Labor Market Impacts of a Large-Scale Public Works Program: Evidence from the Indian Employment Guarantee Scheme

Laura Zimmermann *
University of Michigan

March 2013

Abstract

Recent years have seen an increasing interest in using public-works programs as anti-poverty measures in developing countries. This paper analyzes the rural labor market impacts of the Indian National Rural Employment Guarantee Scheme, one of the most ambitious programs of its kind, by using a regression discontinuity design. I find that the overall effect of the program on employment and wages is likely to be small, but that the scheme may provide a safety net for rural households living in areas with high income volatility.

JEL: H53, I38, J22, J23, J38

Keywords: public works program, National Rural Employment Guarantee Scheme, NREGA, NREGS, India, regression discontinuity design

*email:lvzimmer@umich.edu. I thank Manuela Angelucci, Raj Arunachalam, Arnab Basu, Gaurav Khanna, David Lam, Jeff Smith, Dean Yang, and participants of the University of Michigan Informal Development Seminar, the University of Michigan Labor Lunch, and the University of Michigan Summer Seminar for valuable comments, feedback and suggestions. Abhiroop Mukhopadhyay was of major help with data-related questions.

1 Introduction

Large-scale public works programs have fallen out of favor as effective active labor market policies in developed countries. Such government programs typically prove unable to raise workers' human capital, and are in danger of crowding out private-sector jobs.[1] In developing countries, on the other hand, recent years have seen a resurgence of interest in government employment programs and employment guarantee schemes, although with different intentions: Public-works programs are increasingly seen as a potential silver bullet for reducing poverty by functioning as conditional cash transfer programs.

Rural labor markets in developing countries often lack adequate employment opportunities during the agricultural off-season, and credit markets are still absent or incomplete in many areas. This means that the rural poor face severe challenges of unemployment, underemployment and lack of access to credit, making it difficult for households to smooth consumption. At the same time, standard government programs meant to target this population are often hampered by incomplete information on eligible households, corrupt government officials, and sluggish and over-centralized implementation structures. In this situation, government public works programs are seen as a potential solution: They allow households to decide if and when to sign up for manual labor work, which increases program flexibility, allows the tailoring of the program to local conditions, and abolishes the need for other formal eligibility criteria. (see e.g. Lal et al. 2010, Subbarao 1997).

We still know relatively little about how well such programs work in developing countries and about how they affect the workings of the rural labor markets, however.[2]

[1]For an overview of active labor market policies see e.g. Kluve (2010) for Europe.

[2]See e.g. Basu (2011), 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.

This paper contributes to a growing literature of empirically well-identified research on the labor market impacts of public works programs in developing countries by analyzing the impact of one of the largest and most ambitious public works programs in the developing world, the Indian National Rural Employment Guarantee Scheme (NREGS)[3]: NREGS is based on the National Rural Employment Guarantee Act (NREGA) passed in the Indian parliament in 2005. The act provides a legal guarantee of up to 100 days of public-sector employment per year for each rural household, which sets this program apart from most other government schemes across the world that lack such guarantees. Additionally, NREGS is a very large program since it applies to all rural households (or about 70 percent of the Indian population). This is also reflected in high annual expenditures on the scheme which are typically around 1 percent of Indian GDP.

NREGS was phased in across India between 2006 and 2008 in a non-random way, with poor districts receiving the program earlier than richer districts according to an existing development index. While the exact algorithm that the Indian government used to assign treatment status to districts is not publicly available, information from official government documents on this and earlier programs is used to construct an algorithm that is arguably very close to the government algorithm. The algorithm is a two-step process which first assigns numbers of treatment districts to Indian states according to the proportion of India's poor living in a given state, and then selects the least developed districts within that state based on a development index. This procedure generates state-specific cutoffs in treatment and allows the use of a regression discontinuity framework. The impacts of NREGS on rural labor markets can then be analyzed by using nationally representative household survey data at a time when the scheme had not yet been rolled out to all rural districts.

[3]The program is also often referred to as NREGA (National Rural Employment Guarantee Act) since the act provides the legal basis of the program. The program was officially renamed to Mahatma Gandhi National Rural Employment Guarantee Scheme in 2009 but the original name continues to be used in many academic and public policy debates.

The results suggest that NREGS has very limited labor market impacts for both men and women: The program does not provide substantial public-sector employment opportunities and in general does not raise private-sector casual wages. There is some evidence of men leaving the private casual sector for family work, however, which is consistent with NREGS altering the relative riskiness of self-employment. The empirical analysis also shows that NREGS employment and private wages are significantly higher in districts with high rainfall volatility, further supporting the interpretation that NREGS provides a safety net for rural households.

The rest of this paper is structured as follows: Section 2 provides some background on the characteristics of NREGS. Section 3 sets up some hypotheses regarding the impact of the program based on a simple conceptual framework. 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 used and the empirical specifications. Section 6 presents the main results and some extensions. Section 7 concludes.

2 Background

2.1 Theoretical Program Characteristics

The National Rural Employment Guarantee Scheme (NREGS)[4] 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[6] at the minimum wage. There are no formal eligibility rules other than that the household lives in a rural area and their

[4]The program was renamed to Mahatma Gandhi National Rural Employment Guarantee Scheme in 2009.

[5]For more details on the scheme see e.g. Dey et al. (2006), Government of India (2009), and Ministry of Rural Development (2010).

[6]The year is the financial year and starts on April 1.

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 is required to be female.

NREGS projects focus mainly on the improvement of local infrastructure and on anti-drought 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. At the introduction of the scheme, this floor wage was Rs 60 per day. It has been raised over time, and was Rs 120 per day in 2009. 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 paid on a weekly basis.

2.2 Empirical Working 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 benefits for the poor: A number of studies document, for example, that NREGS seems to be successful in providing employment during the agricultural off-season and after idiosyncratic shocks, allowing households to earn additional income and increase their food and durable expenditures (see e.g. Centre for Science and Environment 2008, Institute of Applied Manpower 2007, NCAER-PIF 2009).

Some papers specifically stress the benefits NREGS provides for women. Khera and Nayak (2009), for example, use a survey of 1060 NREGS workers in six North-Indian states to study the impact the scheme has had on women, and conclude that NREGS has improved women's access to jobs with reasonable wages and working conditions.

Narayan (2008) and Jeyaranjan (2011) document similar benefits for women in their case studies in Tamil Nadu.

While these studies suggest substantial benefits for the rural poor in India, they 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 (Centre for Science and Environment 2008, Institute of Applied Manpower 2007, Khera 2009, NCAER-PIF 2009, Samarthan Centre for Development Support 2007). 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 (Dutta et al. 2012, Institute of Applied Manpower 2007, NCAER-PIF 2009).

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. Johnson (2009b) finds that the working of NREGS in Andhra Pradesh does not seem to be strongly affected by the parties in power at the local level, suggesting that the program is not vulnerable to extensive political pressures in this state. Johnson et al. (2009) also provide a detailed descriptive overview of the working of the program in Andhra Pradesh on the basis of administrative data for the state.

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 (2011a and 2011b) 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.

An increasing number of papers in economics also analyzes the impact of the program on rural labor markets. Imbert and Papp (2012) 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, but that it takes between 6 and 11 months 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 assumption is unlikely to hold, however, which could substantially affect their results. The regression discontinuity approach used in this paper, on the other hand, does not require such an assumption and therefore provides cleaner empirical identification of the impacts of NREGS. The results document no real evidence for substantial wage increases, although the time frame of the analysis would be consistent with Berg et al.'s finding that wage effects may take some time to take effect.

To fix ideas about the expected impacts of the scheme, it is useful to set up a simple theoretical framework.

3 A Simple Conceptual Framework

Consider a household making decisions over an infinite time horizon. In the current period, which can be intuitively thought of as an agricultural year, a household chooses how much time to work in family employment on the own farm and how much time to spend working for the landlord in the village as agricultural laborers.[7] After the employment decisions are made, an income shock (such as a rainfall shock or shock to output prices) is realized that determines the payoff from self-employment from the previous period. A household derives utility from the proportion of time spent working on the own farm (1 - l) and from the earned income. The wage earned in the private casual sector is constant.[8]

A household maximizes the expected lifetime utility given by

$$V(y) = \max_{l}[u(1-l) + u((1-l_{-1})y + l_{-1}w) + \beta EV(y')]$$

where $u' > 0, u'' < 0, u''' > 0$. l is the fraction of time spent working for the big

[7]In the NSS data used for my empirical specification, 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. These stylized facts suggest that at least a large subset of rural Indian households has an alternative to working in the private casual sector.

[8]This 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, which does not seem like a strong assumption.

landowner, y is self-employment income and w is the private-sector casual wage.[9]

Assuming that an interior solution exists and solving for the first-order condition leads to the equation

$$\frac{u'(1-l)}{\beta} = \int u'((1-l)y + lw)(w-y)f(y)dy \tag{1}$$

which implicitly pins down the optimal proportion of time l spent working in the private sector.

A priori, the introduction of NREGS may have three impacts: First, the scheme could increase competition in the rural labor market by introducing the government as an additional employer. Second and closely related, NREGS could practically enforce existing minimum wage laws and thereby raise the equilibrium private-sector wage. And third, NREGS may reduce income volatility by introducing a safety net for rural households.

In general, we would expect both the enforcement of the minimum wage and higher competitiveness in the labor market to lead to an increase in the private-sector wage, whereas employment changes depend on the structure of the labor market and would require a model of aggregate labor supply and labor demand in addition to the individual household optimization setup outlined above. There is no consensus on the structure of the typical Indian rural labor market in the literature, however. The models in Basu (2002) and Basu (2011), 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

[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. Considering that one period in this framework is thought of as an agricultural year, which includes peak times like planting and harvesting, this may not be a very strong assumption. 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.

of India, other papers like Kaur (2012) argue that daily labor contracts are the norm in Indian rural labor markets. Imbert and Papp (2012) focus heavily on small farmers with simultaneous labor supply and demand decisions without saying much about how common this type of workers is relative to the number of landless laborers. As my empirical analysis will show, I find no evidence for large-scale public-sector employment or private-sector wage increases due to NREGS, which rule out substantial effects due to the addition of an employer in rural areas or a better enforcement of the minimum wage laws. I therefore do not model the labor market structure in more detail at this point.

Ruling out the first two potential NREGS effects implies that the big landowner's profit-maximizing wage and number of hired workers should be unaffected by the introduction of the employment guarantee scheme. Similarly, the actual changes in the labor market should be negligible for a worker if there is not much NREGS employment and the private-sector wage does not increase. Workers may still adjust their labor supply decisions, however, if NREGS is perceived to reduce the relative riskiness of self-employment, for example by providing an ex post insurance against bad income shocks. In terms of the household utility maximization framework above, this means that the new distribution of y first-order stochastically dominates the old one. In equation (1), this will lower marginal utility u'(.) for given values and thereby decrease the optimal fraction of time spent working in the private sector. Private-sector employment should therefore decrease.

The conceptual framework also predicts that we should expect differential responses based on the volatility of income. Suppose the income volatility in district A second-order stochastically dominates that in district B. This means that the distribution of self-employment income y will be more volatile in district B, which increases marginal utility u'(.) for given values and leads to an increase in the optimal value of l in (1).

This means that households living in riskier districts should be more likely to work in the private sector than those living in less risky districts at baseline. Additionally, these households are more likely to have experienced a bad income shock, which should lead them to demand more NREGS employment. Since the maximum amount a household can earn under NREGS is capped at a 100 times the minimum wage, NREGS may or may not provide full insurance for a given negative income shock. If negative income shocks are very large relative to NREGS money, full insurance is not provided and households in risky districts may not be substantially more likely to leave private-sector employment than households in less risky environments. These hypotheses will be tested empirically.

4 Program Rollout and Regression Discontinuity Design

4.1 Program Timeline and Details of the Rollout

The National Rural Employment Guarantee Act (NREGA), which forms the legal basis for NREGS, was passed in the Indian Parliament in August 2005 and lays down the characteristics of the program and the entitlements of workers under the employment guarantee scheme. 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. This means that since 2008 the scheme operates in 99 percent of Indian districts (the scheme excludes districts with a 100 percent urban population) (Ministry of Rural Development 2010). I will refer to the district phases as Phase 1, Phase 2, and Phase 3, respectively. Taking into account the rollout timing is important for the empirical identification strategy.

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.[10] 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.[11] 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

[10] In practice this provision also ensures that all states (union territories are usually excluded from such programs) receive at least one treatment district.

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

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.[12]

According to this proposed algorithm, a number of eligible districts was therefore allocated to states first in a given phase of the NREGS rollout, and districts within states were then supposed to be chosen based on an existing development ranking.[13] 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).[14] 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 used in pre-NREGS district initiatives, although 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 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

[12]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.

[13]NREGS Phase 1 allocation assignment had an additional feature: In that phase, all districts on an existing list of districts strongly affected by left-wing terrorism received NREGS regardless of their rank. This means that in the within-state allocations, terrorist-affected districts were prioritized. I drop all of these districts in my empirical analysis.

[14]More details are provided in the next section.

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[15], 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.

Table 1 gives an overview of how well the proposed 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.

[15]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.

[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.

Table 1 shows that the overall prediction success rate of the proposed algorithm is about 83 percent in Phase 1 and about 82 percent in Phase 2[17], so there is some slippage in treatment assignment in both phases. The table also reveals that there is considerable heterogeneity in the performance of the algorithm across states, but that the algorithm performs quite well in almost all of the 17 states. This is especially true for the Phase 2 district assignment which will be used in the empirical analysis, since the Phase 1 information cannot be exploited to evaluate the impact of NREGS in an RD design because of the timing of the available data.

While Table 1 therefore suggests that the proposed algorithm works quite well for predicting Phase 1 and Phase 2 district allocations, there are a number of potential problems. These are discussed in the next section.

4.2 Regression Discontinuity Design

Given that the proposed algorithm of NREGS treatment assignment has a two-step procedure, where the number of eligible districts is allocated to states first, and districts are then chosen within a state to fill up this number of slots based on their rank, 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

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

this paper.[18]

Treatment cutoffs differ by state, so for the empirical analysis ranks are made state-specific and are normalized 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. This makes it easy to pool the data across states to estimate the treatment effect at a 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 according to the district ranking and will function as control districts in my empirical analysis.

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. 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.

Figure 1 reports observations based on the predicted NREGS receipt of the proposed 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 in these two different types of districts are solely attributable to the introduction of the unemployment guarantee scheme.

[18] This other cutoff tests the time pattern of NREGS receipt, that is whether the length of time that a district has had the program matters for labor market outcomes. The power to detect statistically significant effects is likely to be much lower in this case since both types of districts have had access to NREGS at the time of the data collection.

In order for the RD design to be valid, districts must have imperfect control over their treatment status in a given phase (Lee 2008). In this specific case, this implies that states and districts should not have been able to manipulate either the index variable used to rank districts, or the criterion used to determine the number of districts that were allocated to a specific state.[19] 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.

That states or districts were able to manipulate the index variable values 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 for districts or states 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 very transparent way. The Planning Commission report outlines the exact procedure with which the index was created, and also lists the raw data for all districts, so that the composite index as well as the district ranking can be perfectly replicated. This implies that there was no room for manipulation at the level of the creation of the district rank variable.

[19] 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 adapted since 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.

Figures 2 and 3 look more closely at the distribution of index values over ranks. Ideally, we would like the assignment variable to be continuous at the cutoff, since discontinuities at the cutoffs are typically taken as signs of potential manipulation (McCrary 2008). 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 big concern.

Manipulation of the criterion used in the proposed algorithm for the allocation of treatment districts across states also seems unlikely: As outlined above, 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 state rural population to calculate the poverty prevalence measures, which was widespread publicly-available information at the time. Again, it therefore was probably impossible for Indian states or districts to exert political influence on the treatment status of individual districts by manipulating the data used in the assignment algorithm.

Given that I do not have access to the actual poverty-prevalence measure used in the algorithm, my proposed algorithm introduces measurement error into predicted treatment status if the Indian government used a different poverty criterion or a different source of estimates for 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 guess estimates and a plausible criterion based on institutional knowledge that could not easily be manipulated by stakeholders.

Overall, it therefore seems plausible that Indian states and districts could not manipulate the algorithm to take influence on individual districts' NREGS treatment status. 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 significantly lower than 100 percent.[20]

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 discontinuity. This suggests that there is indeed a discontinuity in the probability of being treated with the employment guarantee scheme at the cutoff. 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.

[20]It can be shown that deviations from the proposed algorithm are correlated with the party affiliation of members of parliament from the same district. 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.

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 Organisation (NSSO). 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.[21] 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.[23] I will exploit this feature of the

[21]Unfortunately, there is no NSS survey data available that was collected at a time when NREGS was only operating in Phase 1 districts and that has the large sample size needed for district-level analysis. The NSSO alternates between 'thick rounds' with large sample sizes like the 64th round, and 'thin rounds' that have a much smaller sample size.

[22]See e.g. Imbert and Papp (2012). The results reported in this paper are qualitatively the same when these potentially contaminated control group observations are excluded.

[23]While the panel is not perfectly balanced in practice, most districts are interviewed in every season.

data empirically by aggregating individual-level information up to the district level for each sub-round separately.[24] In addition to increasing sample size, this has the added advantage that it is possible to look at seasonal differences in the impact of NREGS on wages and employment.

Consistent with other NREGS papers, I restrict my sample to individuals of prime age (18-60) 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 at all in a job of a given characteristic (public or private casual wage employment, which excludes unpaid employment on the family farm or in a family business) in the past 7 days, 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. In a typical RD design, baseline information is not necessary for empirical identification, but controlling for baseline characteristics can increase the precision of the estimates by reducing residual variance. This is especially true in this case: NREGS cutoffs are based on district rankings using information from the early to mid-1990s, whereas the first districts received the program in 2006. Since districts close to the cutoff on either side in the early 1990s are likely to have been more similar to each other at the time the raw data was collected than they are 15 years later, controlling for baseline information of the outcome variables will help to control for differential district development during the 1990s and early 2000s. Results are qualitatively similar when baseline variables are

[24]This feature of the data has also been exploited in other NREGS papers like Imbert and Papp (2012).

excluded.

5.2 Empirical Specification

I control for the lagged outcome variables by including the baseline outcome variable as an additional regressor.[25] As the number of observations near the cutoff is limited, I use parametric regressions to estimate the impact of NREGS empirically.[26] To test the robustness of the estimates, all main result tables show the estimated coefficients for linear and quadratic regression curves in the running variable with and without constraining the slope of the curves to be the same on either side of the cutoff.[27] I also drop Phase 1 districts to ensure that the parametric estimates of the treatment effects are not affected by districts that are far away from the cutoff. It is important to note that since the RD design depends on observations being close to the cutoff for identification it is impossible to compare Phase 1 and Phase 3 districts since these will be far apart from each other by design. In contrast to difference-in-difference estimations, the RD design therefore cannot estimate the impact of having had the program for up to two years compared to the control group.

The equation below shows the regression equation for the most flexible specification with a quadratic regression curve with the slope not constrained to be identical on both sides of the cutoff:

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

[25] An alternative to this is to have the outcome variable be the difference between post- and pre-NREGS time periods, but this option is nested in the other specification since this option constrains the coefficient on the baseline outcome variable to be 1.

[26] This is in line with the advice in Lee and Lemieux (2009) in such a situation.

[27] F-tests 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. The quadratic flexible specification only statistically outperforms the linear flexible specification in some instances.

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 index, and η are state fixed effects.[28]

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 Phase 2 according to the state-specific algorithm, and zero otherwise. Corresponding appendix tables report 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.[29] Results are reported for men and women separately.

5.3 Summary Statistics

Table 2 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 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 wages are about Rs.66 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

[28]Including sub-round fixed effects often makes the first stage estimation highly unstable since a number of employment outcomes are highly correlated with specific seasons. I therefore don't include season-fixed effects, although including them has no big impacts on the second-stage results.

[29]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.

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 2 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 3 through 5 present the main results of the impact of NREGS for men and women separately. The figures show the regression discontinuity 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 private-sector daily casual wage earned in the past week. The outcome variables are the residuals of a regression of the outcome variable of interest on the baseline outcome variable and state fixed effects to make the graphs comparable to the results in the tables although, in contrast to the table results, standard errors here are not clustered at the district level. One scatter point represents the average residual district outcome value in a given season. The regression lines are quadratic in the rank variable, with the slope allowed to differ between the two sides of the cutoff. The graphs also show the 95 percent confidence intervals. With the excpetion of Figure 6, none of the figures show a statistically significant discontinuity in public or private employment or in the earned private wage at the cutoff value 0, suggesting that the impacts of NREGS on the Indian rural labor market are limited. Figure 6, on the other hand, suggests that private employment for men in NREGS districts is significantly lower than that in conrol districts.

Tables 3 to 5 focus on different empirical specifications of the RD design in more detail. All estimated models control for the baseline outcome variable and include state fixed effects. Whereas these tables present the intent-to-treat effects, appendix tables A1 and A2 show the corresponding IV results, where actual NREGS receipt is instrumented with predicted NREGS treatment status. Those tables also report the F-statistic of the first stage of the two-stage least squares models. In all tables, one observation is a given district in a specific season.

Tables 3 and 4 show the main results for men and women, respectively. Each row presents the impact of NREGS on the outcome variables of interest for a different parametric functional form of the running variables. Table 3 looks at the estimates for men and column 1 demonstrates again that NREGS does not seem to have a large impact on public-sector casual employment, the main outcome that we would expect to be majorly affected by the introduction of a large-scale employment guarantee scheme. The typical estimate is positive but small in absolute value and statistically insignificant. The coefficient estimate in the first row of column 1, for example, suggests that being in a NREGS district increases a rural prime-aged men'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 Table 3 reveals that NREGS may have some impact on male private casual employment. The estimated coefficients are negative and are all statistically significant at the 10 percent level. They suggest that NREGS lowers private-sector casual employment for men by about 4 percentage points across specifications, which translates into a substantial percentage change of about 12 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, suggesting that men leave the private casual sector to work in family employment. Family employment includes working on the family farm as well as in a family business.

Table 3 also shows the results for the daily private casual wage.[30] The outcome variable in column 4 is the level wage earned, conditional on having earned a positive daily wage, whereas column 5 uses log wages. Since column 2 shows some evidence of private employment changes, any wage impacts of NREGS in these specifications 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. The specification in column 6 is the unconditional private daily wage. According to column 4, the impact of NREGS on private wages is very small in absolute value and statistically insignificantly different from zero. The estimated coefficient in the first row of column 4, for example, suggests that the private wage for men employed in casual private-sector work decreased by 0.02 rupees in treatment districts relative to control districts at the cutoff, while the average wage across districts is 66 rupees. Overall, the conditional wage effects across specifications in column 4 suggest no change at all, and this is supported by similarly small and statistically insignificant estimates for log wages in column 5. The effect on the unconditional wage, which includes all men with zero private-sector wages, is larger and negative, but again statistically insignificant.

Table 4 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 absolute magnitude and statistically insignificantly different from zero. Column 2 shows that the impact of NREGS on casual private-

[30]Since public-sector casual employment is very low, daily private casual wages are almost identical to total daily wages from casual labor.

sector employment for women is negative and very small, suggesting no real impacts on the private sector, although the confidence intervals are typically wide and include economically significant magnitudes. Overall, Table 4 therefore shows that in contrast to men a change in the selection of women into the workforce does not seem to be a major concern.

Additionally, Table 4 suggests that NREGS has no large-scale effects on private-sector wages for casual work for women. The estimates are all negative and statistically insignificantly different from zero at conventional levels, although the estimates are larger in magnitude than those for men. The specification in the first row of column 4, for example, says that NREGS decreases the female daily casual wage, conditional on earning a positive amount, by about 1.3 rupees. The effect on the unconditional private-sector daily wage earned by women in the last 7 days is also small in absolute magnitude. These estimates suggest again that NREGS has not substantially affected the composition of women in private employment.

Existing evidence suggests that seasonal effects are important, with NREGS work being especially attractive during the agricultural off-season. Table 5 reports the seasonality results for men and women, respectively. Consistent with the existing literature, seasonality is treated quite crudely[31]: The months July through December roughly correspond to major harvesting and planting seasons for most crops and in wide parts of the country, and are therefore treated as the agricultural main season. January through June, on the other hand, are assigned to be the agricultural off-season or dry season. Table 7 shows that, consistent with existing evidence, public-sector employment for both men and women picks up in the dry season, although the effects are not statistically significantly different from zero at conventional levels, whereas the main effects are typically small in absolute magnitude and negative.

[31]See e.g. Imbert and Papp (2012)

6.2 Robustness Checks and Discussion

A couple of alternative specifications can be used to test how robust the presented main results are. Potentially the most significant 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 in the first stage of the government algorithm 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 Tables 3 and 4 without the districts right around the cutoff by excluding districts with a ranks 0 and 1, which correspond to the last treated and untreated districts, respectively. This approach is typically referred to as the donut-hole approach.[32] Applying this approach has two disadvantages, however: 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, which is a concern especially in this case where the sample size is limited to begin with and reduces the power to detect effects.

The results of the donut hole approach are reported in Tables 6 and 7. The estimates are only imprecisely estimated but the qualitative patterns are similar to those in Tables 3 and 4. The previously statistically significant decrease in private casual employment for men is attenuated and no longer significant, although the estimates are otherwise qualitatively similar to those in Table 3. For women, the estimated coefficients on family employment are similar in absolute magnitude but switch sign when compared to Table 4, but the standard errors on the estimates are also large. Overall, however,

[32] see e.g. Almond and Doyle Jr. (2011) for a similar application.

Tables 6 and 7 support the conclusion that the impact of NREGS is limited.

A second robustness check relates to the level of aggregation of the data. Tables 3 and 4 estimate the impacts of NREGS at the district level and therefore give us an estimate of the impacts of NREGS on district equilibrium outcomes. Since the original data comes from surveys at the individual level, however, it is also interesting to see whether the impacts for an individual differ importantly from the impacts on an average worker in the district. Tables 8 and 9 therefore show the results corresponding to Tables 3 and 4 at the individual level, where individual observations are weighted using sampling weights.[33] Overall, the results are qualitatively similar to those reported in Tables 3 and 4 and again suggest that the labor market impacts of NREGS are limited. Wage and employment effects are typically small and statistically insignificant. As in Table 3, men seem to be less likely to work in the private sector and more likely to be engaged in family employment in NREGS districts, although the estimates tend to be somewhat attenuated relative to the district-level results and are not statistically significant in three out of the four presented specifications.

Overall, the results suggest that NREGS only has a very limited 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. These results are 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.[34]

Maybe most surprising is the fact that a large-scale public works program like

[33]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 Tables 8 and 9 do not control for any baseline outcomes, but the results are robust to controlling for the baseline district average in the outcome.

[34]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.

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 increases in public employment on the order of magnitude of 15 times, statistical power is not big enough to precisely estimate such small effects in absolute magnitude. The reported confidence intervals in Tables 3 and 4 imply that the empirical analysis can typically rule out increases in public employment above 2-3 percentage points. For such a large and ambitious program that aimed at providing flexible employment opportunities for a large portion of poor rural households, even this upper bound seems like a relatively modest effect.

One potential 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 these estimates to expected effect sizes based on administrative data. While some papers have documented that administrative records are exaggerating the effectiveness of NREGS due to corruption issues (see e.g. Niehaus and Sukhtankar 2011a, b), the administrative records provide an upper bound on NREGS impacts.

According to administrative records, the employment guarantee scheme provided 1435.9 million person-days of employment in 1.78 million projects in the 330 Phase 1 and Phase 2 NREGS districts in 2007-2008.[35] 61.15 percent of this employment was given to women. The average daily wage paid was 75 rupees. This means that in a typical week, the scheme generated .0598 workdays in general, and .0699 workdays for women, and there were about 104 projects operating in the average district. In the NSS

[35]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.

data, the number of public-works workdays in Phase 1 and Phase 2 districts are .0617 for prime-aged adults, and .0414 for women, respectively. 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, although my sample underestimates the employment effects for women relative to the official data. This, in turn, suggests that, if at all, the male results in this paper are overestimates, while the female results are underestimates of the effects we would expect from an analysis of NREGS using administrative data. At the same time, however, these back-of-the-envelope calculations also support the public employment results in this paper in that generated employment opportunities seem to be relatively modest at the local level.

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. While it seems likely that the parallel trend assumption necessary for internal validity in that approach is violated because poor and richer districts probably experience different trends in labor market outcomes, difference-in-difference specifications also 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. Tables 10 and 11 estimate the labor market effects for the sample used for the regression discontinuity analysis using a difference-in-difference approach. Panel A reports the results using actual treatment assignment, whereas Panel B presents the results using predicted treatment status from the algorithm. The two tables show that in my sample even the difference-in-difference analysis does not generate the substantial wage increases found in other papers. Public-sector employment is statiscally significantly higher in NREGS districts after the program is introduced, although the estimates are again relatively 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 10 compared to the regression discontinuity results in Table 3. Overall, Tables 10 and 11 show that the different results found in this paper are not just due to the choice of the empirical specification, but that differences in the sample composition may also be important: In contrast to other papers that lump Phase 1 and Phase 2 districts together and compare them to the Phase 3 control districts, the sample in this paper drops Phase 1 districts that are on average much poorer and likely to be very different from control districts. Additionally, I focus on states where assignment was plausibly made according to the set algorithm and drop the states for which treatment assignment is likely to have been highly endogenous because the development index is missing.

6.3 NREGS as a Safety Net?

The main results rule out that NREGS has led to substantial wage increases in the private sector. Taken together with the result that the increase in public-sector employment is not large, either, this suggests that NREGS has neither increased competition in rural labor markets, nor led to a better enforcement of existing minimum wage laws. While this makes NREGS largely ineffective at altering the labor market conditions for rural workers, it does not rule out the hypothesis that the program may provide a safety net and reduce the income volatility of households. Consistent with the conceptual framework outlined above, the private-sector employment estimates for men are typically negative, although they are not always statistically significant across robustness checks. If changes in the perceived riskiness of self-employment are an effect of the introduction of NREGS, we should also see heterogeneous treatment effects in the baseline riskiness of income. According to the conceptual framework, private employment should be higher, and family employment lower, in high-risk districts at baseline. After the introduction of NREGS, public employment should increase more in high-risk districts, 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.

Table 12 reports the empirical results 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.[36] The regressions also control for a district's expected rainfall and current-season rainfall shock. Consistent with the conceptual framework, column 1 shows that public employment is significantly higher in high-variance districts. The interaction effect for private employment is typically positive, although not statistically significant. Additionally, the private-sector wage is substantially higher in risky districts, suggesting that in these districts the higher demand for NREGS also leads to upward pressure on wages. Overall, the results in Table 12 are consistent with NREGS indeed providing a safety net in high-risk areas.

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 effects on the labor market are small, although many of the coefficients are so imprecisely estimated that larger effects cannot be completely ruled out. The general qualitative pattern is robust across a range of different empirical specifications, however: The introduction of the public-works scheme only leads to small increases is public employment and no changes in the private-sector wage. There is some evidence that men may drop out of the private sector and move

[36] There is no discontinuity in the standard deviation of rainfall of districts at the cutoff. Results for women are not reported here. There is no evidence that rainfall volatility affects labor market outcomes in the corresponding results for women.

into family employment, which is consistent with NREGS providing a safety net function that decreases the relative riskiness of self-employment for some households. The safety net function is further supported by empirical results that indicate that NREGS employment and private-sector wages are significantly higher in high-risk districts than in low-risk districts.

Overall, these results suggest that except in high income-risk districts with above median volatility in rainfall, NREGS is largely ineffective in 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 especially in high-volatility areas, however, where negative income shocks are potentially more severe. Anecdotal as well as growing quantitative evidence point to the effectiveness of NREGS being hampered at the implementation stage: Rationing of public employment in the face of excess demand seems to be a very widespread problem that is documented throughout India. If severe enough, this limits the ability of the scheme to function as an alternative source of income considerably. While part of this problem may be solved over time once local institutions learn how to be better prepared for high public employment demand, some anecdotal evidence implies that the big landowners may also try to strategically water down the employment guarantee program by demanding restrictions on NREGS projects during the peak agricultural seasons.

For a large government program with expenditures roughly equal to 1 percent of India's GDP, the results raise the question whether the provided welfare benefits are large enough to warrant the existence of such an ambitious scheme, or whether the money would be more effectively spent on other anti-poverty measures. More research on the direct and indirect effects of NREGS on the lives of the rural poor is therefore needed to get a better sense of the scheme's total costs and benefits.

References

[1] Almond, Douglas, and Joseph J. Doyle. 2011. 'After Midnight: A Regression Discontinuity Design in Length of Postpartum Hospital Stays.' *American Economic Journal: Economic Policy*, 3(3): 1-34.

[2] Azam, Mehtabul. 2012. 'The Impact of Indian Job Guarantee Scheme on Labor Market Outcomes: Evidence from a Natural Experiment.' IZA Discussion Paper 6548.

[3] Basu, Arnab. 2002. 'Oligopsonistic Landlords, Segmented Labor markets, and the Persistence of Tied-Labor Contracts.' *American Journal of Agricultural Economics*, 84(2): 438-453.

[4] Basu, Arnab. 2011. 'Impact of Rural Employment Guarantee Schemes on Seasonal Labor Markets: Optimum Compensation and Workers' Welfare.' IZA Discussion Paper 5071.

[5] Besley, Timothy, and Stephen Coate. 1992. 'Workfare versus Welfare: Incentive Arguments for Work Requirements in Poverty-Alleviation Programs.' *American Economic Review*, 82(1): 249-261.

[6] Centre for Science and Environment. 2008. 'An Assessment of the Performance of the National Rural Employment Guarantee Programme in Terms of its Potential for Creation of Natural Wealth in India's Villages.'

[7] Datt, Gaurav, and Martin Ravallion. 1994. 'Transfer Benefits from Public-Works Employment: Evidence from Rural India.' *The Economic Journal*, 104(427): 1346-1369.

[8] Dey, Nikhil, Jean Dreze, and Reetika Khera. 2006. *Employment Guarantee Act: A Primer.* (Delhi: National Book Trust, India)

[9] Dutta, Puja, Murgai, Rinku, Ravallion, Martin, and Dominique van de Walle. 2012. 'Does India's Employment Guarantee Scheme Guarantee Enployment? World Bank Policy Research Working Paper 6003.

[10] Government of India. 2009. 'The National Rural Employment Guarantee Act.'

[11] Gupta, Santanu. 2006. 'Were District Choices for NFFWP Appropriate?' *Journal of Indian School of Political Economy*, 18(4): 641-648.

[12] Imbert, Clement, and John Papp. 2012. 'Equilibrium Distributional Impacts of Government Employment Programs: Evidence of India's Employment Guarantee.' http://www.parisschoolofeconomics.eu/docs/imbert-clement/2012-03-19-pse-working-paper-equilibrium-distributional-impacts-of-government-programs–imbert-papp.pdf.

[13] Institute of Applied Manpower Research. 2007. 'All-India Report on Evaluation of NREGA - A Survey of 20 Districts.'

[14] Jeyaranjan, J. 2011. 'Women and Pro-Poor Policies in Rural Tamil Nadu: An Examination of Practices and Responses.' *Economic and Political Weekly*, XLVI(43): 64-74.

[15] Johnson, Doug. 2009a. 'Can Workfare Serve as a Substitute for Weather Insurance? The Case of NREGA in Andhra Pradesh.' Institute for Financial Management and Research, Centre for Micro Finance, Working Paper 32.

[16] Johnson, Doug. 2009b. 'How Do Caste, Gender and Party Affiliation of Locally Elected Leaders Affect Implementation of NREGA?' Institute for Financial Management and Research, Centre for Micro Finance Working Paper 33.

[17] Johnson, Doug, Ajay Tannirkulam, and Caroline Laroche. 2009. 'NREGA in Andhra Pradesh - Seven Lessons from the Data.' Centre for Micro Finance Focus Note.

[18] Kaur, Supreet. 2012. 'Nominal Wage Rigidity in Village Labor Markets.' $http : //www.people.fas.harvard.edu/\ kaur/papers/Kaur_{J}MP_{W}ageRigidity.pdf$.

[19] Khera, Reetika. 2009. 'Group Measurement of NREGA Work: The Jalore Experiment.' Centre for Development Economics Delhi School of Economics Working Paper 180.

[20] Khera, Reetika, and Nandini Nayak. 2009. 'Women Workers and Perceptions of the National Rural Employment Guarantee Act.' *Economic and Political Weekly*, XLIV(43): 49-57.

[21] Klonner, Stefan and Christian Oldiges. 2012. 'Employment Guarantee and Its Welfare Effects in India.' Mimeo

[22] Kluve, Jochen. 2010. 'The effectiveness of European active labor market programs.' *Labour Economics*, 17(6): 904-918.

[23] Lal, Radhika, Steve Miller, Maikel Lieuw-Kie-Song, and Daniel Kostzer. 2010. 'Public Works and Employment Programmes: Towards a Long-Term Development Approach.' International Policy Centre for Inclusive Growth Working Paper 66.

[24] Lee, David S. 2008. 'Randomized Experiments from Non-Random Selection in U.S. House Elections.' *Journal of Econometrics*, 142(2): 675-697.

[25] Lee, David S., and Thomas Lemieux. 2009. 'Regression Discontinuity Designs in Economics.' NBER Working Paper 14723.

[26] McCrary, Justin. 2008. 'Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test.' *Journal of Econometrics*, 142(2): 698-714.

[27] Ministry of Rural Development, Department of Rural Development, Government of India. 2010. 'Mahatma Gandhi National Rural Employment Guarantee Act 2005 - Report to the People 2nd Feb 2006 - 2nd Feb 2010.'

[28] Narayanan, Sudha. 2008. 'Employment Guarantee, Women's Work and Childcare.' *Economic and Political Weekly*, XLIII(9): 10-13.

[29] NCAER-PIF. 2009. 'Evaluating the performance of the National Rural Employment Guarantee Act.'

[30] Niehaus, Paul, and Sandip Sukhtankar. 2012a. 'The Marginal Rate of Corruption in Public Programs.' http://www.dartmouth.edu/ sandip/niehaus-sukhtankar-marginal-corruption.pdf.

[31] Niehaus, Paul, and Sandip Sukhtankar. 2012b. 'Corruption Dynamics: The Golden Goose Effect.' http://www.dartmouth.edu/ sandip/niehaus-sukhtankar-golden-goose.pdf.

[32] Planning Commission. 2003. 'Report of the Task Force: Identification of Districts for Wage and Self Employment Programmes.'

[33] Planning Commission. 2009. 'Report of the Expert Group to Review the Methodology for Estimation of Poverty.'

[34] Planning Commission (MLP Division). 2003. 'Backward Districts Initiative - Rashtriya Sam Vikas Yojana - The Scheme and Guidelines for Preparation of District Plans.'

[35] Samarthan Centre for Development Support. 2007. 'Status of NREGA Implementation: Grassroots Learning and Ways Forward - 1st Monitoring Report.'

[36] Subbarao, K. 1997. 'Public Works as an Anti-Poverty Program: An Overview of Cross-Country Experience.' *American Journal of Agricultural Economics*, 79(2): 678-683.

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	0.7619	0.7500
Assam	23	7	6	0.9130	0.7500
Bihar	36	22	14	0.8333	1.0000
Chhattisgarh	15	11	3	0.6667	1.0000
Gujarat	20	6	3	0.8000	0.9286
Haryana	18	2	1	0.7222	0.9375
Jharkhand	20	18	2	0.8000	1.0000
Karnataka	26	5	6	0.8846	0.5238
Kerala	10	2	2	0.7692	1.0000
Madhya Pradesh	42	18	10	0.7619	0.8750
Maharashtra	30	12	6	0.9333	0.5556
Orissa	30	19	5	0.7333	0.9091
Punjab	15	1	2	1.0000	0.9286
Rajasthan	31	6	6	0.9032	0.7200
Tamil Nadu	26	6	4	0.8846	0.9500
Uttar Pradesh	64	22	17	0.8906	0.7857
West Bengal	17	10	7	0.7647	1.0000
Total	447	180	100	0.8345	0.8202

Note: Table includes all districts with non-missing development index rank 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 rank 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. The proposed algorithm states that the number of treatment districts a state is assigned in a given phase is proportional to the percent of India's poor living in that state, and that this quota should be filled with the least developed districts according to the Indian Planning Commission's ranking of districts (Planning Commission 2003). In the first phase, districts on an existing official list of districts majorly affected by left-wing terrorism were prioritized regardless of their rank.

Table 2: 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	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 here is a district with non-missing Planning Commission rank in a given season in the baseline data (July 2004-June 2005). Summary statistics are calculated from aggregated and weighted individual NSS data.

Table 3: NREGS impact: wages and employment (men)

Specification	public employment	private employment	family employment	conditional private wage	conditional log wage	unconditional private wage
Linear	0.0012	-.0351*	0.0253	-0.02	-0.0041	-1.46
	(.0038)	(.0208)	(.0247)	(2.40)	(.0377)	(1.64)
R-squared	0.063	0.2791	0.3119	0.4102	0.3982	0.3275
Linear Flexible Slope	0.0011	-.0351*	0.0256	-0.02	-0.0041	-1.46
	(.0038)	(.0208)	(.0244)	(2.40)	(.0377)	(1.63)
R-squared	0.0644	0.2791	0.3127	0.4103	0.3983	0.3277
Quadratic	0.0007	-.0369*	0.0292	-0.09	-0.0070	-1.76
	(.0038)	(.0204)	(.0243)	(2.36)	(.0375)	(1.56)
R-squared	0.0636	0.2794	0.313	0.4103	0.3984	0.3288
Quadratic Flexible Slope	0.0018	-.0522*	0.0302	-0.77	-0.0196	-3.08
	(.0045)	(.0273)	(.0331)	(3.11)	(.0500)	(2.22)
R-squared	0.0652	0.2805	0.3131	0.4116	0.4023	0.3312
N	1063	1063	1063	1009	1007	1063
outcome mean	0.0069	0.3279	0.4846	66.38	4.1212	21.86

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors for clustering at district level in parenthesis. An observation is a district in a given season. The employment outcome variables are the mean employment proportion of a given type of working-age adults (18-60) with at most secondary education in the rural areas of a district. The specifications are parametric regressions with different levels of flexibility. The wage outcome variables are the mean daily casual private-sector wage of working-age adults (18-60) with at most secondary education in the rural areas of a district in columns 4 to 6. The wage in column 4 is the absolute wage conditional on private-sector employment in the last 7 days, whereas it is the conditional log wage in column 5. Column 6 uses the unconditional absolute wage. The treatment indicator NREGS is the predicted treatment status based on the proposed algorithm.

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

Specification	public employment	private employment	family employment	conditional private wage	conditional log wage	unconditional private wage
Linear	0.0013	-0.0035	0.0166	-1.30	0.0041	-0.38
	(.0044)	(.0166)	(.0259)	(3.39)	(.0660)	(.88)
R-squared	0.1344	0.4454	0.2611	0.2203	0.264	0.2523
Linear Flexible Slope	0.0013	-0.0034	0.0161	-1.29	0.0038	-0.38
	(.0044)	(.0166)	(.0256)	(3.44)	(.0663)	(.88)
R-squared	0.1345	0.4459	0.2667	0.2203	0.2641	0.2523
Quadratic	0.0015	-0.0020	0.0108	-1.42	0.0050	-0.40
	(.0045)	(.0165)	(.0255)	(3.29)	(.0660)	(.88)
R-squared	0.1345	0.4458	0.2644	0.2204	0.2641	0.2523
Quadratic Flexible Slope	-0.0026	-0.0073	0.0340	-4.96	-0.0706	-0.61
	(.0043)	(.0210)	(.0334)	(5.04)	(.0925)	(1.08)
R-squared	0.1396	0.4459	0.268	0.2223	0.2664	0.2526
N	1063	1063	1063	663	656	1063
outcome mean	0.0053	0.1309	0.2285	43.50	3.6488	5.10

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors for clustering at district level in parenthesis. An observation is a district in a given season. The employment outcome variables are the mean employment proportion of a given type of working-age adults (18-60) with at most secondary education in the rural areas of a district. The specifications are parametric regressions with different levels of flexibility. The wage outcome variables are the mean daily casual private-sector wage of working-age adults (18-60) with at most secondary education in the rural areas of a district in columns 4 to 6. The wage in column 4 is the absolute wage conditional on private-sector employment in the last 7 days, whereas it is the conditional log wage in column 5. Column 6 uses the unconditional absolute wage. The treatment indicator NREGS is the predicted treatment status based on the proposed algorithm.

Table 5: Seasonality of public employment (men and women)

Panel A: men

	(1)	(2)	(3)	(4)
NREGS	-0.0025	-0.0012	-0.0033	0.0007
	(.0036)	(.0037)	(.0037)	(.0052)
NREGS*dry season	0.0073	0.0052	0.0073	0.0035
	(.0053)	(.0061)	(.0053)	(.0054)
R-squared	0.0833	0.0867	0.0845	0.0889
N	1063	1063	1063	1063
outcome mean	0.0069	0.0069	0.0069	0.0069
state FE	Yes	Yes	Yes	Yes
polynomial order	1	1	2	2
flexible slope	No	Yes	No	Yes

Panel B: women

	(1)	(2)	(3)	(4)
NREGS	-0.0017	-0.0018	-0.0020	-0.0039
	(.0033)	(.0036)	(.0032)	(.0046)
NREGS*dry season	0.0060	0.0064	0.0061	0.0034
	(.0047)	(.0053)	(.0047)	(.0045)
R-squared	0.1468	0.1472	0.147	0.1583
N	1063	1063	1063	1063
outcome mean	0.0053	0.0053	0.0053	0.0053
state FE	Yes	Yes	Yes	Yes
polynomial order	1	1	2	2
flexible slope	No	Yes	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1

Robust standard errors for clustering at district level in parenthesis. An observation is a district in a given season. The outcome variable is the mean public employment proportion of working-age adults (18-60) with at most secondary education in the rural areas of a district. The specifications are parametric regressions with different levels of flexibility. The treatment indicator NREGS is predicted treatment status based on the proposed algorithm.

Table 6: NREGS impact: wages and employment (men, donut hole)

Specification	public employment	private employment	family employment	conditional private wage	conditional log wage	unconditional private wage
Linear	0.0013	-0.0279	0.0207	0.90	0.0136	-0.40
	(.0049)	(.0260)	(.0299)	(3.05)	(.0473)	(2.08)
R-squared	0.0727	0.2894	0.3292	0.3977	0.3912	0.3243
Linear Flexible Slope	0.0010	-0.0277	0.0216	0.95	0.0138	-0.47
	(.0049)	(.0258)	(.0297)	(3.03)	(.0472)	(2.05)
R-squared	0.0739	0.2894	0.3295	0.3978	0.3912	0.3246
Quadratic	0.0006	-0.0301	0.0260	0.77	0.0081	-0.88
	(.0050)	(.0255)	(.0296)	(3.01)	(.0473)	(1.98)
R-squared	0.0733	0.2896	0.3302	0.3978	0.3916	0.3258
Quadratic Flexible Slope	0.0024	-0.0459	0.0289	0.32	0.0085	-1.91
	(.0067)	(.0399)	(.0462)	(4.71)	(.0741)	(3.24)
R-squared	0.0744	0.2924	0.3318	0.3998	0.396	0.3294
N	944	944	944	894	892	944
outcome mean	0.0072	0.3285	0.4840	66.48	4.1221	21.85

Table 7: NREGS impact: wages and employment (women, donut hole)

Specification	public employment	private employment	family employment	conditional private wage	conditional log wage	unconditional private wage
Linear	0.0030	-0.0064	-0.0149	-1.44	0.0162	-0.34
	(.0061)	(.0208)	(.0308)	(4.39)	(.0850)	(1.20)
R-squared	0.157	0.4477	0.2711	0.2145	0.2618	0.2509
Linear Flexible Slope	0.0031	-0.0054	-0.0186	-1.42	0.0174	-0.33
	(.0062)	(.0208)	(.0301)	(4.35)	(.0842)	(1.20)
R-squared	0.1574	0.4484	0.2765	0.2146	0.2623	0.251
Quadratic	0.0036	-0.0039	-0.0232	-1.62	0.0173	-0.36
	(.0064)	(.0207)	(.0303)	(4.24)	(.0848)	(1.20)
R-squared	0.1576	0.4482	0.2743	0.2147	0.2618	0.2509
Quadratic Flexible Slope	-0.0026	-0.0176	-0.0389	-9.65	-0.1359	-0.99
	(.0071)	(.0288)	(.0446)	(7.70)	(.1377)	(1.72)
R-squared	0.1596	0.4487	0.2793	0.22	0.2674	0.2522
N	944	944	944	584	577	944
outcome mean	0.0057	0.1288	0.2305	44.01	3.6561	5.07

Table 8: NREGS impact: wages and employment at the individual level (men)

Specification	public employment	private employment	family employment	conditional private wage	conditional log wage	unconditional private wage
Linear	-0.0025	-0.0286	0.0341	3.08	0.0472	-0.59
	(.0039)	(.0208)	(.0253)	(2.73)	(.0417)	(1.66)
R-squared	0.0158	0.0439	0.0545	0.2415	0.2173	0.0592
Linear Flexible Slope	-0.0024	-0.0286	0.0339	3.07	0.0468	-0.59
	(.0039)	(.0209)	(.0250)	(2.74)	(.0416)	(1.67)
R-squared	0.0161	0.0439	0.0547	0.2415	0.2174	0.0592
Quadratic	-0.0031	-0.0296	0.0391	2.88	0.0454	-0.72
	(.0040)	(.0205)	(.0251)	(2.71)	(.0418)	(1.61)
R-squared	0.016	0.0439	0.0548	0.2416	0.2173	0.0592
Quadratic Flexible Slope	-0.0031	-.0531**	.0595*	2.21	0.0441	-2.52
	(.0056)	(.0252)	(.0320)	(3.08)	(.0480)	(2.06)
R-squared	0.0164	0.0445	0.055	0.2424	0.22	0.0602
N	37224	37224	37224	12138	12062	37224
outcome mean	0.0082	0.3261	0.4756	64.89	4.0444	21.16

Table 9: NREGS impact: wages and employment at the individual level (women)

Specification	public employment	private employment	family employment	conditional private wage	conditional log wage	unconditional private wage
Linear	0.0009	-0.0025	0.0254	-0.72	-0.0231	-0.14
	(.0036)	(.0171)	(.0251)	(2.09)	(.0528)	(.85)
R-squared	0.0212	0.0896	0.0533	0.1974	0.1606	0.0502
Linear Flexible Slope	0.0010	-0.0032	0.0274	-0.64	-0.0220	-0.16
	(.0035)	(.0172)	(.0250)	(2.14)	(.0537)	(.85)
R-squared	0.0214	0.0898	0.0544	0.1975	0.1607	0.0503
Quadratic	0.0008	-0.0015	0.0199	-0.82	-0.0257	-0.15
	(.0036)	(.0172)	(.0248)	(2.09)	(.0532)	(.86)
R-squared	0.0212	0.0897	0.0543	0.1978	0.161	0.0502
Quadratic Flexible Slope	-0.0027	-0.0125	0.0409	-1.85	-0.0585	-0.50
	(.0041)	(.0206)	(.0327)	(2.55)	(.0606)	(.96)
R-squared	0.0226	0.0901	0.0546	0.1984	0.1619	0.0508
N	41978	41978	41978	5148	5148	41978
outcome mean	0.0046	0.1234	0.2106	39.05	3.5222	4.82

Table 10: NREGS impact: Difference-in-Difference Estimates (men)

Panel A: Actual Treatment

	public employment	private employment	family employment	private cond. wage	private log wage	private uncond. wage
NREGS*post period	0.0083**	0.0060	-.0344**	-0.47	0.0100	0.60
	(.0036)	(.0160)	(.0173)	(1.73)	(.0297)	(1.25)
NREGS	0.0019	-0.0019	.0319**	-4.17***	-.0741***	-1.71**
	(.0018)	(.0122)	(.0137)	(1.55)	(.0297)	(.82)
post period	0.0014	0.0147	-.0555***	5.76***	.0832***	2.65***
	(.0009)	(.0103)	(.0103)	(1.20)	(.0179)	(.91)
R-squared	0.0599	0.2847	0.3275	0.4684	0.4111	0.3897
N	2126	2126	2126	2018	2014	2126
outcome mean	0.0047	0.3194	0.5188	63.59	4.08	20.42

Panel B: Predicted Treatment

	public employment	private employment	family employment	private cond. wage	private log wage	private uncond. wage
NREGS*post period	.0056*	0.0141	-0.0405**	-0.9800045	-0.0075	0.64
	(.0031)	(.0159)	(.0165)	(1.72)	(.0289)	(1.24)
NREGS	-0.0022	-0.0192	.0404***	-4.27***	-.0664**	-2.34***
	(.0016)	(.0121)	(.0142)	(1.58)	(.0283)	(.86)
post period	0.0022	0.0114	-.0523***	5.98***	.0900***	2.62***
	(.0016)	(.0104)	(.0110)	(1.25)	(.0188)	(.93)
R-squared	0.0491	0.2856	0.3288	0.4691	0.411	0.3911
N	2126	2126	2126	2018	2014	2126
outcome mean	0.0047	0.3194	0.5188	63.59	4.08	20.42

Table 11: NREGS impact: Difference-in-Difference Estimates (women)

Panel A: Actual Treatment

	public employment	private employment	family employment	private cond. wage	private log wage	private uncond. wage
NREGS*post period	.0075**	0.0035	0.0049	-1.40	-0.0126	0.30
	(.0035)	(.0109)	(.0174)	(2.05)	(.0461)	(.52)
NREGS	0.0028	0.0115	-0.0167	-2.24	-0.0458	0.12
	(.0019)	(.0102)	(.0186)	(1.47)	(.0369)	(.45)
post period	0.0007	-0.0104	-0.0793***	2.01	-0.0058	-0.35
	(.0005)	(.0064)	(.0119)	(1.60)	(.0288)	(.33)
R-squared	0.0779	0.436	0.2619	0.2474	0.2721	0.2809
N	2126	2126	2126	1326	1312	2126
outcome mean	0.0036	0.1354	0.2672	41.99	3.64	5.22

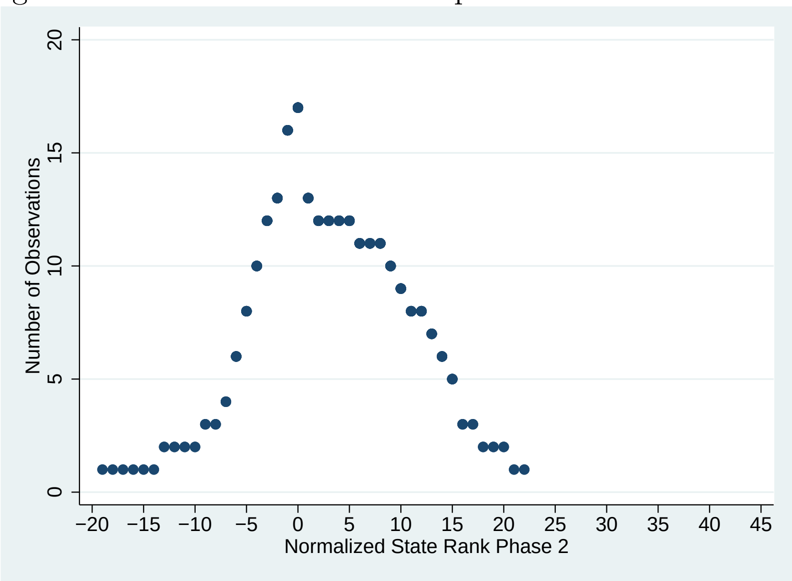
Panel B: Predicted Treatment

	public employment	private employment	family employment	private cond. wage	private log wage	private uncond. wage
NREGS*post period	0.0043	0.0073	0.0159	-2.72	-0.0249	0.35
	(.0031)	(.0104)	(.0173)	(2.10)	(.0451)	(.51)
NREGS	-0.0001	0.0176*	0.0073	-4.42***	-.1013***	0.15
	(.0014)	(.0099)	(.0198)	(1.45)	(.0358)	(.44)
post period	0.0018	-.0119*	-.0837***	2.69	0.0004	-0.38
	(.0012)	(.0069)	(.0122)	(1.74)	(.0305)	(.35)
R-squared	0.0646	0.438	0.2623	0.2563	0.2805	0.2811
N	2126	2126	2126	1326	1312	2126
outcome mean	0.0036	0.1354	0.2672	41.99	3.64	5.22

Table 12: NREGS impacts and risk heterogeneity: Wages and employment (men)

Specification	public employment	private employment	family employment	conditional private wage	conditional log wage	unconditional private wage
Linear	-0.0042	-.0443*	0.0338	-4.42	-0.0568	-3.13*
	(.0043)	(.0243)	(.0274)	(2.86)	(.0425)	(1.84)
NREGS*high standard deviation	.0096**	0.0162	-0.0149	7.66***	.0925**	2.98*
	(.0038)	(.0200)	(.0223)	(2.46)	(.0375)	(1.53)
R-squared	0.0677	0.2813	0.3132	0.419	0.4053	0.3336
Linear Flexible Slope	-0.0062	-0.0418	0.0340	-5.00*	-0.0568	-3.14*
	(.0044)	(.0257)	(.0272)	(3.01)	(.0425)	(1.83)
NREGS*high standard deviation	.0132***	0.0116	-0.0150	8.78***	.0925**	2.98*
	(.0050)	(.02439	(.0224)	(2.87)	(.0375)	(1.53)
R-squared	0.0702	0.2814	0.3141	0.4193	0.4054	0.3338
Quadratic	-0.0047	-.0460*	0.0376	-4.47	-0.0596	-3.44*
	(.0043)	(.0238)	(.0270)	(2.82)	(.0423)	(1.77)
NREGS*high standard deviation	.0096**	0.0162	-0.0150	7.65***	.0925**	2.98*
	(.0038)	(.0200)	(.0224)	(2.46)	(.0375)	(1.53)
R-squared	0.0683	0.2816	0.3144	0.419	0.4055	0.3348
Quadratic Flexible Slope	-0.0043	-0.0462	0.0387	-5.74	-0.0838	-4.51*
	(.0049)	(.0321)	(.0352)	(3.57)	(.0571)	(2.51)
NREGS*high standard deviation	.0109*	-0.0134	-0.0149	8.77***	.1146**	2.45
	(.0056)	(.0288)	(.0224)	(3.13)	(.0498)	(2.14)
R-squared	0.0714	0.2841	0.3144	0.4206	0.4097	0.3376
N	1063	1063	1063	1009	1007	1063
outcome mean	0.0069	0.3279	0.4846	66.38	4.1212	21.86

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 normalized 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

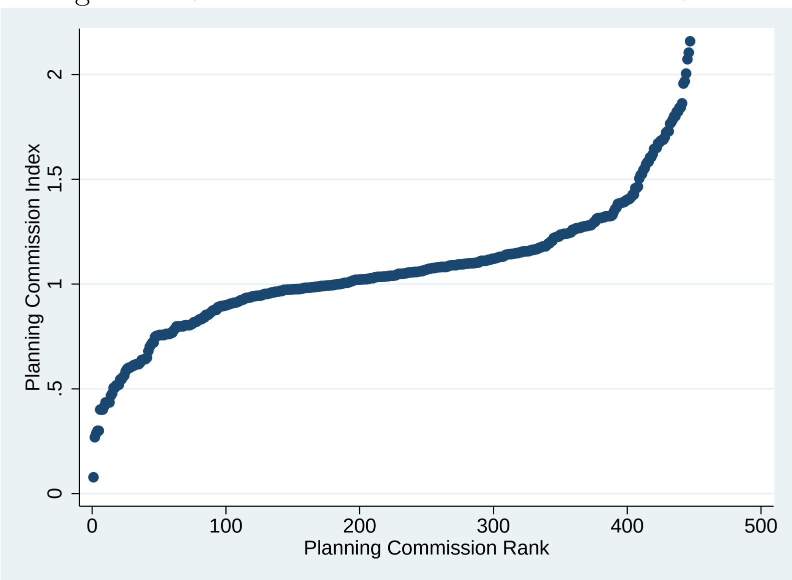


Figure 3: Distribution of Index over State-Specific Ranks (Phase 2 vs Phase 3)

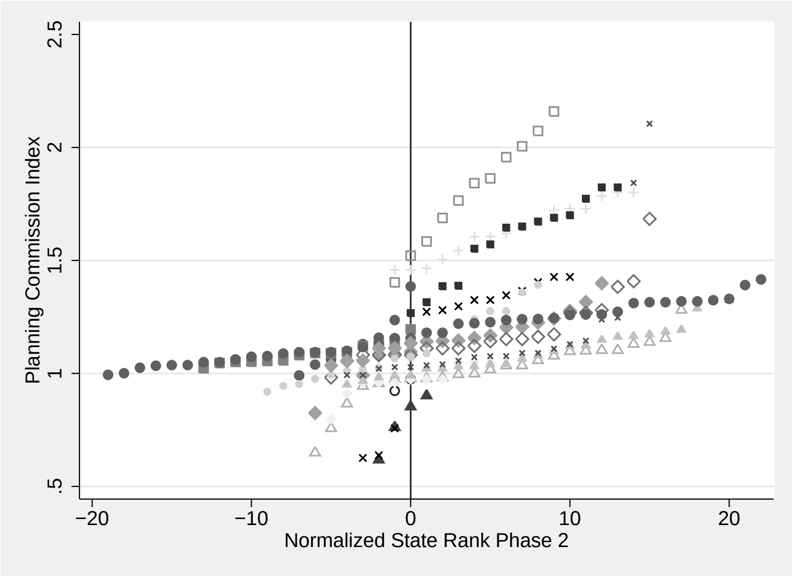
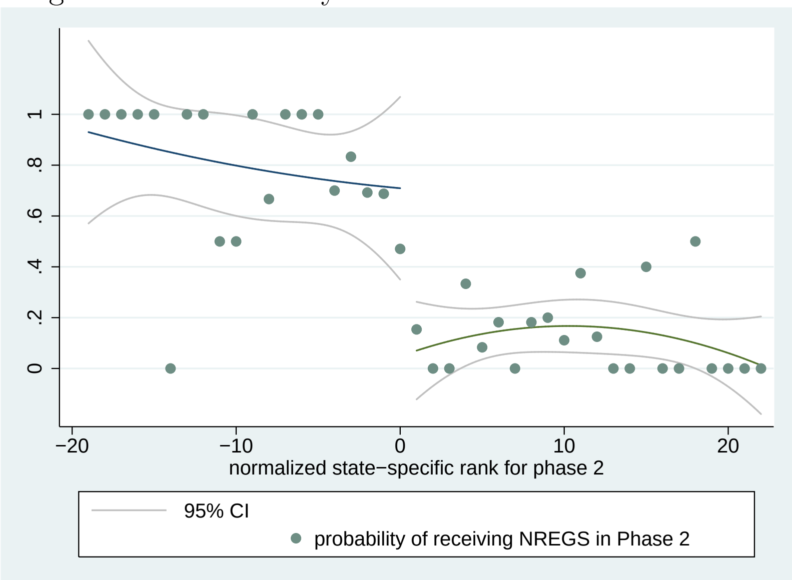
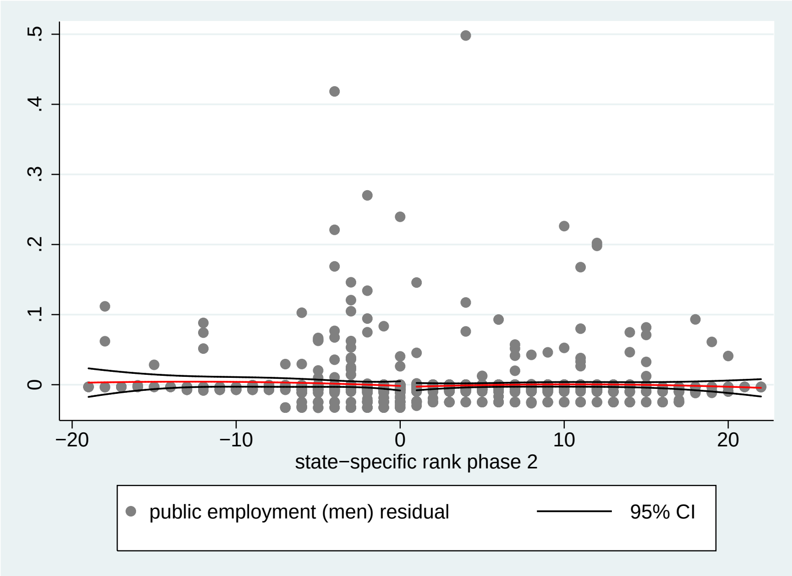


Figure 4: Discontinuity of treatment status for Phase 2



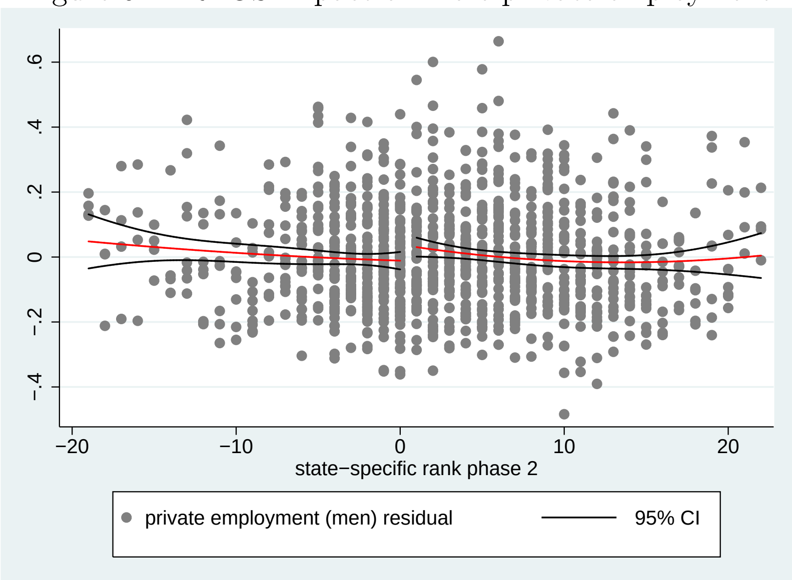
Note: Figure 4 excludes Phase 1 districts. The used bin size is 1, so each individual rank.

Figure 5: NREGS impact on male public employment



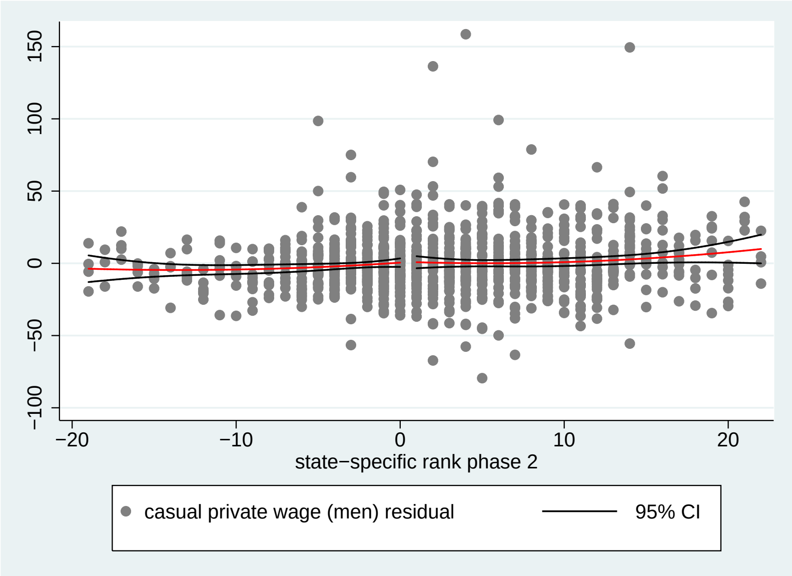
Note: Outcome variable is difference of outcome values and baseline values. An observation is the average district-level change at a given rank.

Figure 6: NREGS impact on male private employment



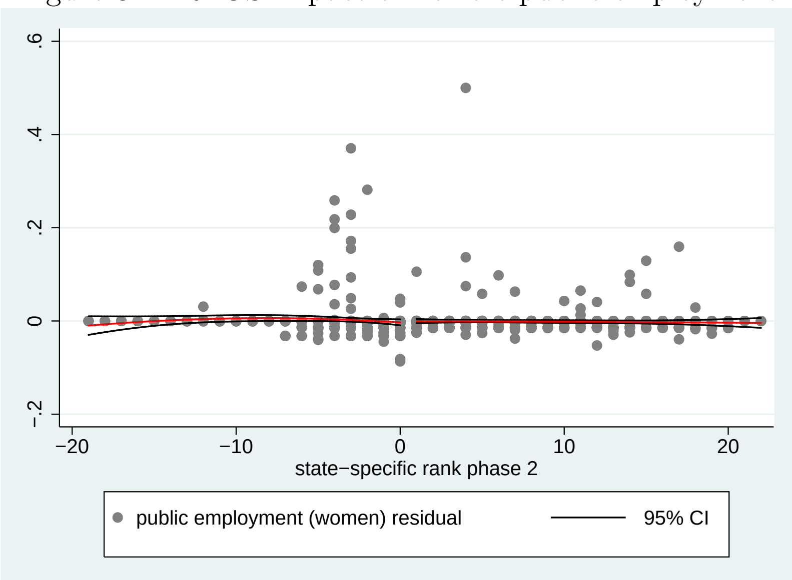
Note: Outcome variable is difference of outcome values and baseline values. An observation is the average district-level change at a given rank.

Figure 7: NREGS impact on male private wage



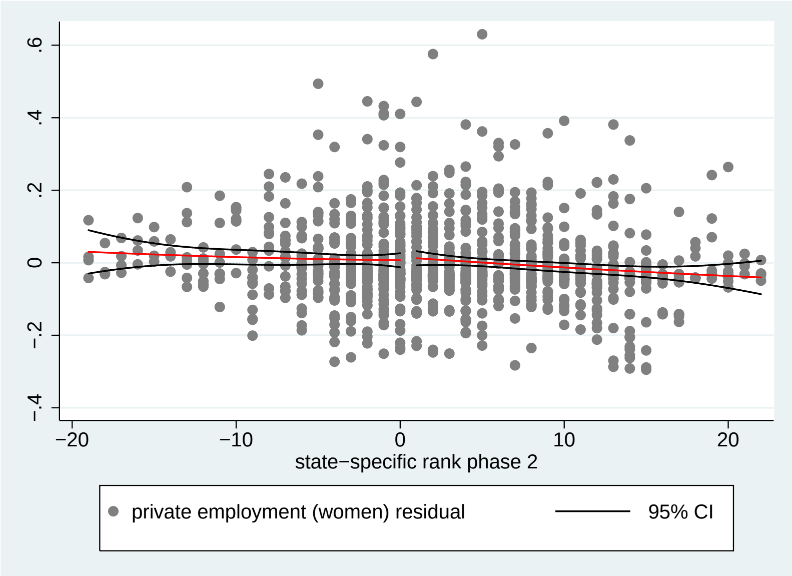
Note: Outcome variable is difference of outcome values and baseline values. An observation is the average district-level change at a given rank.

Figure 8: NREGS impact on female public employment



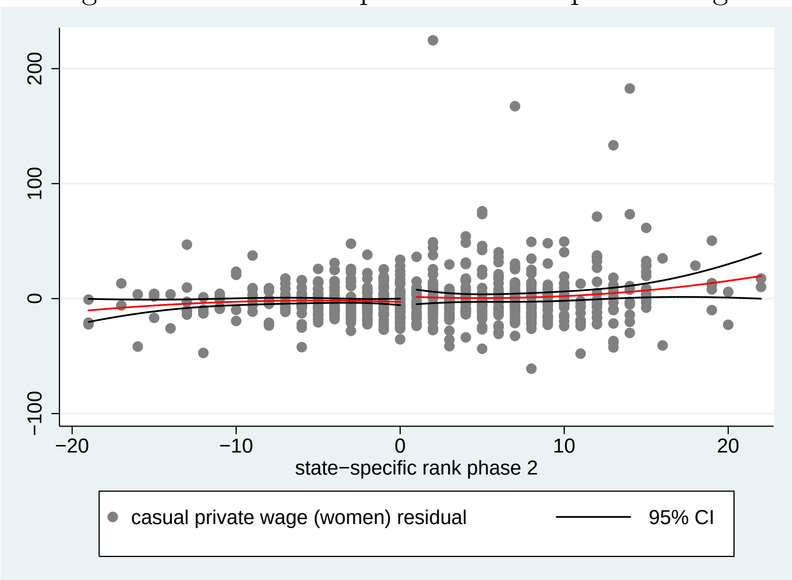
Note: Outcome variable is difference of outcome values and baseline values. An observation is the average district-level change at a given rank.

Figure 9: NREGS impact on female private employment



Note: Outcome variable is difference of outcome values and baseline values. An observation is the average district-level change at a given rank.

Figure 10: NREGS impact on female private wage



Note: Outcome variable is difference of outcome values and baseline values. An observation is the average district-level change at a given rank.

Appendix

Table A1: IV NREGS impact: wages and employment (men)

Specification	public employment	private employment	family employment	conditional private wage	conditional log wage	unconditional private wage
Linear	0.0027	-0.0799	0.0579	-0.06	-0.0093	-3.33
	(.0085)	(.0508)	(.0583)	(5.39)	(.0847)	(3.82)
R-squared	0.0683	0.2536	0.3015	0.4103	0.3983	0.3224
F-stat first stage	114.8	122.07	116.39	110.12	105.8	116.47
Linear Flexible Slope	0.0018	-0.0850	0.0591	0.23	-0.0087	-3.38
	(.0087)	(.0529)	(.0576)	(5.61)	(.0853)	(3.79)
R-squared	0.0566	0.2329	0.3037	0.4067	0.3977	0.3207
F-stat first stage	70.65	86.99	111.15	71.03	74.7	93.61
Quadratic	0.0008	-.0982*	0.0696	0.18	-0.0165	-4.20
	(.0090)	(.0543)	(.0608)	(5.62)	(.0871)	(3.86)
R-squared	0.0658	0.2448	0.2991	0.4102	0.3986	0.3212
F-stat first stage	95.99	111.15	113.35	95.02	92.47	108.26
Quadratic Flexible Slope	0.0105	-.1056*	0.0603	-3.14	-0.1010	-5.95
	(.0124)	(.0567)	(.0631)	(6.00)	(.1098)	(3.60)
R-squared		0.204	0.301	0.22		0.2982
F-stat first stage	53.45	56.9	71.38	51.74	49.39	53.91
N	1063	1063	1063	1009	1007	1063
outcome mean	0.0069	0.3279	0.4846	66.38	4.1212	21.86

Note: *** p<0.01, ** p<0.05, * p<0.1

Table A2: IV NREGS impact: wages and employment (women)

Specification	public employment	private employment	family employment	conditional private wage	conditional log wage	unconditional private wage
Linear	0.0030	-0.0081	0.0376	-2.90	0.0091	-0.88
	(.0100)	(.0376)	(.0594)	(7.49)	(.1439)	(2.02)
R-squared	0.1402	0.4452	0.2466	0.218	0.2642	0.2493
F-stat first stage	119.11	116.22	135.67	1084.9	1382.4	117.87
Linear Flexible Slope	0.0027	-0.0083	0.0370	-2.90	0.0063	-0.88
	(.0101)	(.0387)	(.0596)	(7.48)	(.1410)	(2.03)
R-squared	0.1401	0.4451	0.2469	0.218	0.2645	0.2487
F-stat first stage	86.54	84.45	95.01	172.11	357.9	96
Quadratic	0.0025	-0.0098	0.0255	-3.36	0.0115	-0.95
	(.0104)	(.0397)	(.0605)	(7.80)	(.1498)	(2.09)
R-squared	0.1394	0.4452	0.2551	0.2175	0.2642	0.2489
F-stat first stage	109.53	106.87	117.98	820.22	2122.75	110.14
Quadratic Flexible Slope	0.0047	-0.0132	0.0906	-7.88	-0.0940	-1.23
	(.0170)	(.0341)	(.0687)	(11.46)	(.2040)	(1.62)
R-squared		0.4013	0.1822	0.0897	0.0601	0.2345
F-stat first stage	55.78	54.45	63.32	109.4	108.95	54.62
N	1063	1063	1063	663	656	1063
outcome mean	0.0053	0.1309	0.2285	43.50	3.6488	5.10

Note: *** p<0.01, ** p<0.05, * p<0.1