discuss, for example, that wages in treatment and control districts were on different trends prior to the introduction of NREGS. They attempt to address potential concerns about the internal validity of their difference-in-difference estimates by including extensive district-level controls.

households to re-optimize their time-allocation decisions. To have a clearer understanding of the overall expected empirical impacts of the scheme, it is useful to set up a simple theoretical model of a household's optimization problem.

3 A Model of the Household Optimization Problem

3.1 The Baseline Model without NREGS

The model describes a household's optimal time allocation in a one-period setting.[8] Before NREGS is introduced, a household can first choose to allocate the total time of their household members, T, between working for a big landowner as agricultural laborer in the private casual sector, l, and working on the family farm, f.[9] After this decision has been made, a weather shock is realized which determines the payoff from farm work.[10] The period ends, and the household earns the fixed wage w in the private sector, and income y for the time spent in farming.[11] The household derives utility both from the time spent working in self-employment on the family farm, and from the total income earned in both activities during the period.[12] The utility function is additively separable in these components, with weight

[8] A more detailed discussion of the model as well as proofs for the model predictions are provided in the appendix.

[9] Implicit in this setup is the assumption that a household has perfect control over l or, put differently, that the household can always get a job in the private sector at wage w for the desired duration. One period in this framework is thought of as an agricultural year, which includes peak times like planting and harvesting. While the views about the structure of Indian rural labor markets differ substantially (see e.g. Kaur 2011 and Basu 2002), theoretical papers like Basu (2002) assume that landlords hire agricultural laborers competitively during the harvesting season.

[10] To fix ideas, the shock in this model is referred to as a weather shock. The model can accommodate all types of shocks that make self-employment more uncertain than private-sector employment, including health or other idiosyncratic shocks. If anything, the model's simplifying assumption that wages are fixed is more likely to hold in such cases. In the NSS data used for my empirical analysis, most households own some land. 53 percent of men self-identify as engaging in family employment as the main occupation, and about 40 percent of men live in households that are self-employed in agriculture.

[11] The fixed-wage assumption is consistent with the cross-sectional relationship between private wages and rainfall for rainfall shocks up to 5 standard deviations at baseline in the data. The analysis controls for mean and standard deviation of rainfall in a district. For rainfall shocks that are larger than 5 standard deviations, the wage is increasing in the rainfall shock. Assuming that the private-sector wage is constant is a simplifying assumption. All that is needed for the model predictions to go through is that private-sector employment is less risky relative to self-employment.

[12] The assumption that households derive utility from working in self-employment ensures that the optimization problem has an interior solution. The qualitative predictions of the model are not affected by relaxing this assumption, however. Bandiera et al. (2013) show that less poor workers are more likely to be self-employed than the poorest, and that an intervention that relaxes credit constraints and improves skills

α given to the utility from self-employment. The probability density function of y is *g(y)*.

At the beginning of the period, a household's optimization problem is

$$\max_l \alpha v(T-l) + (1-\alpha)E[u((T-l)y + lw)]$$

with $u' > 0, u'' < 0$, $v' > 0, v'' < 0$. This leads to the first-order condition

$$\alpha v'(T-l) = (1-\alpha)\int u'((T-l)y + lw)(w-y)g(y)dy \tag{1}$$

(1) pins down the optimal proportion of time l spent working in the private sector implicitly. Intuitively, the expected marginal utility from being self-employed needs to equal the expected marginal utility from working in the private sector.

An interesting extension is how the optimal proportion of time spent in private employment changes with the variability of the weather-shock distribution. As I show in the appendix, assuming that the distribution of y in district B is a mean-preserving spread of the distribution in district A, it can be shown that households spend more in time in casual private employment in the riskier district B than in less risky district A, and therefore substitute away from the risky activity farming.

3.2 The Model with NREGS

After NREGS is introduced, the program can be used both as an alternative source of employment regardless of the weather shock, and as an insurance tool after bad weather shocks. This alters the baseline model in two ways. The household now first makes a time-allocation decision among three alternatives: working in the private casual sector (l), working on the family farm (f_1), and taking up a NREGS job (n_1). After this decision has been made, as before a weather shock is realized that affects the payoff from farm work. The time originally allocated to farm work,f_1, can then be split between actually working on the farm (f_2) and

for the very poor leads to substantial increases in the self-employment rate. Banerjee et al. (2011) report similar results. These findings are consistent with a general preference for self-employment, which is also in line with anecdotal evidence from developing countries.

taking up public employment in a NREGS project (n_2) instead. After this decision, the period ends and the payoffs are realized. As before, the payoff from farm employment is y and the private-sector wage is w. The NREGS program wage is $\overline{w}$. The household again derives utility from the time spent in self-employment and from the total income earned.

The new household optimization problem at the beginning of the period is now given by

$$\max_{l,n_1} E[\alpha v(T-l-n_1-n_2^*)+(1-\alpha)u((T-l-n_1-n_2^*)y+n_2^*\overline{w}+lw+n_1\overline{w})]$$

Where n_2^* is the best-response function of n_2 given y since the household can optimize the time spent working for NREGS and actually working on the family farm after the weather shock has occurred and y has been realized. Once a household chooses the fraction of time to spend on NREGS employment after the weather shock has occurred, l, n_1, and y are fixed. The household therefore chooses n_2 to maximize

$$\max_{n_2} \alpha v(T-l-n_1-n_2)+(1-\alpha)u((T-l-n_1-n_2)y+n_2\overline{w}+lw+n_1\overline{w})$$

Leading to the first-order condition

$$\alpha v'(T-l-n_1-n_2)=(1-\alpha)u'((T-l-n_1-n_2)y+n_2\overline{w}+lw+n_1\overline{w})(\overline{w}-y) \qquad (2)$$

Define the shock y_0 as the shock at which the first-order condition implies n_2^*=0. Then the first-order condition traces out the best-response function n_2^* for all weather shocks that imply a farming income of y_0 or less. For all larger values of y, the optimal n_2 is zero.

Knowing n_2^* and the distribution of y, at the beginning of the period the household needs to decide how much time to spend in the private sector, in NREGS employment, and in anticipated farming. A household will never work in both private-sector work l and in ex-ante NREGS employment n_1, but will work in the job that pays more. This is because l and n_1 are perfect substitutes for a household in terms of their contribution to household utility. Both are safe sources of employment that need to be committed to before the weather shock is realized. A household therefore maximizes utility by choosing the alternative that pays a higher wage. Define j as the amount of time spent working in the activity that pays the higher wage, such that

$$j = \begin{cases} n_1 & w \leq \overline{w} \\ l & w > \overline{w} \end{cases}$$

And define $\widetilde{w}$ analogously as the corresponding wage.

Working in the fact that the optimal n_2 is zero at large positive shocks, the first order condition of the household maximization problem is

$$\frac{\alpha}{1-\alpha}\left[\int_{y \leq y_0} v'(T-j-n_2^*)(1+\frac{\partial n_2^*}{\partial j})g(y)dy + v'(T-j)\right] - \int_{y>y_0} u'((T-j)y+j\widetilde{w})(\widetilde{w}-y)g(y)dy$$

$$= \int_{y \leq y_0} u'((T-j-n_2^*)y+n_2^*\overline{w}+j\widetilde{w})(\widetilde{w}-y+(\overline{w}-y)\frac{\partial n_2^*}{\partial j})g(y)dy \quad (3)$$

It can be shown that a sufficient condition for the existence of a solution is that the Arrow-Pratt measure of absolute risk aversion is high 'enough'.[13]

A couple of predictions about the impact of NREGS follow from the model setup under reasonable assumptions and are derived in the appendix. The appendix also discusses the impact of a couple of extensions on the model, including the NREGS cap of 100 days, implementation problems, and private-sector wage variability.

1. If NREGS is predominantly used as a new form of employment regardless of the shock, NREGS employment rises, private-sector employment falls and the impact on farm employment is ambiguous.

2. If NREGS is predominantly used as a safety net after negative shocks, then the program

[13]See appendix for the proof. This condition does not depend on the sign of $\frac{\partial n_2^*}{\partial j}$, which is ambiguous. Intuitively, how the time allocated to the ex-post NREGS employment responds to an increase in the time allocated to precautionary activity j depends on the attractiveness of the wage for j relative to the NREGS wage $\overline{w}$ and y. In other words, j only functions well as a precautionary savings tool if the paid wage in that activity is not too low relative to the payoffs that can be achieved through NREGS employment and farming after the weather shock is realized. A sufficient condition for j and n_2^* being substitutes for shocks $y \leq y_0$ is $\widetilde{w} \geq \overline{w}$.

has two effects

- Ex post effect: NREGS employment is higher after a negative shock.
- Ex ante effect: if no negative shock occurs, NREGS employment is low overall. Private employment decreases and farm employment increases.

Intuitively, the first set of predictions arises because NREGS introduces a more attractive form of safe employment that can be used as a risk-mitigation measure, which directly crowds out private-sector work. Since private-sector and NREGS employment are perfect substitutes in the model, this effect requires that the NREGS wage is higher than the private wage. The impact on farm employment is theoretically ambiguous: a higher wage in the safe form of employment makes working there more attractive, but households can now accumulate the same amount of money from safe employment as before in less time. The new optimal time-allocation pattern therefore depends on the magnitude of $\overline{w}$ relative to y and on the household's degree of risk aversion. The larger the implementation problems of the program are, for example in terms of rationing or underpayment of wages, the less likely can NREGS function as a new general form of employment.

The second set of predictions follows from the fact that NREGS as a safety net tool makes self-employment less risky than before since it can be taken up after bad shocks. This reduces the need for a household to insure against adverse shocks by working in the private sector. Households therefore spend less time in private employment and more time being self-employed. NREGS take-up will be low unless a bad shock is actually realized.

Both of these channels can potentially interact as well: in riskier districts, the probability of a large negative shock occuring is higher than in less risky districts. Knowing this, households in risky districts will decrease their time spent in safe employment less than households in less risky districts if the NREGS cap of 100 days of employment and any implementation problems mean that insurance is incomplete after large negative shocks. At the same time, however, households in riskier districts may also have a higher demand for NREGS as a safe form of employment to mitigate the higher risk they face from self-employment in general than they would in a less risky district.

3.3 Wage Impacts of NREGS

The model assumes that the private-sector wage is fixed and does not change in response to workers spending less time in the private sector to work on their own farms. This is clearly a simplifying assumption. How the private-sector wage changes after the introduction of NREGS depends on the industry structure of local labor markets and on the composition of the workforce, but there is little consensus in the existing literature about the best way of modelling the Indian casual private sector.[14] In a standard perfectly competitive setup where employers pay workers their marginal product and the marginal product is decreasing in the number of workers employed, for example, a decrease in the supply of labor because of NREGS will lead to a higher marginal product of labor for the remaining workers and therefore to higher wages, which in turn attenuates the negative impact NREGS has on private-sector employment. Wages should also rise if the public-works program practically enforces the existing minimum wage laws.

Wages could also fall under certain conditions, although such a scenario in general requires much more detailed assumptions about local structures and the shape of the production function. Suppose, for example, that each worker gets paid their marginal product, but that the marginal product is independent of the number of workers employed. There is heterogeneity in terms of a worker's productivity, with higher-productivity workers deriving more utility from self-employment (a higher α in terms of the model). NREGS will then make farming more attractive for high-productivity workers than for lower-productivity workers, which changes the composition of the workforce to consist of a higher percentage of low-productivity workers than before. Since a worker's marginal product is independent of the number of workers employed, wages for a worker of a given productivity will remain unchanged. Due to the change in the composition of the workforce, the average wage paid in the private sector will fall, however.

[14] The models in Basu (2002) and Basu (forthcoming), for example, are built on the existence of two types of workers: those with long-run contracts, and those with short-run contracts. While the papers cite some evidence of the existence of such long-run contracts in some parts of India, other papers like Kaur (2012) argue that daily labor contracts are the norm in Indian rural labor markets. Imbert and Papp (2013) focus heavily on small farmers with simultaneous labor supply and demand decisions.

The impact NREGS has on private-sector wages is therefore an empirical question.

4 Program Rollout and Regression Discontinuity Design

4.1 Program Timeline and Details of the Rollout

The National Rural Employment Guarantee Act (NREGA) was passed in the Indian Parliament in August 2005. NREGS came into force in February 2006 in the first 200 districts. The scheme was then extended to the rest of the country in two steps: an additional 130 districts received the program in April 2007, and all remaining rural districts started NREGS in April 2008 (Ministry of Rural Development 2010). I will refer to the district phases as Phase 1, Phase 2, and Phase 3, respectively.

This phasing in of the employment guarantee scheme allows the empirical analysis of the program's labor market impacts by using a regression discontinuity (RD) design since the government assigned districts to implementation phases based on an algorithm. Unfortunately, the criteria used in the algorithm are not explicitly explained in the official documents on the program and the algorithm values are not directly publicly available. To be able to construct the running variable required for the regression-discontinuity design I therefore uncover and reconstruct the government algorithm. I do this by combining information from a number of government documents on NREGS, earlier development programs and other government reports. The algorithm had been used to determine the treatment status of earlier programs, and its use in the case of NREGS is confirmed by a former member of the Indian Planning Commission.[15]

Treatment assignment for each implementation phase was made according to a two-step algorithm: In the first step, the number of eligible districts was allocated to states according to the proportion of India's poor living in a given state. In the second step, districts within states were then supposed to be chosen based on an existing development ranking of districts,

[15] More detailed information on the algorithm can be found in the appendix.

with poor districts receiving the program first.

While the algorithm values themselves are not available directly, knowledge of the procedure allows their reconstruction. The development index values used in the second step of the algorithm are publicly available from a Planning Commission report (Planning Commission 2003). The exact headcount poverty ratio values used in the first step are not known with complete certainty since three new states were created after the data used to calculate the values was collected. The poverty measure therefore needed to be adjusted. The Planning Commission published such revised values in 2009 (Planning Commission 2009), so those values are likely to be very close to the values used in the NREGS treatment assignment decisions, and are therefore used in this paper.

Table 1 provides an overview of how well the algorithm predicts NREGS receipt in the first and second phase for 17 major Indian states for all districts with non-missing development rank information.[16] The first column provides the number of non-missing rank districts per state, whereas columns 2 and 3 show the actual number of NREGS treatment districts for each state in Phase 1 and Phase 2, respectively. Columns 4 and 5 give the success rate of the proposed algorithm in predicting the treatment status of districts in Phases 1 and 2. The prediction success rate is calculated as the percent of treated districts of a given phase where actual and predicted treatment status are the same.

Table 1 shows that the overall prediction success rate of the proposed algorithm is about 84 percent in Phase 1 and about 82 percent in Phase 2, so there is some slippage in treatment assignment in both phases.[17] The prediction success rates are considerably higher than the ones that would be expected from a random assignment of districts, which are 40.27 percent for Phase 1 and 37.45 percent for Phase 2, respectively. The table also reveals that there is

[16] Rank data is available for 447 of 618 districts in India. Data for the index creation was unavailable in some states, in most cases because of internal stability and security issues during the early 1990s when most of the data was collected. A former member of the Planning Commission says that in these states state governments may have had considerable say in district allocation, so in the absence of a general rule treatment status in these states is likely to be endogenous. I therefore exclude these states from my analysis. Rank data in the 17 major Indian states is complete for all districts classified as rural by the Planning Commission in their report, so there is no endogeneity in the availability of data in these states. Urban districts in the Planning Commission report are districts that either include the state capital or that have an urban agglomeration of more than one million people.

[17] Prediction success rates for Phase 2 are calculated after dropping Phase 1 districts from the analysis.

considerable heterogeneity in the performance of the algorithm across states, but that the algorithm performs well in almost all of the 17 states.[18] Potential threats to internal validity are discussed in the next section.

4.2 Regression Discontinuity Design

Given the treatment algorithm's two-step procedure, the generated cutoffs that can be used for a regression discontinuity (RD) analysis are state-specific. Two cutoffs can be empirically identified: the cutoff between Phase 1 and Phase 2, corresponding to Phase 1 treatment assignment, and the cutoff between Phase 2 and Phase 3, which is equivalent to the Phase 2 rollout of the program. Since the dataset that I will be using in my empirical analysis was collected at a time when NREGS had been rolled out to Phase 1 and Phase 2 districts, but not yet to Phase 3 districts, only the cutoff between Phase 2 and Phase 3 can be used to analyze the impact of the government program. I therefore focus on this cutoff in the remainder of this paper.

Treatment cutoffs differ by state, so for the empirical analysis ranks are made state-specific and are re-centered so that a district with a normalized state-specific rank of zero is the last district in a state to be eligible for receiving the program in Phase 2. The data are then pooled and the treatment effects are estimated at the common discontinuity. Negative numbers are assigned to districts with lower ranks than the cutoff rank, whereas positive numbers are assigned to the districts that are too developed to be eligible and that will function as control districts.

Figure 1 shows the number of observations at each state-specific rank for Phase 2 district assignment. It reveals that all 17 states used in the analysis have at least one district receiving NREGS in Phase 2, but that only few states have districts further away from the 0 cutoff.[19]

[18] As for the general sample, at the state level the relationship between predicted and actual treatment is usually much tighter than the one that would be predicted by random assignment of districts. The main exception to this are the Phase 2 assignments for the states Bihar, Jharkhand and West Bengal, since all remaining districts in those states are treated in Phase 2. In this case, random and algorithm-based assignment therefore yield the same results.

[19] While this pattern mostly reflects that there are only few states with a large number of districts, a number of states are also fully treated after Phase 2 assignment so that they have no Phase 3 districts and therefore no positive-rank districts in Figure 1. The results in this paper are robust to dropping fully-treated

Figure 1 reports observations based on the predicted NREGS receipt of the algorithm. As Table 1 shows, however, actual program receipt does not completely follow this assignment. Therefore, the empirical identification strategy is a fuzzy RD design. The fundamental assumption of the RD design is that districts that were just poor enough to receive the program, and districts that were just too rich to be included are similar to each other in terms of unobserved characteristics, so that outcome differences are solely attributable to the introduction of the employment guarantee scheme.

In order for the RD design to be valid, districts must have imperfect control over their treatment status in a given phase (Lee 2008). This implies that states and districts should not have been able to manipulate either the index variable used to rank districts, or the quotas allocated to states.[20] Otherwise, districts close to the cutoff on either side are not plausibly similar to each other in terms of unobserved characteristics, but differ on characteristics such as perceived benefit from the program or political influence.

That states or districts were able to manipulate the poverty index seems unlikely. First, the index was constructed based on somewhat dated available information: the Planning Commission used data from the early to mid-1990s for the ranking of districts, rather than collecting current information from districts. This limits the possibility to strategically misreport information. Second, the ranking had originally been used to target earlier development programs for especially poor districts, although with lower cutoffs of 100 and 150 districts, which implied lower state-specific cutoffs as well. So if districts were able to act strategically, the incentive would have been to be among the 150 poorest districts, but not among the 200 poorest districts used for NREGS in the first phase, and certainly not among the 330 poorest districts that received NREGS in either Phase 1 or Phase 2. Third, the creation of the index from the raw data by the Planning Commission is done in a transparent way. The Planning Commission report outlines the exact procedure with which the index was created, and also

states from the analysis.

[20]The all-India number of treatment districts in each phase, 200 and 130, do not seem to have been chosen to accommodate state or district demands for a certain number of treatment districts. 200 was the number of districts the Planning Commission suggested for an earlier development program which never really took off. The number 130, on the other hand, seems to have been adopted because a number of states that had received many NREGS districts in the first phase had only few untreated districts left that could be treated in Phase 2.

lists the raw data for all districts, so that the composite index as well as the district ranking can be perfectly replicated.

Figures 2 and 3 look more closely at the distribution of index values over ranks. Figure 2 shows the relationship between the poverty index value and the assigned rank by the Planning Commission for all 447 districts for which data is available. Across India, the distribution of poverty index values is smooth and continuous across ranks. As the chosen cutoffs are state-specific, Figure 3 plots the relationship between the Planning Commission's index and the normalized state-specific ranks for the Phase 2 cutoff. For most states, the poverty index values seem pretty smooth at the cutoff of 0. Overall, these patterns suggest that manipulation of the underlying poverty index variable is not a serious concern.

Manipulation of the criterion used for the allocation of treatment districts across states also seems unlikely: The state headcount ratios are calculated from mid-1990s information that had long been available at the time of NREGS district assignment. Additionally, I use 2001 Census information on the states' rural population to calculate the poverty prevalence measures, which also was widespread publicly-available information at the time. Again, it was therefore probably impossible for Indian states or districts to exert political influence on the treatment status of individual districts by manipulating the data.[21]

Given that I do not have access to the actual poverty-prevalence measure used in the algorithm, my reconstructed values introduce measurement error into predicted treatment status if the Indian government used different values to make state allocations. While this potentially makes the regression discontinuity design fuzzier than it really is, it should not introduce systematic bias into the calculations since I am using the best available source of estimates.

Another way of analyzing whether manipulation is likely to be a problem is to test whether there are discontinuities at the cutoff in the baseline data: if the RD specification is valid,

[21] This does not mean, however, that actual treatment assignment was not subject to political pressures, since Table 1 reveals that compliance with the proposed algorithm is substantially lower than 100 percent. It can be shown that deviations from the algorithm are correlated with the party affiliation of members of parliament from the same district (Zimmermann 2013). This finding is in line with research like Gupta (2006) who analyzes the correlation of political party affiliation and treatment status in an earlier district development program. This program most likely also used the two-step algorithm proposed in this paper, however, which is not taken into account in Gupta's paper and could potentially affect the results in substantial ways.

we would expect baseline outcomes to be smooth at the cutoff if treatment and control districts near the cutoff are indeed similar on observables and unobservables in the absence of treatment. Table 2 reports the results of such tests for all the used labor-market outcome variables as well as for three other outcomes for which data is available at baseline (years of education, area of land owned and log per capita expenditure) for all parametric specifications of the RD design used in this paper. The estimates show that the large majority of the 64 coefficents are not statistically significant. The only variable for which a discontinuity at the cutoff is quite consistently found is the years of education variable: Both men and women in NREGS Phase 2 districts have fewer years of education than people in the control districts, although the magnitude of the effect is small. For women, two coefficients are significant among the outcome variables, but this pattern is not consistent across empirical specifications. Again, widespread manipulation of treatment assignment seems unlikely based on these results. To control for any baseline differences in outcomes as well as to soak up residual variance, the main results in this paper control for the baseline outcome variable, however, even though the estimated coefficients are not substantially affected by the exclusion of the baseline controls.

With the fuzzy RD design used in this paper, we need to verify that there is indeed a discontinuity in the probability of receiving NREGS at the cutoff values for Phase 2 NREGS districts. Figure 4 shows this graphically for the normalized state-specific cutoff for Phase 2. It plots the probability of receiving NREGS in the given phase for each bin, as well as fitted quadratic regression curves and corresponding 95 percent confidence intervals on either side of the cutoff. The graph shows that the average probability of receiving NREGS jumps down at the cutoff. This suggests that there is indeed a discontinuity in the probability of being treated. Figure 4 also shows that compliance with the algorithm is relatively low directly at the normalized cutoff of zero, which could for example be a function of measurement error in the first step of the algorithm. In a robustness check of my main results, I therefore drop observations right around the cutoff in an application of the donut hole RD approach.

5 Data and Empirical Specification

5.1 Data and Variable Creation

The data used in this paper comes from household surveys collected by the National Sample Survey (NSS) Organisation. These surveys are representative of the Indian population, and drawn from the population in a two-stage stratified sample design. In the first stage, villages are selected, and individual households within these villages are sampled in the second stage. The dataset that can be used to analyze the impact of NREGS on wages and employment is the 64th round of NSS data, which was collected from July 2007 to June 2008. It has a sample size of about 120,000 households and interviews were carried out over the course of a year in four sub-rounds, each spanning three months. By this time, NREGS had just been rolled out to Phase 2 districts in April 2007. Phase 3 districts received the program in April 2008, although general delays in implementation suggest that Phase 3 districts can be treated as control districts even for the last three months of the survey.[22] To analyze the labor market impacts of NREGS by using an RD design, I therefore focus on the state-specific cutoffs between Phase 2 and Phase 3 and drop Phase 1 districts.

The dataset collects wage and employment information as well as a number of socio-demographic characteristics. Additionally, a sample of households are interviewed in a given district in every sub-round, if possible. While the household data is strictly cross-sectional, this means that at the district level it is possible to generate a sub-round panel with up to four observations per round. I will exploit this feature of the data empirically by aggregating individual-level information up to the district level for each sub-round separately.

Consistent with other NREGS papers, I restrict my sample to individuals of prime age (18-60 years) who are living in rural areas and have at most secondary education. The NSS employment module asks detailed questions about an individual's work status in the last 7 days. I use these questions to create various employment and wage variables, focusing on casual jobs. Employment measures are dummy variables equal to 1 if an individual worked

[22]See e.g. Imbert and Papp (2013). The results reported in this paper are qualitatively the same when these potentially contaminated control group observations are excluded.

at all in a public-sector job, a private-sector job or in family employment in the past 7 days, respectively, and 0 otherwise. I add up the value of wages received in cash and kind for private-sector casual jobs and divide it by the amount of time spent in that type of work to create a daily private wage for workers. I then aggregate the labor market measures up to the district-sub-round level using sampling weights. Data from the 61st round (July 2004-June 2005) is used as baseline information.

5.2 Empirical Specification

The preferred way of estimating the treatment effect at the cutoff in an RD design is to restrict the sample to observations close to the cutoff and to then run separate local linear regressions on both sides (Lee and Lemieux 2010). The difference of the regression lines at the cutoff then provides the estimate of the treatment effect. In choosing which observations are 'close' to the cutoff, researchers need to trade off concerns about precision and bias: The larger the window of observations used in the regressions, the more precise the estimates are likely to be since the number of observations is higher. At the same time, however, this implies that observations further away from the cutoff are used, which may bias the estimate of the treatment effect at the cutoff.

This trade-off is of particular relevance in the case of NREGS where the number of districts is limited so that there are few districts 'close' to the cutoff. To get an idea of how bad the bias introduced by using observations further away from the cutoff is, appendix figures C11 to C16 non-parametrically plot the relationship between the running variable and three outcomes of interest for men and women separately. The graphs show the averaged outcomes of all district observations with a given state-specific rank and also include the estimated regression function for a quadratic function on both sides of the cutoff. The graphs show that a quadratic function fits the data quite well in all specifications, and that the estimated regression lines for public employment are even well approximated by a linear function. These patterns suggest that using the whole sample of Phase 2 and Phase 3 districts and estimating the treatment effect at the cutoff using linear and quadratic functions of the running variable is not a bad approximation of the underlying data. That a larger bandwidth may be plausible is also supported by Figure 3, which showed that the underlying poverty index is smooth at the cutoff. F-tests also reject the null hypothesis that higher-order polynomials add important flexibility to the model. More flexible models also tend to be unstable, although the estimated coefficients are often qualitatively similar to the quadratic results.

My overall preferred empirical specification therefore uses quadratic regression curves estimated on either side of the cutoff (referred to as 'quadratic flexible slope' in the result tables). As a robustness check, all my results also report the estimates using a quadratic function constrained to have the same slope on either side of the cutoff, and corresponding flexible and constrained linear regression lines. Additionally, I also report the estimates of the main results when using a linear flexible regression function, but restricting the sample to observations closer to the cutoff, in Table 7.

The equation below shows the regression equation for the most flexible specification:

$$y_{ijk} = \beta_0 + \beta_1 rank_{ij} + \beta_2 rank_{ij}^2 + \beta_3 nregs_{ij} + \beta_4 nregs * rank_{ij} + \beta_5 nregs * rank_{ij}^2 + \beta_6 baseline\ y_{ij} + \eta_j + \epsilon_{ijk}$$

where the subscripts refer to individual i in district j in season k, y is an outcome variable of interest, *rank* is a district's rank based on the state-specific normalized rank, and η are state fixed effects.

The main results report the intent-to-treat effect of NREGS, so *nregs* is an indicator variable equal to 1 if a district is predicted to have received NREGS in Phase 2 according to the state-specific algorithm, and zero otherwise. A corresponding appendix table reports the treatment-on-the-treated estimates where actual NREGS receipt is instrumented with predicted NREGS receipt. The coefficient of interest is β_3. In all empirical specifications, standard errors are clustered at the district level.[23] Results are reported for men and women separately.

[23] The results from reweighting observations by their 2001 Census population size are qualitatively very similar to these results and therefore not presented here. This extension takes into account that district-averages will be more precisely estimated in large districts than in small ones since the individual-level data is representative of the Indian population. At the same time, however, such a specification assumes that there is no district heterogeneity in treatment effects.

The above specification uses the commonly employed technique of re-centering the treatment cutoffs and pooling the data to estimate the treatment effect at a single cutoff. An alternative approach is a meta analysis used for example in Black et al. (2007): the treatment effect is estimated for each cutoff separately, and the estimates are then combined to a single estimate afterwards by using appropriate weights. I report the results for such an analysis in Table 8 for a simple average and a population-weighted average of the state treatment effects. These estimates also take into account that the covariance between the state-specific estimates may not be zero.

Lastly, there is the choice of the running variable. While the specification above uses the state-specific rank as the running variable, an alternative would be to use the poverty index instead. Treatment assignment was made according to the state-specific rank, however: the first step of the government algorithm determines the size of the treatment group in a given state, which is then filled with the poorest districts according to their rank. The relevant distance of a district from the cutoff is therefore its rank and not its index value, since in many cases a district could have a very different poverty index value without altering its rank or distance from the cutoff. Additionally, the plotted conditional mean function using the rank variable is flatter than the one using the index values, suggesting that a larger bandwidth is less problematic when using the rank variable.[24] I report the estimates of the main results using the state-specific index variable as a robustness check in appendix table C4.

5.3 Summary Statistics

Table 3 presents baseline wage and employment summary statistics for districts separately by phase for men and women respectively. As the table shows, early NREGS districts have lower baseline wages for men than later districts, consistent with the idea that NREGS was

[24] A potential worry with the rank variable is that, in contrast to the index variable, it is not a truly continuous variable. As an ordinal variable, while the rank values can be changed without altering the interpretation of the variable, by construction the distance between any two observations is always identical. This characteristic is not unlike the measurement error for many discrete variables. Dong (2013) suggests a simple method of adjusting the RD estimates to this problem, and the results in this paper are robust to using this method.

rolled out to poorer districts first. The daily wage of a typical male casual worker of prime age with at most secondary education in an average Phase 2 district is about 53 rupees, whereas the corresponding wage is about 66 rupees in Phase 3 districts. Private-sector daily wages are very similar to overall casual daily wages, and there is no substantial difference between public-sector and private-sector wages.

In general, however, it is very uncommon to work in the public sector in all districts: 0.4 percent of workers work in the public sector in a typical Phase 2 district in the week prior to the survey, and the corresponding number for Phase 3 districts is 0.2 percent. In contrast, in all districts about 30 percent of males work in private casual jobs, and about 58 percent work in a family business or on the family farm. The remainder are males who are unemployed or out of the labor force. Table 3 also shows that the situation for Indian women is qualitatively similar to that of men, but that women are about half as likely to work in casual jobs of any kind or in family employment as men.

6 Results

6.1 Main Results

Figures 5 to 10 and Tables 4 and 5 present the main results of the impact of NREGS for men and women separately. The figures show the RD design graphically for the probability of being employed in a public works program or in casual private-sector work in the past 7 days and for the log private-sector daily casual wage earned in the past week. One scatter point represents the average residual outcome value in a given season. The residuals come from a regression of the outcome variable on the baseline outcome variable and state fixed effects to make the graphs comparable to the results in the tables. The regression lines are quadratic in the running variable, with the slope allowed to differ between the two sides of the cutoff. The graphs also plot the 95 percent confidence intervals. With the exception of Figure 6, none of the figures show a statistically significant discontinuity at the cutoff value 0, suggesting that the impacts of NREGS on the Indian rural labor market are limited for districts near the state-specific cutoff. Figure 6, on the other hand, reveals that private employment for men in NREGS districts is statistically significantly lower than that in control districts.

Tables 4 and 5 focus on different empirical specifications of the RD design in more detail. Whereas these tables present the intent-to-treat effects, appendix table C1 shows the corresponding IV results for the main results. In all tables, one observation is a district in a specific season. Table 4 shows the main results for men and women in Panel A and Panel B, respectively. Each row presents the impact of NREGS on the outcome variables of interest for a different parametric functional form of the running variable. Panel A looks at the estimates for men and column 1 demonstrates again that NREGS does not have a large impact on public-sector casual employment: the typical estimate is positive but small in magnitude and statistically insignificant. The coefficient in the first row of column 1, for example, suggests that being in a NREGS district increases the average rural prime-aged man's probability of having had a public-works job in the last 7 days by 0.12 percentage points. This translates into an increase of 17.4 percent since mean public employment is only 0.69 percent, but the effect is statistically insignificant.

Column 2 of Panel A reveals that the NREGS impact on private casual employment is negative and statistically significant at the 10 percent level. The estimated coefficients suggest that NREGS lowers private-sector casual employment for men by about 3-5 percentage points across specifications, which translates into a percentage change of 11-16 percent. The impact of NREGS on the probability of being in family employment in column 3 is positive and of about the same absolute magnitude as the estimates in column 2, although imprecisely estimated. The overall impact of NREGS on total employment is negative but statistically insignificant.

Panel A also shows the results for the log daily private casual wage. The outcome variable in column 5 is the average district log wage earned, conditional on having earned a positive daily wage. Since column 2 provides some evidence of private employment changes, any wage impacts of NREGS in the conditional log wage should be seen as a potential combination of changes in the selection of workers into private employment and of wage changes of workers conditional on workforce composition. According to column 5, the impact of NREGS on private wages is small and statistically insignificantly different from zero. If anything, the results point to a decrease in the private-sector wage. The estimated coefficient in the first row of column 5, for example, suggests that the average log private wage for men employed in casual private-sector work decreased by 0.4 percent in treatment districts relative to control districts at the cutoff.

Panel B shows the corresponding results for women. As column 1 demonstrates, the impact of NREGS on the probability of being employed in a public-works project for women is typically positive and of a similar magnitude as the one for men, although the estimates are again small in magnitude and statistically insignificantly different from zero. Column 2 shows that the impact of NREGS on casual private-sector employment for women is negative and typically smaller than for men, although the confidence intervals are typically wide. In contrast to men, however, the total employment coefficients are positive, although they are again very imprecisely estimated. Additionally, Panel B suggests that NREGS has no large-scale effects on private-sector wages for casual work for women and a positive, but statistically insignificant impact on family employment.

These results show that the general impacts of NREGS on labor-market outcomes seem to be limited, although the coefficients are often imprecisely estimated. There is no statistically significant increase in public employment and the empirical analysis can rule out public-employment increases larger than one percentage point. The employment guarantee has also not led to upward pressure on the private-sector wage. If anything, private-sector wages fall, which rules out that NREGS enforces existing minimum wage laws or increases competition in local labor markets that forces employers to substantially raise the private-sector wage. In contrast, there is some evidence of male workers leaving the private casual sector. Taking the imprecise estimates on public, family, and total employment at face value, the analysis suggests that most men switch from private to family employment, which would be consistent with the ex ante effect of NREGS functioning as a safety net: The availability of NREGS as a safety net after bad economic shocks would then lower the relative riskiness of family employment and therefore lead men to leave the private casual sector even when no shock occurs. On the other hand, the estimates provide no support for the idea that NREGS is predominantly taken up as a new alternative form of employment. Overall, Table 4 therefore implies that NREGS has not had large labor-market impacts. At best there is some role for NREGS as a safety net.

Whether the employment guarantee scheme can function as a safety net after bad economic shocks can also be tested more directly. If this is true, we should see an increase in the take-up of public employment after a negative shock, which was the ex post effect in the model. Table 5 reports the results of such an analysis: the specification focuses on districts during the agricultural off-season (January to June), but considers rainfall shocks that occurred at the beginning of the previous agricultural season in the months July to September, which roughly corresponds to the monsoon season. This gives the rainfall shock some time to feed through to household incomes. The main treatment variable is interacted with an indicator variable equal to one if a district experienced a negative rainfall shock (so lower rainfall than expected based on average rainfall in the district) in the agricultural main season.

As column 1 of Panel A shows, NREGS take-up for men is indeed statistically significantly higher after such an adverse shock, with interaction effects of around 3 percentage points. The sum of the main effect on NREGS and the interaction effect with the negative shock is also always statistically significantly different from zero at conventional levels, implying that that the NREGS impact in bad rainfall shock areas is also statistically significantly different from zero. This higher take-up of the employment guarantee after bad rainfall shocks confirms the take-up effects found in Johnson (2009a) for Andhra Pradesh. The magnitude of the effect is similar for women, as reported in Panel B, but imprecisely estimated. Again, there is little evidence of large employment or wage impacts, however: the employment guarantee scheme does not lead to a net increase in employment in NREGS districts even after a bad rainfall shock. Taken at face value, the statistically significant increase in public employment after a bad rainfall shock for men in treatment districts comes at the cost of private-sector employment rather than providing work to unemployed workers, although the coefficients on private employment are estimated imprecisely.

Taken together, Tables 4 and 5 therefore support the idea that there are no large benefits for workers from the introduction of NREGS, but that the safety net feature of the program plays some role. As appendix table C2 shows for the male sample, there is also no evidence that the employment guarantee scheme has had a large impact on per-capita expenditures, the total wage or remittances received in the past year. In results not reported here, I also find no effect of the program on the variance of household expenditures.

The theoretical model also implied that we should expect heterogeneous treatment effects depending on the riskiness of a district's farming income distribution, and that safety net and employment functions of NREGS may interact in those districts. If NREGS does indeed mostly work through providing a safety net after bad economic shocks, we should see that these impacts are especially pronounced in higher-risk districts. According to the model, public employment should increase more in high-risk districts because they are more likely to have experienced a bad economic shock, and private-sector employment may or may not decrease more strongly in high-risk districts depending on the magnitude of negative income shocks relative to the maximum amount of income that can be earned from NREGS, with family employment mirroring the private-sector employment effects. Additionally, public employment in higher-risk districts may also increase more because of a higher demand for safe employment. Since households know that they live in a more risky area, the demand for a buffer stock may be higher, especially if NREGS cannot provide full insurance after bad economic shocks and if there is rationing in NREGS or private-sector employment so that the buffer-stock demand for work cannot be met by just working in the higher-paying alternative as in the model. So households may demand NREGS employment even in the absence of a negative shock in addition to private-sector work.

Table 6 reports the empirical results that analyze the importance of risk heterogeneity. The table shows the estimates for men where NREGS treatment is interacted with an indicator variable equal to 1 if a district has a higher than median variance in rainfall at baseline, which proxies for income volatility. The results for women are qualitatively similar to those for men, but the coefficients are typically smaller and not statistically significantly different from zero. To make the estimates comparable to the ones in Table 5, the sample is again restricted to the agricultural off-season observations, although the results are very similar when using the full sample. The regressions also control for expected rainfall. Column 1 of Panel A shows that public employment is statistically significantly higher in high-variance districts. The interaction effect for private employment is typically positive, although not statistically significant, and the family employment results mirror these effects. Additionally, the private-sector wage in column 5 is substantially higher in risky districts, although only statistically significant at the 5 percent level in the complete sample and not in the off-season sample reported in Table 6. Total employment is statistically significantly higher in riskier districts, on the other hand, which is consistent with the idea that there is a higher demand for employment in these districts. The sum of the main effect and the interaction effect for the public and total employment outcomes is not statistically significant at conventional levels. This means that while the impact of the employment guarantee is larger for these outcome variables in riskier districts than in less-risky districts, the impact of the program in risky areas is not statistically significantly different from zero.

To analyze whether the increase in public employment is due to a higher demand for NREGS as a safety net or as an alternative form of employment in riskier districts, in a specification not reported in this paper I re-run the public-employment regressions from Table 6 for men, but control for the last monsoon rainfall. This specification at least to some extent controls for the demand for insurance after adverse shocks. Public employment is still statistically significantly higher in riskier NREGS districts than in low-risk treatment districts when controlling for monsoon rainfall, and the magnitudes are very similar to the estimated coefficients when last-season rainfall is excluded. This suggests that the public-employment effects in Table 6 to a large degree reflect risk-mitigation motives. Overall, the magnitude of public-employment changes is still small relative to the private- and family-employment coefficents, however, potentially implying that the insurance mechanism is more important.

Tables 4 to 6 therefore suggest that the overall impacts of NREGS are small, but that there are instances where the program works as a safety net and, in risky districts, as an alternative form of employment.

6.2 Robustness Checks

A couple of alternative specifications can be used to test how robust the presented main results are. One check is to change the sample restrictions: the main results keep all Phase 2 and Phase 3 districts in the analysis, which potentially biases the estimates since observations far away from the treatment cutoff can influence the estimate of the treatment effect at the cutoff. Table 7 therefore reports the analogous results to Table 4 for a linear flexible specification and three more restrictive definitions of the sample to observations with state-specific ranks between -5 and 5, -4 and 4, and -3 and 3, respectively. As the results show, the qualitative pattern of the results from Table 4 persists, although, consistent with the tradeoff between precision and bias, the coefficients tend to be more imprecisely estimated than before.

A second potential concern about the reported estimates is that they may be heavily affected by measurement error: since the exact numbers used to determine the number of treatment districts assigned to states are not known, my choice of the most plausible values introduces measurement error right around the state-specific cutoff values. To test how sensitive the estimates are to this, I re-estimate Table 4 without the districts right around the cutoff by excluding districts with a normalized rank between -1 and 1. This approach is typically referred to as the donut-hole approach.[25] The results of the donut hole approach are reported in appendix Table C3 and are very similar to those in Table 4.

A different way of checking the robustness of the main results is to use a meta-analysis approach to estimate the treatment effect at the cutoff. So far, the impact of NREGS was estimated by re-centering the state-specific cutoffs and then pooling the data to estimate the

[25] See e.g. Almond and Doyle (2011) for a similar application. Applying this approach has its disadvantages as well: First, the regression discontinuity design relies on estimating the treatment effect in the neighborhood of the cutoff, so dropping the observations closest to the cutoff weakens the fundamental assumption that districts close to the cutoff on either side are similar to each other in terms of all characteristics except the treatment status of NREGS. Second, dropping observations reduces the sample size.

overall treatment effect. An alternative method is to estimate the treatment effect separately for each state, and to then combine those estimates in a meta analysis. The results of this analysis are reported in Table 8 for men. The results for women tend to be qualitatively similar, but, as in the other tables, smaller and less precisely estimated. Since the number of observations for an individual state is often small and more flexible specifications are often highly collinear with the treatment variable, Table 8 only reports the results for two of the four specifications from Table 4. The first two rows report the results based on a simple average of the state-specific treatment effects, whereas the last two rows weight the state-specific treatment effects by the state population. The results in Table 8 show that the empirical patterns are robust to this alternative estimation technique.

Two additional robustness checks are provided in the appendix: table C4 re-estimates Table 4 using the state-specific index value instead of the rank as the running variable, whereas Table C5 estimates the impacts of NREGS at the individual rather than the district level.[26] Overall, results are again qualitatively similar to those reported in Table 4 and again suggest that the labor market impacts of NREGS are limited.

Lastly, the main results are also robust to a number of other specifications not reported here, like the exclusion of the baseline outcome variables, the inclusion of additional control variables and the exclusion of potentially contaminated Phase 3 districts due to the timing of data collection.[27]

6.3 Discussion

Overall, the results suggest that NREGS only has a very limited direct influence on the Indian rural labor market, although in a number of empirical specifications the effects are not precisely enough estimated to rule out more substantial effects. Instead, NREGS seems to work as an insurance tool that reduces the riskiness of family employment relative to

[26]The individual observations are weighted using sampling weights. Since the data at the individual level are cross-sectional, we cannot control for the baseline outcome variable in the same way as before. The regressions reported in Table C5 do not control for any baseline outcomes, but the results are robust to controlling for the baseline district average in the outcome.

[27]Phase 3 districts received NREGS in April 2008, whereas the data was collected between July 2007 and June 2008 and Phase 3 districts are treated as controls throughout in the main specifications.

private-sector work, even though the risk heterogeneity results suggest that buffer stock considerations are not completely absent, either.

The safety net does not seem to generate substantial welfare benefits in the form of higher per-capita expenditures, however. One potential explanation for this finding is that such effects may take longer to be realized. The analysis in this paper is limited to the first year of NREGS implementation because of data limitations and since the program is rolled out to control districts afterwards. Medium- to long-term benefits of NREGS can therefore not be captured. Even if there are no household expenditure impacts, however, the program may have substantial welfare implications through the occupational changes and may therefore alter the unobserved utility households derive from employment.

Maybe most surprising is the fact that a large-scale public-works program like NREGS does not seem to significantly increase the working-age population's probability of having held a public-works job in the past 7 days. Mean public employment is only 0.69 percent for men and 0.53 percent for women in Phase 2 and Phase 3 districts. So while some of the estimated coefficients are equivalent to large increases in public employment in percentage terms, statistical power is not big enough to precisely estimate such small effects. The estimates in Table 4 imply that the empirical analysis can rule out increases in public employment above 1 percentage point.

While the theoretical model suggests that we should not expect large increases in public employment if NREGS is mainly used as a safety net rather than as an additional form of employment in a typical year, one potential alternative explanation for these small effects is the time frame of the household survey. Since employment information is based on a 7-day recall window, it is by design much noisier than employment histories over a longer time horizon, although there should be no issues with recollection error. It is therefore useful to compare the prevalence of NREGS employment in the household survey data to the employment numbers based on administrative data. While some papers have documented that administrative records are exaggerating the effectiveness of NREGS due to corruption issues at least in some Indian states (see e.g. Niehaus and Sukhtankar 2013, forcoming), the administrative records should provide an upper bound on NREGS impacts.

According to administrative records, the employment guarantee scheme provided 1.4 billion person-days of employment in 1.78 million projects in the 330 Phase 1 and Phase 2 NREGS districts in 2007-2008.[28] 61.15 percent of this employment was given to women. The average daily wage paid was 75 rupees (about $1.8). This means that in a typical week, the scheme generated 83677 workdays of employment in 104 projects in the average district. With an average prime-aged district population of 1.10 million people, this translates into 0.0764 NREGS workdays per week per person. In the NSS data, the number of public-works workdays in Phase 1 and Phase 2 districts are 0.0789 for prime-aged adults, or about 4 days of public employment per person per year. This means that the NREGS employment generated for the chosen sample of prime-aged adults in this paper is in the same ballpark as that suggested by administrative sources, and is low at the local level: the implied weekly number of NREGS workdays per prime-aged adult in the average district would be 0.9615, for example, if we assume that 50 percent of workers have a NREGS job for 100 days per year. These back-of-the-envelope calculations therefore support the public employment results in this paper in that generated employment opportunities seem to be relatively modest at the local level.[29]

This conclusion runs counter to the results obtained in most of the difference-in-difference papers that analyze the impact of NREGS on wages and employment and typically find substantial wage effects. I discuss this issue in more detail in the appendix and find that the overall results of my paper do not directly contradict the DID results of other papers, but mostly reflect different choices about sample composition and the main empirical specification: Analyzed for a general sample of the working-age population the overall labor-market impacts of NREGS are relatively modest. This is also confirmed by Table C7, which provides the estimates of a DID approach for the sample used to generate the RD results and

[28] The NREGS year starts on April 1, whereas the NSS household survey data starts in July, so the overlap of both data sources is not perfect.

[29] Another way of scaling the public-employment impacts is to calculate the annual increase in NREGS employment implied by the regression results. Taking the RD estimates for public employment from the Phase 2 vs Phase 3 regressions of Table 4 literally, they imply a 6 percentage point increase in public employment per year. According to administrative data, the average person worked 42 days in that year. This implies that about 1 percent of a district's population had a NREGS job at some point over the course of the year.

finds no statistically significant wage effects. As appendix figure C.17 shows, however, the parallel trend assumption underlying the DID approach is violated for private employment at baseline, and this is also true for a number of other labor-market outcomes not reported here. This implies that the regression-discontinuity estimates, which do not require this assumption, provide the more believable program effects.

7 Conclusion

Using a regression discontinuity design, this paper has analyzed the impacts of the Indian National Rural Employment Guarantee Scheme (NREGS) on the rural labor market. The results suggest that the overall direct effects on the labor market are small, although many of the coefficients are so imprecisely estimated that larger effects cannot always be ruled out. The general qualitative pattern is robust across a range of different empirical specifications, however: the introduction of the public-works scheme at best only leads to small increases is public employment and, if at all, lowers the private-sector wage. There is some evidence that workers drop out of the private sector and move into family employment. The NREGS employment impacts are also statistically significantly higher in high-risk districts than in low-risk districts and after a negative rainfall shock.

Overall, these results suggest that NREGS is ineffective at raising private-sector casual wages through increased competitiveness in rural labor markets or a better enforcement of minimum wage laws. The program seems to work better at providing a safety net for rural populations, although this does not translate into substantial improvements in other variables like per-capita expenditures, at least in the short run. The results, while imprecise, are also consistent with NREGS indirectly subsidizing self-employment activities by making them less risky. NREGS here mainly functions as an insurance tool after bad economic shocks rather than as a way to accumulate precautionary savings.

Given the large size of a program like NREGS with expenditures of about 1 percent of Indian GDP, the results raise the question whether the provided welfare benefits are large enough to warrant the existence of such an ambitious scheme, at least in its current form, or whether the money would be more effectively spent on other anti-poverty measures. In the presence of widely documented implementation problems like rationing of NREGS jobs, the program may disproportionately benefit the poor that have the option of becoming self-employed rather than the most economically vulnerable households with few employment alternatives. Broader welfare benefits will therefore depend heavily on improving implementation quality, although some other research on NREGS also suggests that wage impacts may take more time to materialize than could be analyzed in this paper.

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Table 1: Prediction Success of Algorithm for Major Indian States

		actual NREGS		prediction success rate	
	N	Phase 1	Phase 2	Phase 1	Phase 2
Andhra Pradesh	21	13	6	0.90	0.75
Assam	23	7	6	0.91	0.75
Bihar	36	22	14	0.81	1.00
Chhattisgarh	15	11	3	0.73	1.00
Gujarat	20	6	3	0.80	0.93
Haryana	18	2	1	0.72	0.94
Jharkhand	20	18	2	0.85	1.00
Karnataka	26	5	6	0.88	0.52
Kerala	10	2	2	0.77	1.00
Madhya Pradesh	42	18	10	0.76	0.88
Maharashtra	30	12	6	0.93	0.56
Orissa	30	19	5	0.73	0.91
Punjab	15	1	2	1.00	0.93
Rajasthan	31	6	6	0.90	0.72
Tamil Nadu	26	6	4	0.88	0.95
Uttar Pradesh	64	22	17	0.88	0.79
West Bengal	17	10	7	0.76	1.00
Total	447	180	100	0.84	0.82

Note: Table includes all districts with non-missing development index value for 17 major Indian states (the only missing districts in these states are urban districts according to the Planning Commission report definition from 2003 and therefore include either the state capital or an urban agglomeration of at least one million people). Column 1 provides the number of non-missing index districts in each state. Columns 2 and 3 give the actual number of treatment districts per state in a given phase of NREGS rollout. Columns 4 and 5 give the success rate of the algorithm in predicting a district's treatment status (NREGS or no NREGS) in a given phase.

Table 2: Baseline tests

Specification	employment public	employment private	employment family	employment total	log private wage	education	land	log per capita expenditure
Panel A: men								
Linear	-0.0006	-0.0188	0.0077	-0.0111	0.0596	-0.16*	83.97	-0.0015
	(0.0024)	(0.0187)	(0.0212)	(0.0201)	(0.0398)	(0.09)	(123.03)	(0.0314)
Linear Flexible Slope	-0.0007	-0.0187	0.0077	-0.0109	0.0596	-0.16*	80.19	-0.0019
	(0.0024)	(0.0187)	(0.0212)	(0.0199)	(0.0397)	(0.09)	(118.21)	(0.0314)
Quadratic	-0.0009	-0.0155	0.0088	-0.0069	0.0527	-0.17*	31.01	-0.0116
	(0.0023)	(0.0187)	(0.0210)	(0.0194)	(0.0396)	(0.09)	(118.39)	(0.0315)
Quadratic Flexible Slope	-0.0013	-0.0365	0.0297	-0.0070	0.0805	-0.04	51.60	-0.0248
	(0.0040)	(0.0265)	(0.0278)	(0.0277)	(0.0542)	(0.11)	(147.20)	(0.0403)
N	1063	1063	1063	1063	1007	1063	1063	1063
outcome mean	0.0025	0.3109	0.5529	0.8663	4.0352	3.32	1099.63	6.34
Panel B: women								
Linear	0.0018	0.0005	0.0459	0.0503	0.0608	-0.17*	53.70	-0.0037
	(0.0012)	(0.0132)	(0.0303)	(0.0336)	(0.0494)	(0.09)	(130.69)	(0.0317)
Linear Flexible Slope	0.0018	0.0003	0.0457	0.0500	0.0609	-0.17*	49.72	-0.0041
	(0.0012)	(0.0130)	(0.0302)	(0.0333)	(0.0495)	(0.09)	(126.00)	(0.0317)
Quadratic	0.0018	-0.0011	0.0420	0.0450	0.0615	-0.18**	-3.91	-0.0133
	(0.0012)	(0.0129)	(0.0298)	(0.0330)	(0.0494)	(0.09)	(123.27)	(0.0319)
Quadratic Flexible Slope	0.0047**	-0.0170	0.0278	0.0183	0.1324**	-0.11	-3.70	-0.0265
	(0.0020)	(0.0162)	(0.0394)	(0.0440)	(0.0645)	(0.11)	(155.16)	(0.0400)
N	1063	1063	1063	1063	656	1063	1063	1063
outcome mean	0.0018	0.1400	0.3059	0.4480	3.6807	2.34	1134.90	6.35

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$ Robust standard errors for clustering at district level in parentheses. An observation is a district in a given season in the baseline data (July 2004-June 2005). An employment outcome is the proportion of working-age adults (18-60 years) with at most secondary education in rural areas working in a given type of employment in the last 7 days. Parametric regressions with different levels of flexibility are reported. The log private wage in column 5 is conditional on private employment.

Table 3: Summary Statistics for Districts at Baseline by Phase (Men and Women)

	Men				Women			
	phase 2		phase 3		phase 2		phase 3	
		N		N		N		N
private employment	0.2975	396	0.2938	668	0.1397	396	0.1332	668
family employment	0.5810	396	0.5271	668	0.2559	396	0.3281	668
public employment	0.0038	396	0.0015	668	0.0028	396	0.0013	668
daily wage (total)	52.75	387	65.71	645	38.19	306	45.93	504
daily wage (private)	52.77	386	65.78	645	37.69	303	45.76	497
daily wage (public)	53.44	18	63.54	22	53.42	12	52.32	17

Note: An observation is a district with non-missing Planning Commission index value in a given season in the baseline data (July 2004-June 2005). Summary statistics are calculated from aggregated and weighted individual NSS data.

Table 4: NREGS impact: wages and employment (men and women)

Specification	employment public	 private	 family	 total	log private wage
Panel A: men					
Linear	0.0012	-0.0351*	0.0253	-0.0069	-0.0041
	(0.0038)	(0.0208)	(0.0247)	(0.0185)	(0.0377)
Linear Flexible Slope	0.0011	-0.0351*	0.0256	-0.0068	-0.0041
	(0.0038)	(0.0208)	(0.0244)	(0.0185)	(0.0377)
Quadratic	0.0007	-0.0369*	0.0292	-0.0055	-0.0070
	(0.0038)	(0.0204)	(0.0243)	(0.0187)	(0.0375)
Quadratic Flexible Slope	0.0018	-0.0522*	0.0302	-0.0165	-0.0196
	(0.0045)	(0.0273)	(0.0331)	(0.0231)	(0.0500)
N	1063	1063	1063	1063	1007
outcome mean	0.0069	0.3279	0.4846	0.8195	4.1212
Panel B: women					
Linear	0.0013	-0.0035	0.0166	0.0140	0.0041
	(0.0044)	(0.0166)	(0.0259)	(0.0301)	(0.0660)
Linear Flexible Slope	0.0013	-0.0034	0.0161	0.0137	0.0038
	(0.0044)	(0.0166)	(0.0256)	(0.0298)	(0.0663)
Quadratic	0.0015	-0.0020	0.0108	0.0101	0.0050
	(0.0045)	(0.0165)	(0.0255)	(0.0296)	(0.0660)
Quadratic Flexible Slope	-0.0026	-0.0073	0.0340	0.0263	-0.0706
	(0.0043)	(0.0210)	(0.0334)	(0.0385)	(0.0925)
N	1063	1063	1063	1063	656
outcome mean	0.0053	0.1309	0.2285	0.3647	3.6488

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$ Robust standard errors for clustering at district level in parentheses. An observation is a district in a given season. An employment outcome is the proportion of working-age adults (18-60 years) with at most secondary education in rural areas working in a given type of employment in the last 7 days. Parametric regressions with different levels of flexibility are reported. The log private wage in column 5 is conditional on private employment.

Table 5: NREGS impacts and safety net (men and women)

	employment				log private
Specification	public	private	family	total	wage
Panel A: men					
Linear	-0.0047	-0.0291	0.0400	0.0057	0.0365
	(0.0090)	(0.0302)	(0.0330)	(0.0262)	(0.0512)
NREGS*negative shock	0.0285*	-0.0212	-0.0129	-0.0026	-0.0605
	(0.0148)	(0.0336)	(0.0411)	(0.0319)	(0.0702)
Linear Flexible Slope	-0.0050	-0.0288	0.0397	0.0101	0.0365
	(0.0090)	(0.0303)	(0.0331)	(0.0260)	(0.0513)
NREGS*negative shock	0.0288*	-0.0219	-0.0124	-0.0019	-0.0610
	(0.0147)	(0.0337)	(0.0414)	(0.0317)	(0.0712)
Quadratic	-0.0058	-0.0282	0.0397	0.0057	0.0326
	(0.0090)	(0.0302)	(0.0330)	(0.0262)	(0.0511)
NREGS*negative shock	0.0286*	-0.0214	-0.0128	-0.0026	-0.0583
	(0.0147)	(0.0337)	(0.0412)	(0.0319)	(0.0708)
Quadratic Flexible Slope	-0.0057	-0.0381	0.0389	-0.0051	-0.0056
	(0.0107)	(0.0404)	(0.0458)	(0.0326)	(0.0677)
NREGS*negative shock	0.0299**	-0.0223	-0.0085	0.0021	-0.0595
	(0.0152)	(0.0337)	(0.0414)	(0.0316)	(0.0717)
N	532	532	532	532	504
outcome mean	0.0115	0.3380	0.4681	0.8176	4.1786
Panel B: women					
Linear	-0.0053	0.0150	0.0011	0.0052	-0.0148
	(0.0081)	(0.0232)	(0.0285)	(0.0342)	(0.0801)
NREGS*negative shock	0.0240	-0.0271	0.0104	0.0158	-0.0071
	(0.0166)	(0.0287)	(0.0401)	(0.0458)	(0.1191)
Linear Flexible Slope	-0.0020	0.0101	0.0173	0.0193	0.0175
	(0.0088)	(0.0249)	(0.0306)	(0.0361)	(0.0873)
NREGS*negative shock	0.0245	-0.0277	0.0127	0.0178	-0.0128
	(0.0166)	(0.0287)	(0.0397)	(0.0454)	(0.1188)
Quadratic	-0.0053	0.0150	0.0011	0.0052	-0.0148
	(0.0081)	(0.0232)	(0.0285)	(0.0342)	(0.0801)
NREGS*negative shock	0.0240	-0.0271	0.0104	0.0158	-0.0071
	(0.0166)	(0.0287)	(0.0401)	(0.0458)	(0.1191)
Quadratic Flexible Slope	-0.0163*	0.0100	0.0404	0.0304	-0.0215
	(0.0094)	(0.0284)	(0.0385)	(0.0433)	(0.1032)
NREGS*negative shock	0.0280	-0.0277	0.0067	0.0149	-0.0049
	(0.0172)	(0.0278)	(0.0396)	(0.0456)	(0.1201)
N	532	532	532	532	321
outcome mean	0.0093	0.1282	0.2114	0.3489	3.7233

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors clustered at district level in parentheses. *negative shock* is a dummy variable equal to 1 if there was a negative deviation of rainfall from expected rainfall during the last monsoon season. Sample is restricted to agricultural off-season.

Table 6: NREGS impacts and risk: wages and employment (men)

Specification	public	employment private	family	total	log private wage
Linear	-0.0056	-0.0447*	0.0335	-0.0241	-0.0153
	(0.0082)	(0.0243)	(0.0274)	(0.0313)	(0.0552)
NREGS*risky	0.0169**	0.0160	-0.0151	0.0527**	0.0765
	(0.0072)	(0.0200)	(0.0223)	(0.0258)	(0.0550)
Linear Flexible Slope	-0.0055	-0.0461	0.0282	-0.0240	-0.0505
	(0.0082)	(0.0350)	(0.0389)	(0.0306)	(0.0593)
NREGS*risky	0.0167**	0.0026	0.0341	0.0525**	0.0780
	(0.0073)	(0.0309)	(0.0319)	(0.0256)	(0.0553)
Quadratic	-0.0064	-0.0351	0.0158	-0.0244	-0.0180
	(0.0081)	(0.0325)	(0.0353)	(0.0313)	(0.0548)
NREGS*risky	0.0168**	0.0021	0.0347	0.0527**	0.0756
	(0.0073)	(0.0308)	(0.0319)	(0.0257)	(0.0552)
Quadratic Flexible Slope	-0.0033	-0.0483	0.0169	-0.0333	-0.0633
	(0.0092)	(0.0418)	(0.0479)	(0.0372)	(0.0729)
NREGS*risky	0.0166**	0.0025	0.0339	0.0521**	0.0773
	(0.0072)	(0.0309)	(0.0319)	(0.0254)	(0.0551)
N	532	532	532	532	504
outcome mean	0.0115	0.3380	0.4681	0.8176	4.1786

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at district level in parentheses. Log private wage in column 5 is conditional on private employment. Risky districts are those with an above-median variance of rainfall. Regressions control for expected rainfall.

Table 7: NREGS impacts and different sample restrictions: wages and employment (men and women)

Specification	employment public	employment private	employment family	employment total	log private wage
Panel A: men					
Linear flexible	-0.0014	-0.0431	0.0058	-0.0342	-0.0747
(-5$\leq$ rank $\leq$ 5)	(0.0052)	(0.0350)	(0.0393)	(0.0272)	(0.0589)
N	543	543	543	543	522
Linear flexible	0.0014	-0.0551	0.0215	-0.0238	-0.0568
(-4$\leq$ rank $\leq$ 4)	(0.0084)	(0.0348)	(0.0436)	(0.0315)	(0.0615)
N	463	463	463	463	445
Linear flexible	-0.0034	-0.0690*	0.0319	-0.0298	-0.1120
(-3$\leq$ rank $\leq$ 3)	(0.0055)	(0.0401)	(0.0531)	(0.0368)	(0.0805)
N	375	375	375	375	358
Panel B: women					
Linear flexible	-0.0001	0.0105	0.0200	0.0304	0.0105
(-5$\leq$ rank $\leq$ 5)	(0.0045)	(0.0278)	(0.0368)	(0.0448)	(0.1010)
N	543	543	543	543	363
Linear flexible	0.0033	0.0107	0.0613	0.0776	0.0697
(-4$\leq$ rank $\leq$ 4)	(0.0077)	(0.0305)	(0.0434)	(0.0518)	(0.0989)
N	463	463	463	463	311
Linear flexible	-0.0035	0.0109	0.1045**	0.1111*	-0.0421
(-3$\leq$ rank $\leq$ 3)	(0.0049)	(0.0352)	(0.0454)	(0.0563)	(0.1124)
N	375	375	375	375	251

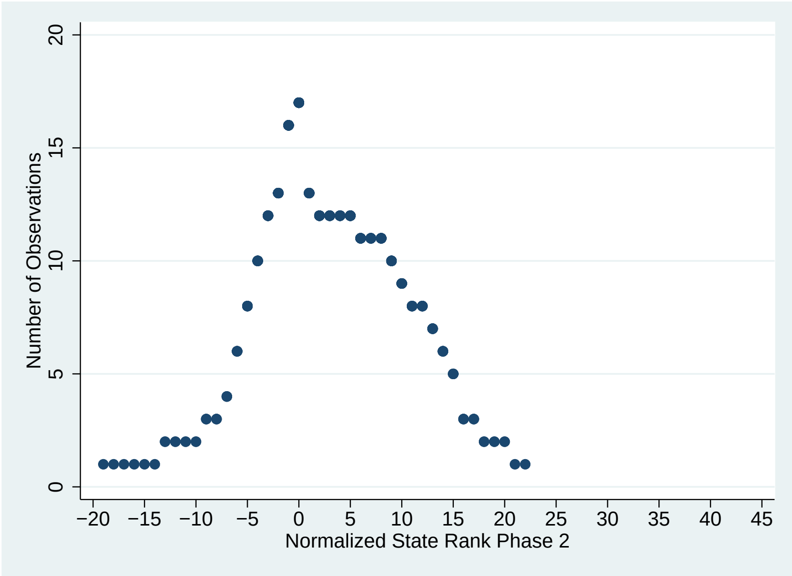
Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors for clustering at district level in parentheses. An observation is a district in a given season. An employment outcome is the proportion of working-age adults (18-60) with at most secondary education in rural areas working in a given type of employment in the last 7 days. The log private wage in column 5 is conditional on private employment. Sample restrictions apply to the re-centered state-specific rank variable.

Table 8: NREGS impact (meta analysis): wages and employment (men)

Specification	employment public	private	family	total	log private wage
Linear (simple average)	-0.0021	-0.0348**	0.0302	-0.0067	0.0153
	(0.4926)	(0.0283)	(0.1113)	(0.6368)	(0.6214)
Quadratic (simple average)	0.0029	-0.0738***	0.0693***	-0.0017	-0.0156
	(0.3468)	(0.0001)	(0.0037)	(0.9108)	(0.6865)
Linear (pop. weighted)	-0.0016	-0.0299*	0.0374*	0.0059	0.0111
	(0.5661)	(0.0632)	(0.0606)	(0.6802)	(0.7301)
Quadratic (pop. weighted)	-0.0003	-0.0501***	0.0616***	0.0113	-0.0059
	(0.9297)	(0.0051)	(0.0067)	(0.4549)	(0.8729)
N	951	951	951	951	927

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors for clustering at district level in parentheses. An observation is a district in a given season. An employment outcome is the proportion of working-age adults (18-60) with at most secondary education in rural areas working in a given type of employment in the last 7 days. Parametric regressions with different levels of flexibility are reported. The log private wage in column 5 is conditional on private employment. NREGS is the predicted treatment status. Treatment effects at the cutoff are estimated separately by state and then combined through a simple average in the *simple average* specifications, whereas the state-specific estimates are weighted by state popualtion in the *pop. weighted* specifications.

Figure 1: Number of observations per state rank for Phase 2



Note: Figure 1 excludes Phase 1 districts. Planning Commission ranks are made state-specific and re-centered such that the last district eligible for receiving NREGS in Phase 2 according to the proposed algorithm has a rank of 0. Districts with positive ranks should be ineligible for the program.

Figure 2: General Distribution of Index over Ranks

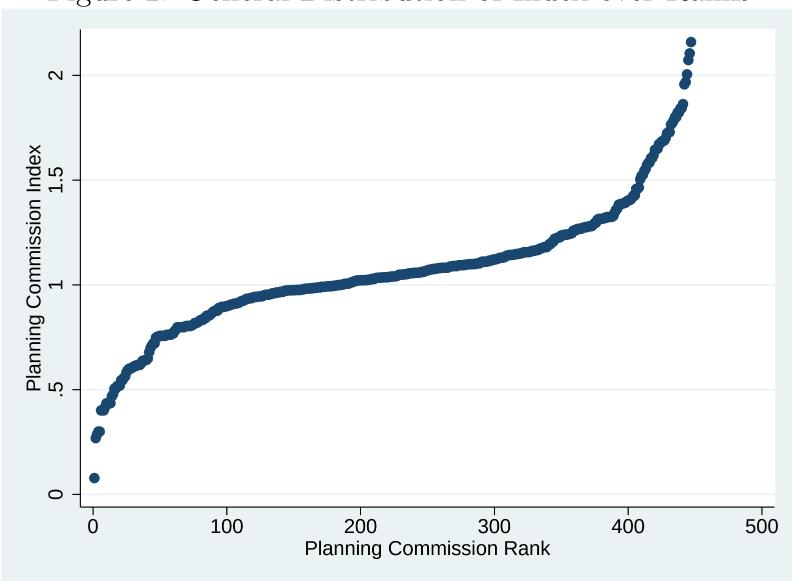


Figure 3: Distribution of Index over State-Specific Ranks (Phase 2 vs Phase 3)

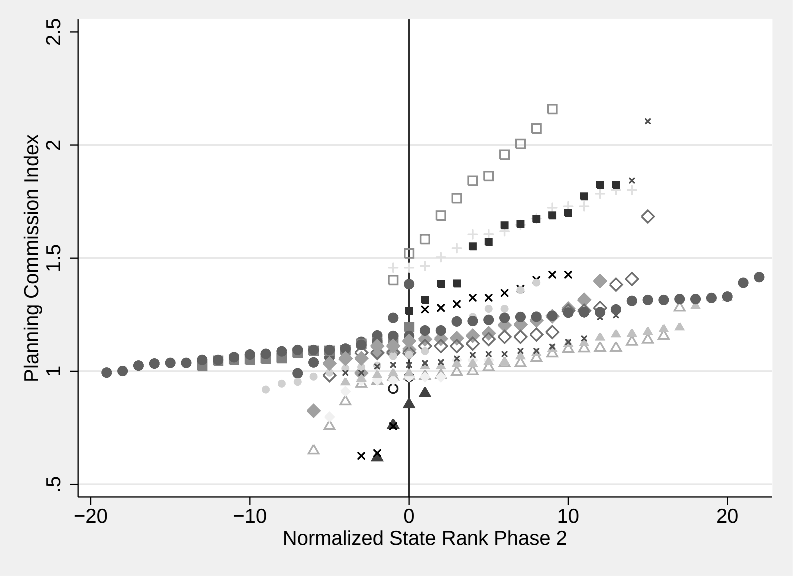
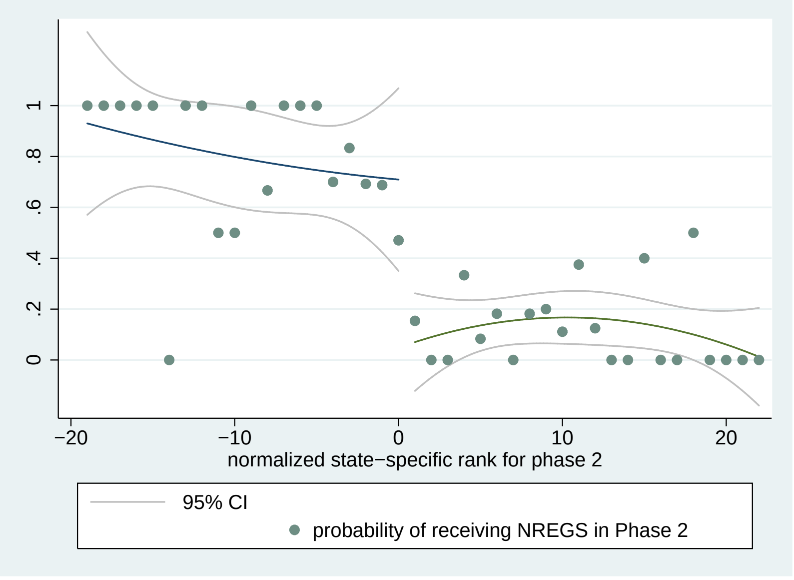


Figure 4: Discontinuity of treatment status for Phase 2



Note: Figure 4 excludes Phase 1 districts. The used bin size is 1, so each individual rank.

Figure 5: NREGS impact on male public employment

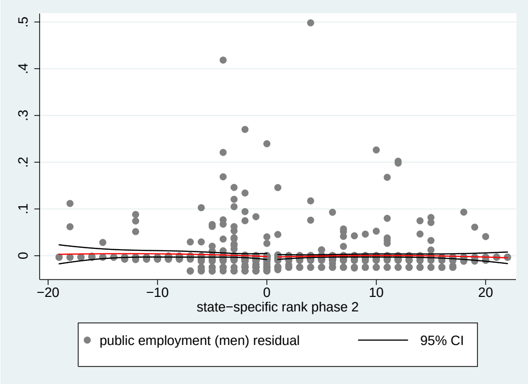


Figure 6: NREGS impact on male private employment

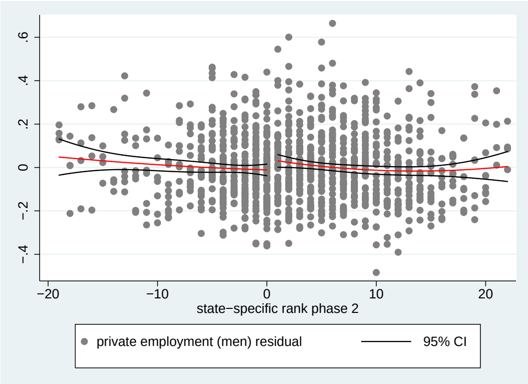
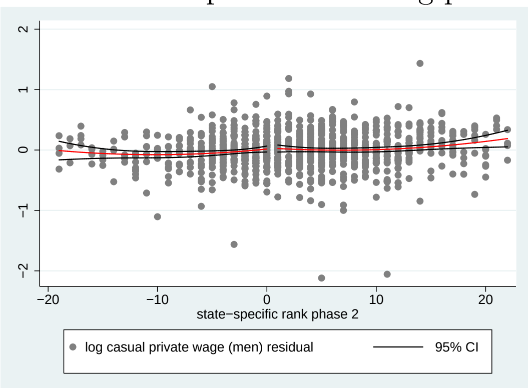


Figure 7: NREGS impact on male log private wage



Note: An observation is the residual average district-season-level outcome at a given rank, where the impact of the baseline outcome variable and state fixed effects has been taken out. Fitted curves are quadratic polynomials.

Figure 8: NREGS impact on female public employment

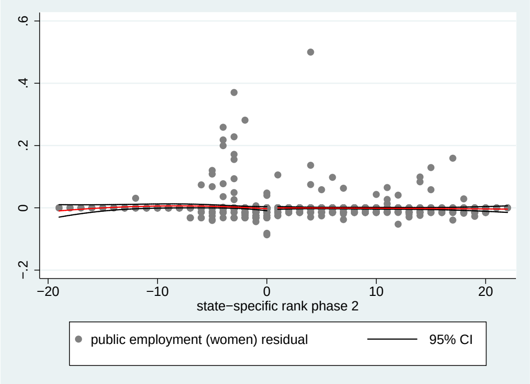


Figure 9: NREGS impact on female private employment

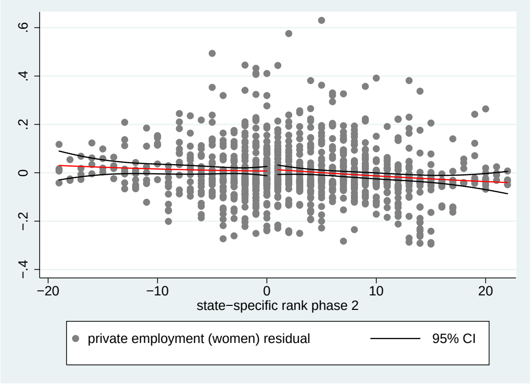
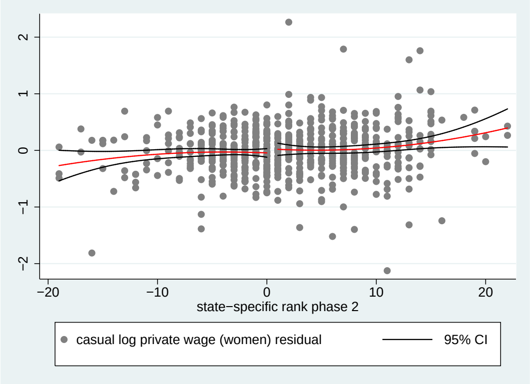


Figure 10: NREGS impact on female log private wage



Note: An observation is the residual average district-season-level outcome at a given rank, where the impact of the baseline outcome variable and state fixed effects has been taken out. Fitted curves are quadratic polynomials.

Appendix

A Additional Information on the Program Rollout and Used Algorithm

The general government documents state that NREGS was rolled out to the poorest districts first, but do not explicitly define the algorithm the government used to decide which districts would receive the program in which phase. While the actual algorithm is not publicly available, institutional knowledge about existing information at the time and about the workings of earlier development initiatives allows the construction of a plausible algorithm that works in two steps: First, the number of districts that are allocated to a given state is proportional to the prevalence of poverty across states. This mechanism ensures that the number of districts allocated to a given state is roughly proportional to the percent of India's poor people living in that state.[30] Second, within a state districts are chosen based on a development ranking, so that poor districts are chosen first.

This algorithm is attractive in the Indian context since it takes into account political fairness of resource allocation across states and within states and had been used for earlier development programs: The Indian Planning Commission explicitly states, for example, that this method had been used for treatment assignment of an earlier much smaller and less ambitious temporary government program aimed at less developed districts.[31] A former member of the Planning Commission also confirms that the development ranking was indeed used for NREGS as well and that state allocations were made proportional to the prevalence of poverty across states. Additionally, given the importance of NREGS and the huge political interest and awareness it created among policymakers at all levels as well as NGOs and the press, it seems very likely that the Indian government adhered to these political fairness norms in the allocation of treatment districts for NREGS as well. A number of NGOs and well-known individuals were actively campaigning for the introduction of an employment guarantee scheme like NREGS, and have been closely monitoring the working of the program since its introduction.[32]

The information used to rank districts is well documented and very transparent: In 2003, the Planning Commission published a report that created a 'backwardness index' from data from the early to mid-1990s on three outcomes (agricultural wages, agricultural productivity, and proportion of low-caste individuals living in the district) (Planning Commission 2003). Districts were then ranked based on their index values. The goal of the index at the time was to identify especially underdeveloped districts for wage and self-employment programs and, as mentioned above, it was actually used in pre-NREGS district initiatives, but those programs were much less extensive than NREGS and usually envisioned as temporary programs.

What is less well-documented is the choice of the poverty criterion to determine across-state allocations of treatment districts, since the Planning Commission report that created

[30] In practice this provision also ensures that all states (union territories, which are administrative units directly ruled by the national government, are usually excluded from such programs) receive at least one treatment district.

[31] See e.g. Planning Commission (MLP Division) 2003 for RSVY district assignment.

[32] Jean Dreze and Reetika Khera have been especially involved in NREGS from the beginning. Examples of monitoring include awareness campaigns for workers' rights under NREGS, survey data collection to find out about common challenges and violations of the law, suing governments for NREGA violations, and drawing attention to corruption. See e.g. Samarthan Centre for Development Support 2007.

the development index did not cover this provision of the algorithm, and other Planning Commission documents that describe the algorithm used for the district assignment of government initiatives just refer to the 'incidence of poverty' as the used criterion, but never explicitly define the term or its operationalization. Given the Indian government's focus on poverty headcount ratios (the percent of people living below the poverty line) in many reports and publications, the best guess of the used poverty definition seems to be the state headcount ratio times the rural state population. This provides an estimate of how many below-the-poverty-line people live in a given state and of how poverty levels compare across states. I therefore use this procedure as the first step of the algorithm, where a state is assigned a percentage of total treatment districts that is equal to the percentage of India's poor living in that state. For the calculations I use the headcount ratios calculated from 1993-1994 NSS data[33], which is nationally representative household survey data that a former member of the Planning Commission says was used to derive state allocations of NREGS districts since the newest available information on poverty at the time from 1999-2000 NSS data was subject to controversies and was therefore not used.

B Derivation of Theoretical Results

B.1 The Baseline Model without NREGS

The model describes a household's optimal time allocation in a one-period setting. Before NREGS is introduced, a household can first choose to allocate the total time of their household members, T, between working for a big landowner as agricultural laborer in the private casual sector, l, and working on the family farm, f. After this decision has been made, a weather shock is realized that determines the payoff from farm work. The period ends, and the household earns the fixed wage w in the private sector, and income y for the time spent in farming. The household derives utility both from the time spent working in self-employment on the family farm, and from the total income earned in both activities during the period. The utility function is additively separable in these components, with $u' > 0, u'' < 0$, $v' > 0, v'' < 0$, and with weight α given to the utility from self-employment.

At the beginning of the period, a household's optimization problem is

$$\max_{l} \alpha v(T-l) + (1-\alpha)E[u((T-l)y + lw)]$$

Which leads to the first-order condition

$$\alpha v'(T-l) = (1-\alpha)\int u'((T-l)y + lw)(w-y)g(y)dy \tag{4}$$

(4) pins down the optimal proportion of time l spent working in the private sector implicitly.

Lemma 1 *There exists a unique optimal private-sector time allocation decision l.*

[33] I use the rural state headcount ratios from Planning Commission (2009), since the original headcount ratio calculations do not have estimates for new states that had been created in the meantime. Since these are official Planning Commission estimates, they seem like the best guess of the information the Indian government would have had access to at the time of NREGS implementation.

Proof. The right-hand side of (4) is decreasing in l, whereas the left-hand side of (4) is increasing in l. By the intermediate value theorem, there must therefore be a value of l at which the first-order condition is satisfied. ∎

Now consider how the optimal proportion of time spent in private employment changes with the variability of the weather-shock distribution. Suppose that the distribution of y in district B is a mean-preserving spread of the distribution in district A. This means that farming is riskier in district B than in district A.

Proposition 2 *Households spend more in time in casual private employment in riskier districts than in less risky districts.*

Proof. As households are risk averse, the expected utility from farming is lower in B, which implies that the right-hand side of (4) is larger in B for given values. The optimal value of l in A is therefore not the optimal time allocation in B. Since the right-hand side of (4) is decreasing in l, whereas the left-hand side is increasing in l, the optimal l in district B is larger than in district A. ∎

B.2 The Model with NREGS

After NREGS is introduced, the program can be used both as an alternative source of employment regardless of the weather shock, and as an insurance tool after bad weather shocks. This alters the baseline model in two ways: The household now first makes a time-allocation decision among three alternatives: working for a big landowner as agricultural laborers in the private casual sector (l), working on the family farm (f_1), and taking up a NREGS job (n_1). After this decision has been made, as before a weather shock is realized that affects the payoff from farm work. The time originally allocated to farm work,f_1, can then be split between actually working on the farm, f_2, and between taking up public employment in a NREGS project instead (n_2). After this decision, the period ends and the payoffs are realized. As before, the payoff from farm employment is y and the private-sector wage is w. The NREGS program wage is $\overline{w}$. The household again derives utility from the time spent in self-employment and from the total income earned.

The new household optimization problem at the beginning of the period is now given by

$$\max_{l,n_1} E[\alpha v(T-l-n_1-n_2^*)+(1-\alpha)u((T-l-n_1-n_2^*)y+n_2^*\overline{w}+lw+n_1\overline{w})]$$

Where n_2^* is the best-response function of n_2 given y since the household can optimize the time spent working for NREGS and actually working on the family farm after the weather shock has occurred and y has been realized. Once a household chooses the fraction of time to spend on NREGS employment after the weather shock has occurred, l, n_1, and y are fixed. The household therefore chooses n_2 to maximize

$$\max_{n_2} \alpha v(T-l-n_1-n_2)+(1-\alpha)u((T-l-n_1-n_2)y+n_2\overline{w}+lw+n_1\overline{w})$$

Leading to the first-order condition

$$\alpha v'(T - l - n_1 - n_2) = (1-\alpha)u'((T - l - n_1 - n_2)y + n_2\overline{w} + lw + n_1\overline{w})(\overline{w} - y) \quad (5)$$

Lemma 3 *There exists a unique optimal amount of time spent in n_2 (NREGS employment as ex-post insurance) for a given y.*

Proof. The right-hand side of (5) is decreasing in n_2, whereas the left-hand side of (5) is increasing in n_2. By the intermediate value theorem, there must therefore be a value of n_2 at which the first-order condition is satisfied. ■

Define the shock y_0 as the shock at which the first-order condition implies n_2=0. Then the first-order condition traces out the best-response function n_2^* for all weather shocks that imply a farming income of y_0 or less. For all larger values of y, the optimal n_2 is zero. Therefore, we have

$$n_2^* = \begin{cases} \text{implied } n_2 \text{ from (5)} & y \le y_0 \\ 0 & y > y_0 \end{cases}$$

Knowing n_2^* and the distribution of y, at the beginning of the period the household needs to decide how much time to spend in the private sector, in NREGS employment, and in anticipated farming.

Lemma 4 *A household will work either in private-sector work l or in ex-ante NREGS employment n_1, and will work in the job that pays more.*

Proof. l and n_1 are perfect substitutes for a household in terms of their contribution to household utility. Both are safe sources of employment that need to be committed to before the weather shock is realized. A household therefore maximizes utility by choosing the alternative that pays a higher wage. ■

Define j as the amount of time spent working in the activity that pays the higher wage, such that

$$j = \begin{cases} n_1 & w \le \overline{w} \\ l & w > \overline{w} \end{cases}$$

And define $\widetilde{w}$ analogously as the corresponding wage.

The household maximization problem can therefore be rewritten as

$$\max_j E[\alpha v((T - j - n_2^*)) + (1-\alpha)u((T - j - n_2^*)y + n_2^*\overline{w} + j\widetilde{w})]$$

Working in the fact that the optimal n_2 is zero at large shocks, the problem can be rewritten as

$$\max_j \int_{y \le y_0} [\alpha v(T - j - n_2^*) + (1-\alpha)u((T - j - n_2^*)y + n_2^*\overline{w} + j\widetilde{w})]g(y)dy$$

$$+ \int_{y > y_0} [\alpha v(T - j) + (1-\alpha)u((T - j)y + j\widetilde{w})]g(y)dy$$

This leads to the first-order condition

$$\frac{\alpha}{1-\alpha}\int_{y\leq y_0} v'(T-j-n_2^*)(1+\frac{\partial n_2^*}{\partial j})g(y)dy + \frac{\alpha}{1-\alpha}v'(T-j)$$

$$- \int_{y>y_0} u'((T-j)y+j\widetilde{w})(\widetilde{w}-y)g(y)dy$$
$$= \int_{y\leq y_0} u'((T-j-n_2^*)y+n_2^*\overline{w}+j\widetilde{w})(\widetilde{w}-y+(\overline{w}-y)\tfrac{\partial n_2^*}{\partial j})g(y)dy \quad (6)$$

Lemma 5 *A sufficient condition for the existence of a unique optimal amount of time spent in employment j is that agents are sufficiently risk averse.*

Proof. For an interior solution to be guaranteed, one side of (6) should be increasing and the other side decreasing in j. Some algebra shows that signing the partial derivatives on both sides is only possible if the sign of $\frac{\partial^2 n_2^*}{\partial j^2}$ is known. If it is positive, the derivative of left-hand side of (6) is positive, whereas each term of the right-hand side derivative is negative as long as

$$-\frac{u''((T-j-n_2^*)y+n_2^*\overline{w}+j\widetilde{w})}{u'((T-j-n_2^*)y+n_2^*\overline{w}+j\widetilde{w})} > \frac{(\overline{w}-y)(-\frac{\partial^2 n_2^*}{\partial j^2})}{(\widetilde{w}-y+(\overline{w}-y)\frac{\partial n_2^*}{\partial j})^2}$$

holds for all possible values of y.

Similarly, if the expression is negative, the derivative of the right-hand side of (6) is negative and all terms of the left-hand side derivative are positive as long as

$$-\frac{v''(T-j-n_2^*)}{v'(T-j-n_2^*)} > \frac{-\frac{\partial^2 n_2^*}{\partial j^2}}{(1+\frac{\partial n_2^*}{\partial j})^2}$$

holds for all possible values of y.

Under these conditions, there is a unique interior solution satisfying the first-order condition according to the intermediate value theorem. ■

Since $\frac{-u''(.)}{u'(.)}$ is the Arrow-Pratt measure of absolute risk aversion, these sufficient conditions mean intuitively that an agent needs to be risk averse 'enough'.

Notice how the sufficient conditions for a unique solution do not depend on the sign of $\frac{\partial n_2^*}{\partial j}$, which is ambiguous. Intuitively, how the time allocated to the ex-post NREGS employment responds to an increase in the time allocated to precautionary activity j depends on the attractiveness of the wage for j relative to the NREGS wage $\overline{w}$ and y. In other words, j only functions well as a precautionary savings tool if the paid wage in that activity is not too low relative to the payoffs that can be achieved through NREGS employment and farming after the weather shock is realized. A sufficient condition for j and n_2^* being substitutes for shocks $y \leq y_0$ is $\widetilde{w} \geq \overline{w}$.

A couple of predictions about the impact of NREGS follow from the model setup.

Proposition 6 *If the NREGS wage is high relative to the private-sector wage, the introduction of NREGS completely crowds out private-sector employment.*

Proof. This follows directly from Lemma 4 for $\overline{w} > w$. NREGS as a precautionary savings tool here directly replaces private-sector employment. ■

Proposition 7 *Even if the NREGS wage is low relative to the private-sector wage, the introduction of NREGS reduces the amount of time spent in private-sector employment under reasonable assumptions. Workers spend more time in farm work and, after bad income shocks, in NREGS employment instead.*

Proof. This follows from comparing (4) and (6), where $j = l$ since $\overline{w} < w$. (6) can be re-written as

$$\alpha v'(T-l)$$

$$=$$

$$(1\text{-}\alpha)\int_{y\leq y_0} u'((T-l-n_2^*)y + n_2^*\overline{w} + lw)(w - y + (\overline{w}-y)\tfrac{\partial n_2^*}{\partial l})g(y)dy$$

$$+\ (1\text{-}\ \alpha)\int_{y>y_0} u'((T-l)y + lw)(w-y)g(y)dy$$

$$-\ \alpha\int_{y\leq y_0} v'(T-l-n_2^*)(1+\tfrac{\partial n_2^*}{\partial l})g(y)dy \ (7)$$

The left-hand side of (7) is identical to the left-hand side of (4), but the first two terms of the right-hand side of (7) taken together are lower than the right-hand side of (4) since NREGS raises the expected utility at low y outcomes and therefore lowers the expected marginal utility for these shocks.

$\frac{\partial n_2^*}{\partial l}$ is negative since $w \geq \overline{w}$. Assume that n_2^* and l are relatively poor substitutes for each other such that $\frac{\partial n_2^*}{\partial l} > -1$ holds. That the substitutability of the two variables is less than 1 in absolute terms is intuitive since one is a precautionary savings tool whereas the other one functions as ex-post insurance. Then, all three terms of (7) taken together are now smaller than the right-hand side of (4). This implies that the old time allocation l is no longer the optimal solution. Since the right-hand side of the equation above is decreasing in l whereas the left-hand side is increasing, this in turn implies that the new optimal l is lower than the old one. ■

Proposition 8 *NREGS take-up is low on average if the program primarily functions as a safety net tool.*

Proof. This follows from Propositions 6 and 7. If NREGS is primarily used as a precautionary savings measure, NREGS employment crowds out private-sector employment and will be high. If NREGS mainly functions as insurance, then NREGS is only taken up after bad shocks to y, and will therefore be low in the absence of large negative aggregate shocks. ■

Proposition 9 *NREGS employment in riskier districts is higher than in less-risky environments.*

Proof. Assume again that district B's distribution of y is a mean-preserving spread of that in district A. This means that the probability of receiving a low draw $y < y_0$ is higher in district B than in district A, which implies that the expected amount of time spent in NREGS employment is higher in B than in A. ■

Proposition 10 *In riskier districts, private-employment decreases more than in less risky districts if NREGS is used as insurance.*

Proof. This follows from Proposition 7, combined with the following observation: The mean-preserving spread of y will reduce the magnitude of the first term of the right-hand side of (7), but will increase both the second and the third term in absolute value. At very bad shocks, the absolute value of the third term goes towards infinity as $n_2^* \to 1$, whereas the second term goes towards a fixed value. For these large shocks, the right-hand side of (7) therefore decreases more in risky than in lower risk districts, which implies a larger reduction in l in risky than in less risky districts. ■

B.2.1 Extensions: NREGS Cap, Implementation Problems and Private-Sector Wage Variability

So far the model assumes that an agent can perfectly choose the amount of NREGS employment that is optimal for him, be it as a precautionary savings measure n_1 or as a safety net measure n_2. In reality, NREGS employment is officially capped at 100 days per household per year. This makes NREGS less attractive both as a risk-mitigation tool and as an ex post insurance mechanism, and will therefore attenuate the labor market impacts of NREGS predicted by the model. An implication of this feature is also that Proposition 10 may no longer hold: If the restriction on the maximum time spent in NREGS employment means that there is much less insurance after exceptionally bad weather shocks than in the absence of this rule, then households living in risky districts will reduce their time spent in private employment l less than agents in less risky districts.

In addition to the cap on NREGS employment, public-works programs in developing countries are often plagued by implementation problems like rationing of jobs or underpayment of wages due to corruption. This limits the amount of time that can be spent in NREGS employment even further in the case of rationing, and will reduce the actual wage received by program participants in the case of corruption. Both of these changes make NREGS less attractive than in the baseline model and therefore again attenuate the impacts NREGS has on labor-market outcomes.

The model also assumes that the private-sector wage is fixed regardless of the weather shock. If the private-sector wage also depends on the weather, private-sector employment is a less useful tool for risk mitigation than in the model, which increases the negative impacts NREGS has on private-sector employment.

C Additional Tables and Figures

Table C1: NREGS impact (TOT): wages and employment (men and women)

Specification	public	employment private	family	total	log private wage
Panel A: men					
Linear	0.0027	-0.0799	0.0579	-0.0157	-0.0093
	(0.0085)	(0.0508)	(0.0583)	(0.0417)	(0.0847)
Linear Flexible Slope	0.0025	-0.0805	0.0591	-0.0155	-0.0087
	(0.0086)	(0.0507)	(0.0576)	(0.0417)	(0.0853)
Quadratic	0.0017	-0.0875*	0.0696	-0.0130	-0.0165
	(0.0089)	(0.0528)	(0.0608)	(0.0439)	(0.0871)
Quadratic Flexible Slope	0.0082	-0.1056*	0.0603	-0.0328	-0.1203
	(0.0092)	(0.0567)	(0.0631)	(0.0438)	(0.1071)
N	1063	1063	1063	1063	1007
outcome mean	0.0069	0.3279	0.4846	0.8195	4.1208
Panel B: women					
Linear	0.0030	-0.0081	0.0376	0.0318	0.0091
	(0.0100)	(0.0376)	(0.0594)	(0.0683)	(0.1439)
Linear Flexible Slope	0.0030	-0.0078	0.0370	0.0311	0.0063
	(0.0101)	(0.0379)	(0.0596)	(0.0684)	(0.1410)
Quadratic	0.0035	-0.0049	0.0255	0.0239	0.0115
	(0.0106)	(0.0388)	(0.0605)	(0.0698)	(0.1498)
Quadratic Flexible Slope	0.0019	-0.0161	0.0906	0.0792	-0.1220
	(0.0118)	(0.0394)	(0.0687)	(0.0800)	(0.1887)
N	1063	1063	1063	1063	656
outcome mean	0.0053	0.1309	0.2285	0.3647	3.6488

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors for clustering at district level in parentheses. An observation is a district in a given season. An employment outcome is the proportion of working-age adults (18-60 years) with at most secondary education in rural areas working in a given type of employment in the last 7 days. Parametric regressions with different levels of flexibility are reported. The log private wage in column 5 is conditional on private employment. Actual treatment with NREGS is instrumented with the predicted treatment status. The first-stage F-statistic is 114.80 for the linear, 82.44 for the linear flexible, 100.53 for the quadratic, and 56.82 for the flexible quadratic specifications.

Table C2: NREGS impacts on other outcomes: Expenditures, total wage, remittances (men)

Specification	log per-capita expenditures	log total wage	log remittances
Panel A: overall sample			
Linear	0.0195	-0.0050	-0.0065
	(0.0346)	(0.0375)	(0.1028)
Linear Flexible Slope	0.0199	-0.0050	-0.0069
	(0.0345)	(0.0375)	(0.1027)
Quadratic	0.0219	-0.0083	-0.0250
	(0.0350)	(0.0372)	(0.1031)
Quadratic Flexible Slope	0.0275	-0.0088	-0.0141
	(0.0488)	(0.0491)	(0.1390)
N	1063	1011	1030
outcome mean	6.4798	4.1267	9.1621
Panel B: rainfall shock			
Linear	0.0115	0.0277	-0.0192
	(0.0391)	(0.0510)	(0.1355)
NREGS*negative shock	-0.0382	-0.0398	0.0181
	(0.0506)	(0.0675)	(0.1673)
Linear Flexible Slope	0.0121	0.0277	-0.0206
	(0.0392)	(0.0510)	(0.1358)
NREGS*negative shock	-0.0394	-0.0394	0.0207
	(0.0501)	(0.0681)	(0.1665)
Quadratic	0.0156	0.0232	-0.0422
	(0.0394)	(0.0504)	(0.1358)
NREGS*negative shock	-0.0389	-0.0378	0.0226
	(0.0502)	(0.0680)	(0.1647)
Quadratic Flexible Slope	0.0435	0.0077	0.0402
	(0.0581)	(0.0678)	(0.1709)
NREGS*negative shock	-0.0476	-0.0419	-0.0267
	(0.0516)	(0.0699)	(0.1658)
N	532	508	514
outcome mean	6.5160	4.1870	9.2775

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors for clustering at district level in parentheses. An observation is a district in a given season. Parametric regressions with different levels of flexibility are reported. NREGS is the predicted treatment status. The log total wage is conditional on having earned a positive wage.

Table C3: NREGS impact (donut hole approach): wages and employment (men and women)

Specification	public	employment private	family	total	log private wage
Panel A: men					
Linear	-0.0001	-0.0408*	0.0511*	0.0132	0.0014
	(0.0040)	(0.0221)	(0.0269)	(0.0182)	(0.0443)
Linear Flexible Slope	-0.0006	-0.0408*	0.0537**	0.0153	0.0036
	(0.0040)	(0.0219)	(0.0265)	(0.0181)	(0.0439)
Quadratic	-0.0008	-0.0431**	0.0578**	0.0167	-0.0007
	(0.0040)	(0.0218)	(0.0269)	(0.0183)	(0.0440)
Quadratic Flexible Slope	0.0010	-0.0462	0.0432	0.0027	-0.0171
	(0.0053)	(0.0283)	(0.0350)	(0.0216)	(0.0554)
N	952	952	952	952	897
outcome mean	0.0062	0.3225	0.4949	0.8236	4.1252
Panel B: women					
Linear	-0.0043	-0.0210	0.0243	-0.0006	-0.0285
	(0.0033)	(0.0168)	(0.0288)	(0.0340)	(0.0678)
Linear Flexible Slope	-0.0046	-0.0206	0.0183	-0.0063	-0.0286
	(0.0032)	(0.0167)	(0.0284)	(0.0335)	(0.0670)
Quadratic	-0.0047	-0.0201	0.0120	-0.0120	-0.0322
	(0.0031)	(0.0167)	(0.0284)	(0.0335)	(0.0680)
Quadratic Flexible Slope	-0.0073*	-0.0300	0.0336	-0.0009	-0.1130
	(0.0041)	(0.0216)	(0.0351)	(0.0398)	(0.0927)
N	952	952	952	952	576
outcome mean	0.0042	0.1275	0.2315	0.3632	3.6489

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors for clustering at district level in parentheses. An observation is a district in a given season. An employment outcome is the proportion of working-age adults (18-60 years) with at most secondary education in rural areas working in a given type of employment in the last 7 days. Parametric regressions with different levels of flexibility are reported. The log private wage in column 5 is conditional on private employment. Observations with a state-specific rank between -1 and 1 are dropped.

Table C4: NREGS impact (index running variable): wages and employment (men and women)

Specification	employment public	 private	 family	 total	log private wage
Panel A: men					
Linear	0.0015	-0.0135	0.0060	-0.0041	-0.0334
	(0.0032)	(0.0174)	(0.0201)	(0.0151)	(0.0324)
Linear Flexible Slope	0.0007	-0.0145	0.0123	0.0005	-0.0311
	(0.0033)	(0.0172)	(0.0197)	(0.0155)	(0.0329)
Quadratic	0.0007	-0.0201	0.0230	0.0055	-0.0324
	(0.0037)	(0.0181)	(0.0213)	(0.0163)	(0.0356)
Quadratic Flexible Slope	0.0000	-0.0353**	0.0338	-0.0183	-0.0044
	(0.0045)	(0.0178)	(0.0250)	(0.0176)	(0.0383)
N	1063	1063	1063	1063	1007
outcome mean	0.0069	0.3279	0.4846	0.8195	4.1212
Panel B: women					
Linear	0.0023	-0.0077	0.0231	0.0164	-0.0379
	(0.0048)	(0.0132)	(0.0221)	(0.0251)	(0.0551)
Linear Flexible Slope	0.0021	-0.0046	0.0284	0.0248	-0.0384
	(0.0050)	(0.0132)	(0.0216)	(0.0243)	(0.0550)
Quadratic	0.0018	0.0012	0.0482**	0.0504*	-0.0412
	(0.0053)	(0.0141)	(0.0222)	(0.0257)	(0.0581)
Quadratic Flexible Slope	0.0006	-0.0191	0.0417*	0.0248	-0.0544
	(0.0057)	(0.0138)	(0.0253)	(0.0294)	(0.0708)
N	1063	1063	1063	1063	656
outcome mean	0.0053	0.1309	0.2285	0.3647	3.6488

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors for clustering at district level in parentheses. An observation is a district in a given season. An employment outcome is the proportion of working-age adults (18-60 years) with at most secondary education in rural areas working in a given type of employment in the last 7 days. Parametric regressions with different levels of flexibility are reported. The log private wage in column 5 is conditional on private employment. NREGS is the predicted treatment status. The running variable is the normalized state-specific poverty index value of a district rather than its rank.

Table C5: NREGS impact (individual level): wages and employment (men and women)

Specification	employment public	 private	 family	 total	log private wage
Panel A: men					
Linear	-0.0025	-0.0286	0.0341	0.0031	0.0472
	((0.0039))	(0.0208)	(0.0253)	(0.0185)	(0.0417)
Linear Flexible Slope	-0.0024	-0.0286	0.0339	0.0028	0.0468
	(0.0039)	(0.0209)	(0.0250)	(0.0182)	(0.0416)
Quadratic	-0.0031	-0.0296	0.0391	0.0065	0.0454
	(0.0040)	(0.0205)	(0.0251)	(0.0184)	(0.0418)
Quadratic Flexible Slope	-0.0031	-0.0531**	0.0595*	0.0033	0.0441
	(0.0056)	(0.0252)	(0.0320)	(0.0211)	(0.0480)
N	37224	37224	37224	37224	12062
outcome mean	0.0082	0.3261	0.4756	0.8099	4.0473
Panel B: women					
Linear	0.0009	-0.0025	0.0254	0.0238	-0.0231
	(0.0036)	(0.0171)	(0.0251)	(0.0296)	(0.0528)
Linear Flexible Slope	0.0010	-0.0032	0.0274	0.0252	-0.0220
	(0.0035)	(0.0172)	(0.0250)	(0.0295)	(0.0537)
Quadratic	0.0008	-0.0015	0.0199	0.0192	-0.0257
	(0.0036)	(0.0172)	(0.0248)	(0.0295)	(0.0532)
Quadratic Flexible Slope	-0.0027	-0.0125	0.0409	0.0257	-0.0585
	(0.0041)	(0.0206)	(0.0327)	(0.0381)	(0.0606)
N	41978	41978	41978	41978	5339
outcome mean	0.0046	0.1234	0.2106	0.3385	3.5428

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors for clustering at district level in parentheses. An observation is a working-age adult (18-60 years) with at most secondary education living in rural areas. The employment outcome variables are indicator variables for having worked in a given employment type in the last 7 days. Parametric regressions with different levels of flexibility are reported. The log private wage in column 5 is conditional on private employment.

Table C6: Seasonality of wages and employment (men and women)

Specification	public	employment private	family	total	log private wage
Panel A: men					
Linear	-0.0025	-0.0392*	0.0285	-0.0115	-0.0249
	(0.0036)	(0.0229)	(0.0266)	(0.0190)	(0.0416)
NREGS*dry season	0.0073	0.0081	-0.0060	0.0091	0.0404
	(0.0053)	(0.0190)	(0.0208)	(0.0155)	(0.0367)
Linear Flexible Slope	-0.0025	-0.0424*	0.0346	-0.0090	-0.0336
	(0.0034)	(0.0237)	(0.0274)	(0.0194)	(0.0442)
NREGS*dry season	0.0071	0.0144	-0.0176	0.0045	0.0577
	(0.0066)	(0.0235)	(0.0257)	(0.0209)	(0.0463)
Quadratic	-0.0030	-0.0410	0.0324	-0.0100	-0.0283
	(0.0036)	(0.0224)	(0.0262)	(0.0192)	(0.0415)
NREGS*dry season	0.0073	0.0081	-0.0061	0.0090	0.0408
	(0.0053)	(0.0190)	(0.0208)	(0.0155)	(0.0367)
Quadratic Flexible Slope	-0.0004	-0.0654**	0.0470	-0.0148	-0.0378
	(0.0042)	(0.0296)	(0.0357)	(0.0242)	(0.0583)
NREGS*dry season	0.0042	0.0261	-0.0328	-0.0034	0.0335
	(0.0065)	(0.0262)	(0.0297)	(0.0279)	(0.0591)
N	1063	1063	1063	1063	1007
outcome mean	0.0069	0.3279	0.4846	0.8195	4.1212
Panel B: women					
Linear	-0.0017	-0.0121	0.0163	0.0015	0.0923
	(0.0033)	(0.0177)	(0.0283)	(0.0325)	(0.0730)
NREGS*dry season	0.0060	0.0170	0.0012	0.0254	-0.1726***
	(0.0047)	(0.0136)	(0.0186)	(0.0205)	(0.0592)
Linear Flexible Slope	-0.0024	-0.0170	0.0181	-0.0027	0.0845
	(0.0031)	(0.0187)	(0.0294)	(0.0335)	(0.0760)
NREGS*dry season	0.0075	0.0271	-0.0033	0.0331	-0.1569**
	(0.0057)	(0.0170)	(0.0233)	(0.0259)	(0.0724)
Quadratic	-0.0016	-0.0106	0.0105	-0.0024	0.0932
	(0.0034)	(0.0176)	(0.0279)	(0.0321)	(0.0729)
NREGS*dry season	0.0060	0.0170	0.0012	0.0254	-0.1726***
	(0.0047)	(0.0136)	(0.0186)	(0.0205)	(0.0593)
Quadratic Flexible Slope	-0.0025	-0.0253	0.0334	0.0070	0.0159
	(0.0031)	(0.0234)	(0.0378)	(0.0431)	(0.1004)
NREGS*dry season	-0.0001	0.0358	0.0017	0.0387	-0.1668**
	(0.0046)	(0.0195)	(0.0278)	(0.0310)	(0.0838)
N	1063	1063	1063	1063	656
outcome mean	0.0053	0.1309	0.2285	0.3647	3.6488

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors for clustering at district level in parentheses. Parametric regressions with different levels of flexibility are reported. The log private wage in column 5 is conditional on private employment. NREGS is the predicted treatment status. July through December roughly correspond to harvesting and planting seasons for most crops in wide parts of the country and are treated as the agricultural main season. January through June are the agricultural off-season or dry season.

Table C7: NREGS impact: Difference-in-Difference Estimates (men and women)

	employment				log private
	public	private	family	total	wage
Panel A: men					
Actual Treatment					
NREGS*post period	0.0083**	0.0060	-0.0344**	-0.0201	0.0100
	(0.0036)	(0.0160)	(0.0173)	(0.0146)	(0.0297)
NREGS	0.0019	-0.0019	0.0319**	0.0317**	-0.0741***
	(0.0018)	(0.0122)	(0.0137)	(0.0135)	(0.0297)
post period	0.0014	0.0147	-0.0555***	-0.0394***	0.0832***
	(0.0009)	(0.0103)	(0.0103)	(0.0101)	(0.0179)
Predicted Treatment					
NREGS*post period	0.0056*	0.0141	-0.0405**	-0.0207	-0.0075
	(0.0031)	(0.0159)	(0.0165)	(0.0144)	(0.0289)
NREGS	-0.0022	-0.0192	0.0404***	0.0190	-0.0664**
	(0.0016)	(0.0121)	(0.0142)	(0.0143)	(0.0283)
post period	0.0022	0.0114	-0.0523***	-0.0387***	0.0900***
	(0.0016)	(0.0104)	(0.0110)	(0.0106)	(0.0188)
N	2126	2126	2126	2126	2014
outcome mean	0.0047	0.3194	0.5188	0.8429	4.08
Panel B: women					
Actual Treatment					
NREGS*post period	0.0075**	0.0035	0.0049	0.0159	-0.0126
	(0.0035)	(0.0109)	(0.0174)	(0.0200)	(0.0461)
NREGS	0.0028	0.0115	-0.0167	-0.0023	-0.0458
	(0.0019)	(0.0102)	(0.0186)	(0.0218)	(0.0369)
post period	0.0007	-0.0104	-0.0793***	-0.0890***	-0.0058
	(0.0005)	(0.0064)	(0.0119)	(0.0131)	(0.0288)
Predicted Treatment					
NREGS*post period	0.0043	0.0073	0.0159	0.0275	-0.0249
	(0.0031)	(0.0104)	(0.0173)	(0.0194)	(0.0451)
NREGS	-0.0001	0.0176*	0.0073	0.0248	-0.1013***
	(0.0014)	(0.0099)	(0.0198)	(0.0224)	(0.0358)
post period	0.0018	-0.0119*	-0.0837***	-0.0939***	0.0004
	(0.0012)	(0.0069)	(0.0122)	(0.0137)	(0.0305)
N	2126	2126	2126	2126	1312
outcome mean	0.0036	0.1354	0.2672	0.4062	3.64

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors for clustering at district level in parentheses. An observation is a district in a given season. An employment outcome is the proportion of working-age adults (18-60 years) with at most secondary education in rural areas working in a given type of employment in the last 7 days. The log private wage in column 5 is conditional on private employment. NREGS is the actual or the predicted treatment status of a district.

Figure C.11: Public employment men

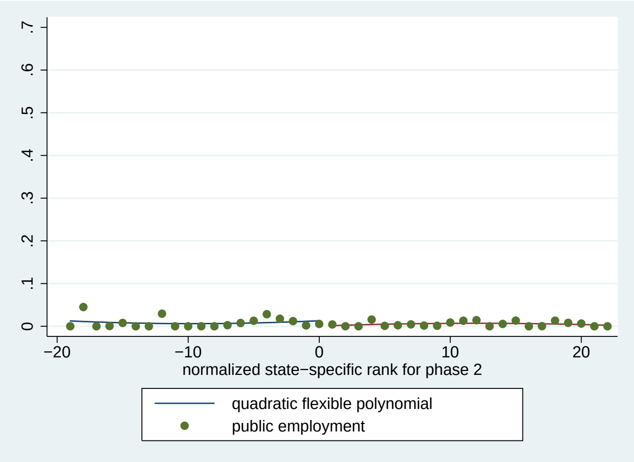


Figure C.12: Private employment men

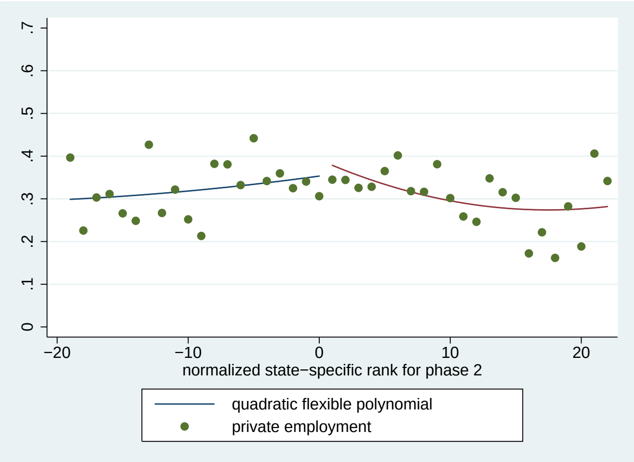
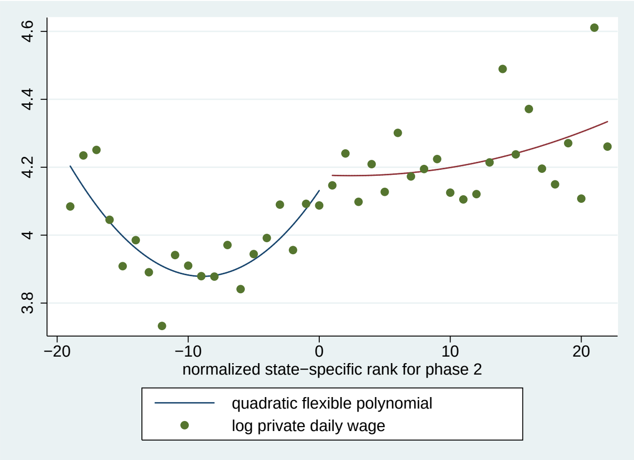


Figure C.13: Log daily private wage men



Note: An observation is the average outcome at a given rank, so the bin size is 1. Fitted curves are quadratic polynomials.

Figure C.14: Public employment women

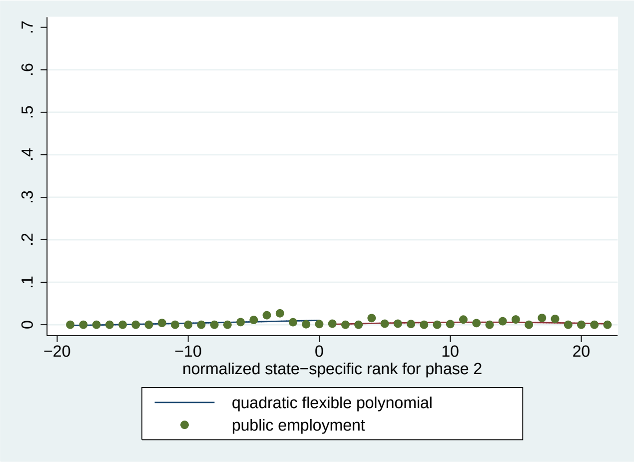


Figure C.15: Private employment women

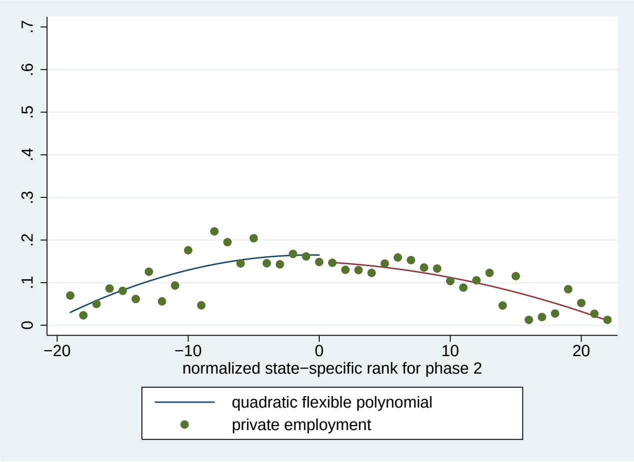
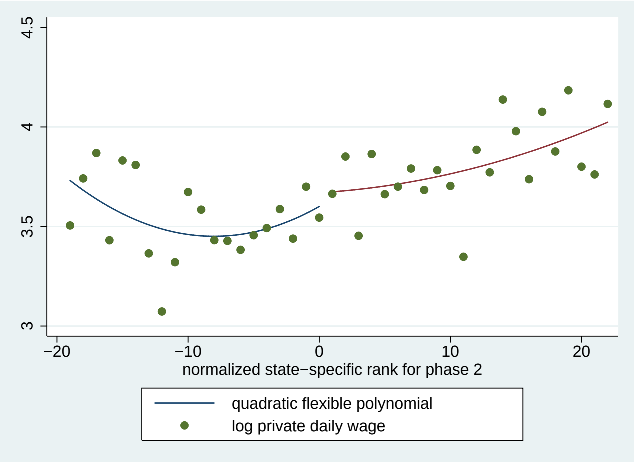
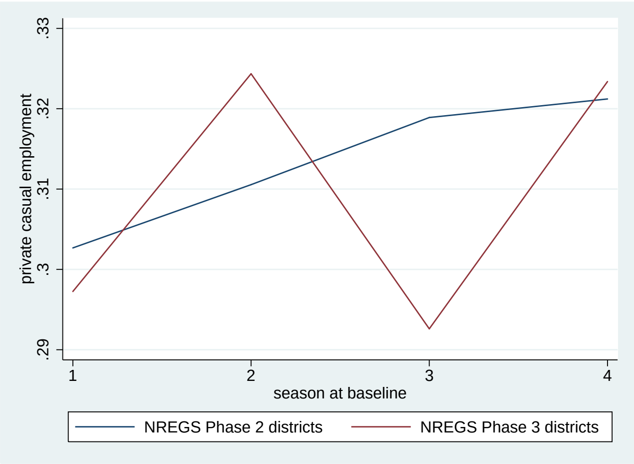


Figure C.16: Log daily private wage women



Note: An observation is the average outcome at a given rank, so the bin size is 1. Fitted curves are quadratic polynomials.

Figure C.17: Private employment trends for men at baseline



Note: Figure plots average casual private-sector employment in Phase 2 and Phase 3 districts for each sub-round in the baseline data (July 2004-June 2005).

D RD versus DID estimates

As Figure C.17 shows, the parallel trend assumption underlying the difference-in-difference approach is violated since Phase 2 and Phase 3 districts have differential trends in casual private employment at baseline. This may lead to substantial bias in the program impacts. Additionally, difference-in-difference specifications estimate the average treatment effect across all observations rather than the treatment effect for districts close to the treatment cutoff as in the regression discontinuity design. Appendix Table C7 estimates the labor market effects for the sample used for the regression discontinuity analysis using a difference-in-difference approach. Panels A and B report the results using actual treatment as well as predicted treatment status from the algorithm for men and women, respectively. The table shows that in my sample even the difference-in-difference analysis does not generate substantial wage increases. Public-sector employment is statiscally significantly higher in NREGS districts after the program is introduced, although the estimates are again modest relative to the proposed scale of the scheme. Somewhat surprisingly the coefficients for private employment and family work for men are flipped in Table C7 compared to the regression discontinuity results in Table 4.

Overall, Table C7 shows that the different results found in this paper are not just due to the choice of the empirical specification. Theoretically, such differences could be driven by differences in the sample composition, the data used, or the chosen specifications. Two impact evaluation papers using DID approaches, Azam (2012) and Imbert and Papp (2013), use almost the same data that is used in this paper, so the data itself is unlikely to be the issue. A replication of the DID results of these two papers (not reported here) allows me to analyze what drives the differences. The results reveal that the choice of the empirical specification seems to be the most important explanation for the differences between Table C7 and the results in Imbert and Papp (2013), although they also use an additional dataset to increase their sample size that improves the precision of the estimates: Their main specification directly estimates the seasonal impacts of NREGS and demonstrates that the impacts are concentrated during the agricultural off-season, whereas my paper focuses on the overall impact of the program.[34] A similar reason explains the wage increases found in the Berg et al. (2012) paper that uses a different dataset on wages than this paper. Their paper, too, does not find important short-run wage increases in the general sample, but documents that wage increases exist in areas with high implementation quality.

The differences between Table C7 and the results in Azam (2012), on the other hand, seem to be mostly due to the choice of the sample: Azam (2012) restricts the sample of casual workers rather than the general working-age population considered in this paper, which explains the higher public employment impacts since NREGS employment should be especially attractive for this group. The wage increases documented for this group of workers is present at the individual level, but disappears once wage impacts are estimated at the district level. Given that who remains a casual worker after the introduction of NREGS may itself be an endogenous decision affected by the program, it is also difficult to generalize these results.

[34]Estimating the seasonal impact of NREGS with the RD specification for men shows that the results are broadly consistent with their results, although the interaction effects in my specification are statistically insignficant. The RD results still do not show any evidence of wage increases even in the agricultural off-season, however, and the interaction effect for women is even statistically significantly negative. The seasonality results are reported in appendix table C6.