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

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Abstract

Most countries around the world implement some form of a safety net program for poor households. A widespread concern is that such programs crowd out private-sector jobs. But they could also improve workers' welfare by allowing them to take on more risk, for example through self-employment. This paper analyzes the employment impacts of the world's largest public-works program using a novel regression-discontinuity design. The analysis exploits detailed institutional information to describe the allocation formula of the program and to construct a benefit calculator that predicts early and late treatment of districts. The results show that there is little evidence of a crowding out of private-sector jobs. Instead, the scheme functions as a safety net after a bad rainfall shock. Male workers also take on more risk by moving into family employment. This self-revealed preference for a different type of job suggests other potential benefits of safety net programs which so far have received little attention in the literature.

JEL: H53, H75, I38, J22, J38

Keywords: public-works program, anti-poverty program, National Rural Employment Guarantee Scheme, NREGA, NREGS, India, regression discontinuity design, safety net, crowding out, risk coping, risk mitigation, insurance

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1 Introduction

Most countries around the world implement some kind of a safety net program to help households deal with poverty, for example in the form of welfare payments or public-works schemes. A major concern with these programs is that the availability of government benefits may reduce job search efforts or hours worked, thereby crowding out private-sector employment. But a safety net may also allow workers to take more risks, for example by giving them the freedom to pursue their desired career path and establish a better long-term job match. In the absence of a safety net, workers may have to choose safe jobs that will pay the bills rather than being able to take a chance on a preferred job with more uncertain wages, such as in self-employment. The existence of a welfare program, which can be used to lower the income uncertainty of those jobs, could therefore improve social welfare by increasing personal autonomy, enhancing the productivity of the workforce and contributing to economic growth.¹ But while an extensive literature studies crowd-out, there is little empirical evidence on whether a safety net affects the type of private-sector job workers choose, for example more risky but with higher expected income.²

I analyze the impact of a safety net program on crowd-out and workers' job characteristics in the Indian context, where the government introduced the world's largest anti-poverty program in a staggered rollout between 2006 and 2008. Under the National Rural Employment Guarantee Scheme (NREGS) every rural household in India is eligible for up to 100 days of government-provided, manual labor jobs per year, irrespective of household income. The public-works program pays the minimum wage and is supposed to be demand-driven, allowing households to take up employment when they need it. NREGS functions as a safety net for the poor and does not aim to provide any job training opportunities.³ While NREGS

¹This idea is related to similar questions in insurance. Teh (2017), for example, shows that while a safety net may crowd out demand for complete insurance, it may increase the demand for incomplete insurance in a society of risk averse households and risk neutral providers. Offering incomplete insurance products leads to a Pareto improvement in welfare for households and providers.

²For an overview of the literature, see e.g. Hoynes (1997) and Hoynes and Rothstein (2019).

³While a goal of the program was to improve local development through the public-works projects, in practice jobs mostly focus on busy work and routine tasks rather than on the creation of infrastructure.

was introduced by the central government, India is a federal state and different levels of government take on key responsibilities in program implementation: Households enroll in the program at their local village council office, which also oversees employment. Projects and budgets have to be approved at higher tiers of the administration, and the program is financed by the central government.

The program was introduced in three implementation phases. Like many other development initiatives in India, NREGS was rolled out at the district level, the next administrative tier below the state level but above the village level. To determine which areas would receive the program early, the government used an allocation mechanism that had been developed by an independent government organization, the Planning Commission. The formula was constructed by technocrats to prioritize poor regions while taking into account fairness norms at multiple levels of the federal system. The allocation mechanism consists of two steps. The first step is supposed to ensure inter-state fairness in the allocation of early treatment areas: Every state receives at least one treatment district in each implementation phase, but a state's total number of treatment districts is proportional to the number of people below the poverty line. Large, poor states therefore receive a higher quota of treatment districts than richer smaller states. The second step then focuses on intra-state fairness: Using a ranking of districts based on proxies for poor economic development (such as low agricultural productivity and a high proportion of low-caste individuals), the quota from the first step is filled by taking the state's poorest districts from that ranking. The allocation mechanism therefore generates state-specific treatment cutoffs for each implementation phase. This procedure limits the room for political bargaining by providing an objective rationale for treatment assignment that is based on a poverty ranking. In practice, however, this predicted assignment was not completely followed, plausibly due to strategic considerations and political pressure by the central government and the state governments. The algorithm therefore provides the predicted treatment status of Indian districts, which may be different from the actual treatment status.

The allocation formula can be exploited to estimate the causal impacts of NREGS in a fuzzy regression-discontinuity design by comparing districts eligible for NREGS in a given treatment phase to similar districts that were too developed to qualify in that round. My analysis focuses on comparing Phase 2 and Phase 3 districts in a time period when Phase 2 had just received access to NREGS, but districts in the last implementation phase were still untreated.⁴ I first test whether the introduction of NREGS led to a crowding-out of private-sector employment, finding no support for such an effect: There is no negative impact on total private-sector employment, and NREGS take-up is low in the overall district-level sample of male workers.

At first glance, this may suggest that the program was completely ineffective, for example due to major implementation quality challenges like rationing or corruption. But I also test whether the availability of NREGS affects the type of private-sector jobs workers hold. If NREGS functions as a safety net that can be taken up after a negative income shock, then the introduction of NREGS may allow workers to take up jobs that would otherwise be considered too risky. The results show that following NREGS, workers move out of private casual wage employment and into family employment, consistent with a willingness to take on more risk. This effect is robust across multiple specifications. To test whether such a mechanism is plausible, I then divide the sample into districts with above and below the median rainfall variance. Since most of the family employment activities that workers engage in are related to agriculture, rainfall shocks provide a proxy for income shocks.

If NREGS allows workers to move to family employment because the program provides partial insurance after a negative rainfall shock, this safety net should more fully compensate for income losses in less risky districts. Workers in high-risk districts may therefore have a higher demand for NREGS to build up a buffer stock for bad shocks. Consistent with this, I find that take-up of NREGS is higher in high-risk districts, but that the move from private casual employment to family employment occurs in both high-risk and low-risk districts. I

⁴This is due to data constraints in the timing of household surveys. There is no continuously collected survey data on employment in India.

also verify that NREGS indeed provides a safety net after negative income shocks, finding that NREGS take-up rises substantially after negative rainfall shocks. Overall, these results suggest that workers are able to take on more risk through self-employment when NREGS is available because the program provides a safety net when faced with income shocks.⁵

These indirect employment effects show that Indian workers have a revealed preference for family employment over private casual employment after the introduction of a safety net, suggesting that there are monetary or non-monetary welfare improvements from this shift. While I find no effect of the introduction of NREGS on household income, the studied time period may be too short to be able to detect such an effect. Since the last implementation phase receives NREGS a year after Phase 2, I can only study the impact of the scheme over the first year of implementation. But behavioral changes could lead to household income increases over time. Using the research design developed in this paper, Hari and Raghunathan (2015), for example, find that households take on more risk after the introduction of NREGS by growing riskier cash crops that have a higher expected return. This investment may pay off in the medium to longer run. Evidence from other contexts is also consistent with the idea that workers derive non-monetary welfare improvements from moving to self-employment, for example because of better working conditions and more freedom in choosing their job (Bosch and Maloney, 2010; Maloney, 2004). Bandiera et al. (2013) show that less poor workers are more likely to be self-employed than the poorest, and that an intervention that relaxes credit constraints and improves skills for the very poor leads to substantial increases in the self-employment rate. Banerjee et al. (2011) report similar results, and Caria and Falco (2018) suggest that policies that lower downward income risk can improve happiness. These findings are consistent with a preference for self-employment over typical unskilled

⁵The empirical results in this paper are consistent with some existing research that finds that the insurance function often dominates the direct income benefits of such schemes, although this literature in general does not consider the time-allocation impacts that should arise even in the absence of shocks (see e.g. Dev (1995) and Subbarao et al. (2013)). Some other papers document changes in time allocation very similar to the results I find in this paper, but do not link these patterns to the safety-net function of public-works programs (see e.g. Berhane et al. (2011); Gilligan et al. (2008)).

casual jobs in rural areas.⁶

My analysis makes three contributions. First, my paper is the first to describe the benefit allocation procedure of NREGS laid out above. Using multiple technical reports by the Planning Commission, I construct a benefit calculator from the allocation formula, and combine it with other government documents to generate information on the predicted and actual treatment status of districts in each implementation phase. In federal systems, and in developing countries more broadly, such formulae are typically very difficult to recover because the political bargaining process is usually carried out behind closed doors, leading to non-random assignments without a general allocation strategy. In the Indian case, the reconstruction of the benefit calculator allows me to describe the allocation algorithm, which seeks to prioritize underdeveloped areas at the state and district levels while ensuring that every state receives treatment areas in each implementation phase. This also allows me to construct both the predicted and the actual treatment assignment. Without information on the allocation mechanism, other papers studying NREGS typically rely on the actual treatment status as their source of variation (Azam, 2012; Berg et al., 2018; Imbert and Papp, 2015). The reconstruction of the algorithm therefore also contributes to the literature studying the impacts of the world’s largest anti-poverty program, and has implications for our understanding of the allocation of resources of other government programs following similar norms.

Second, my paper studies how NREGS affected employment in rural areas, both with respect to crowding out of private-sector employment and the ability of households to take on more risk. The question of crowd-out has been studied extensively, revealing the importance of specific program design features for labor supply decisions, like the points in the income distribution at which subsidies are phased in and phased out, the tax rate on earned income

⁶They are also consistent with evidence from Vieider et al. (2018) that the poor tend to be less risk averse than richer households.

or the targeted subgroup eligible for the program.⁷⁸ But the existing literature focuses heavily on developed rather than on developing countries. Additionally, the focus is on labor supply and hours worked, but typically does not analyze whether the existence of a safe job or a government subsidy allows households to take on more risk.⁹ My analysis therefore provides some of the first evidence on this question for a large government program, highlighting potential unintended behavioral responses to government programs.¹⁰ This potential tradeoff between crowding out of private employment and the ability to make different job decisions is also an important part of the discussion surrounding Universal Basic Income (UBI) proposals, which have recently generated widespread interest in both developing and developed countries (Banerjee et al., 2019; Ghatak and Muralidharan, 2019; Hanna and Olken, 2018; Hoynes and Rothstein, 2019). Under these schemes, every household receives a monthly transfer that is not tied to any eligibility restrictions. While research on UBI is still in its infancy and has not been implemented at scale anywhere in the world, one big concern is that it may crowd out private-sector employment by lowering people's incentives to work. One often raised potential advantage of such programs is, however, that it may give individuals the freedom to pursue a job they are interested in rather than one they have to choose to make ends meet (Hanna and Olken, 2018; Van Parijs and Vanderborght, 2017).

So far, predictions about UBI impacts have to focus heavily on studies of existing welfare programs. But most operating welfare schemes are very different from a UBI since they are

⁷See e.g. Ahn (2018); Bargain and Doorley (2011); Bergolo and Cruces (2014); Bettendorf et al. (2014); Deuchert and Eugster (2019); Eissa and Hoynes (2006); Eissa et al. (2008); Gruber (2000); Hotz and Scholz (2003); Hoynes and Patel (2018); Hoynes and Schanzenbach (2012); Jensen (2003); Karlström et al. (2008); Laun (2017); Marinescu (2017); Meyer and Sullivan (2004); Michalopoulos et al. (2005); Milligan and Stabile (2007); Moffitt (2003); Nichols and Rothstein (2016); Stancanelli (2008)

⁸Traditional welfare programs disincentivize labor supply through negative income and substitution effects. Other programs, such as the EITC, are conceptualized to create strong incentives to work for households as a whole, but the existence of regions of the income distribution in which benefits are phased out reduce hours worked and the labor-supply incentives for secondary earners. See e.g. Eissa and Hoynes (2004), Hoynes (1997) and Hoynes and Rothstein (2019).

⁹For earlier research that raised some of these issues, see Ravallion (1991) and Datt and Ravallion (1994).

¹⁰Some papers analyze whether government programs aimed to increase the attractiveness of self-employment work. See e.g. Caliendo and Künn (2011) and Heim and Lurie (2010). In my case, changes to self-employment are an indirect and unintended consequence of providing government jobs.

typically targeted to households below the poverty line or, especially in developing countries, are only in effect for a short time period before being withdrawn. NREGS is much more similar to UBI programs on these dimensions: It does not have an income restriction and is a long-standing scheme that was conceptualized as a predictable safety net that would be available to households when they need it, rather than just being implemented during a recession or drought. This may actually allow households to take on more risk by reoptimizing their time use and employment options.

Third, my analysis contributes to the public finance literature in the developing country context. While there has been a recent surge in research on tax policy in developing countries, less research has focused on the expenditure side (Best et al., 2015; Carrillo et al., 2017; Gadenne, 2017; Kleven and Waseem, 2013; Olken and Singhal, 2011; Pomeranz, 2015; Pomeranz and Vila-Belda, forthcoming; Waseem, 2018). Yet, given the tax instruments utilized, in most developing countries government spending is more important to understand the within-country equity distribution. Government revenue relies heavily on tariffs and indirect taxes¹¹, which have less influence on a redistribution of income than a highly progressive income tax. In India, only about 2-3 percent of the population pays any income tax, and the agricultural sector is largely exempt from direct taxation (Piketty and Qian, 2009). This implies that government spending is the main tool to affect inequality and incentivize employment responses. Indeed, even in developed countries, many would argue that the first duty of the tax system is to raise revenue as efficiently as possible and then redistributive concerns can be addressed using welfare programs.

2 Background

Over the last 10 to 15 years, public-works programs have seen a resurgence in the developing world, with a new emphasis on predictability and versatility.¹² They are supposed to operate

¹¹See Besley and Persson (2013) and Gadenne and Singhal (2014) for an overview.

¹²The World Bank, for example, funded public-works programs in 24 countries between 2007 and 2009. See e.g. Lal et al. (2010). For an overview of public-works programs in developing countries see Subbarao

as both a supplemental income source for underemployed workers and as a reliable safety net. Both of these functions are particularly important in rural agricultural labor markets in developing countries. Despite the growing interest in using large public-works initiatives to address poverty in the developing world, evaluating their effects is a persistent challenge, because such initiatives are typically implemented in a manner that is endogenous to the outcomes of interest.¹³

2.1 Program Characteristics

The National Rural Employment Guarantee Scheme (NREGS) is one of the most ambitious government development programs in the world, with annual expenditures amounting to 1 percent of Indian GDP.¹⁴ It is based on the National Rural Employment Guarantee Act (NREGA) that legally guarantees each rural household 100 days of public-sector work a year at the minimum wage. It marks the first time that a national anti-poverty government scheme was given such a legal basis, introducing the idea of a right to work. There are no formal eligibility rules other than rural residency and the willingness to do manual work at the minimum wage. Households can apply for work at any time, and men and women are paid equally. At least one third of the NREGS workforce in a village is supposed to be female.

NREGS projects are supposed to advance local development primarily through drought-proofing, flood prevention and irrigation measures, and need to be carried out without the help of contractors or machines. During the time of the empirical analysis (July 2007 to June 2008) paid wages were the state minimum wage for agricultural laborers, although NREGA specified a floor wage. Wages must be paid within 15 days.

et al. (2013) and Zimmermann (2014).

¹³See e.g. Basu (2013), Besley and Coate (1992), and Datt and Ravallion (1994) for some examples of theoretical and empirical analyses. For an overview of public-works programs in developed countries see Kluge (2010).

¹⁴For more details, see Dey et al. (2006), Government of India (2009), and Ministry of Rural Development (2010).

2.2 Implementation and Effectiveness of the Program

Existing papers document varying levels of NREGS implementation across Indian states¹⁵: Johnson (2009) shows that participation in public-works projects in Andhra Pradesh increases after low rainfall, and Dasgupta (2017) finds that the program helps households in the same state alleviate the negative child health outcomes associated with droughts. Deininger and Liu (2013) find that NREGS increases nutritional intake and household assets in the same state, and Fetzer (2014) finds that the impact of rainfall shocks on Maoist-related conflict in India is attenuated in NREGS districts. But while these papers suggest that NREGS works well in some states, other research documents that this is not true everywhere: Niehaus and Sukhtankar (2013a) analyze corruption in NREGS implementation in 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, which is especially common in poorer states.

Some recent papers also analyze the impact of the program on employment. My results both complement and challenge an earlier study by Imbert and Papp (2015). Imbert and Papp (2015) focus on the heterogeneous impacts of NREGS with respect to seasonality, and show that public employment in NREGS during the dry season crowded out private employment and led to an increase in private-sector wages, whereas take-up is much lower during the rainy season and has no effects for the private sector. I use roughly the same employment data as Imbert and Papp (2015), but my analysis differs from theirs both conceptually and methodologically. Methodologically, I use a novel regression-discontinuity design rather than the difference-in-difference approach they employ, which leads to a different local average treatment estimate (LATE). This helps explore the robustness of the results to different identification strategies. The magnitude of the NREGS take-up effect in my analysis is similar to theirs once take-up is averaged across seasons, and they also find that the em-

¹⁵See Khera (2009) and Khera and Nayak (2009) for descriptive evidence.

ployment effects of NREGS occur predominantly on the intensive rather than the extensive margin. I also document that there is heterogeneity in NREGS take-up by showing that public employment is higher in districts with higher rainfall variability.

Using a finer distinction between different employment categories, I provide new results on the indirect employment effects of the scheme. While Imbert and Papp (2015) just distinguish between public and private employment, which includes domestic work, my results differentiate between private and family employment and show that an important effect is the willingness of workers to move out of private employment and into family employment due to NREGS' safety net function. I use this result and additional empirical tests to explore the relative importance of NREGS as a supplemental income tool and as a safety net, finding that many of the labor-market impacts are driven by the safety net function. In contrast to Imbert and Papp, I also do not find positive wage effects.¹⁶

In addition to Imbert and Papp (2015), two other studies use a version of a difference-in-difference approach to look at the program's impact on wages and employment, but focus on a narrower set of outcomes. Azam (2012) restricts the analysis to casual workers, and finds that public-sector employment increases by 2.5 percent while wages for males and females increase by 1 and 8 percent, respectively. While casual workers are one of the target groups most likely to take up NREGS, this occupational restriction cannot fully test whether workers re-optimize their time allocation after the introduction of NREGS. Berg et al. (2018) analyze the impact of NREGS on agricultural wages by using monthly information on agricultural wages from 2000 to 2011. The results in the paper suggest that agricultural wages have increased by about 5 percent in districts with a high implementation quality, but that it takes between 6 and 11 months after program rollout for these wage effects to be realized. Given the unavailability of employment outcomes in their dataset, they are unable to say

¹⁶Muralidharan et al. (2016) also find positive impacts on wages as a consequence of improvements in implementation quality of NREGS through the introduction of biometric smartcards. In addition to a different research question, they employ a randomized controlled trial in the Indian state of Andhra Pradesh. Both geographic coverage and empirical estimation strategy are therefore different from my study. Specifically, Andhra Pradesh is one of a few states typically classified as 'star states' for implementing NREGS better than other states.

anything about the broader employment impacts.

Both papers therefore do not consider potential substitution effects between various categories of non-public employment, which could arise if the introduction of NREGS induces households to re-optimize their time-allocation decisions. That this is important is also suggested by Hari and Raghunathan (2015), who use the RD approach developed in this paper and find that NREGS increases the riskiness of the portfolio of crops grown, which is consistent with the time-allocation effects found in this paper.

3 Benefit Calculator and Regression Discontinuity Design

The National Rural Employment Guarantee Act (NREGA) was passed in the Indian Parliament in August 2005. NREGS came into force in February 2006 in the first 200 districts. An additional 130 districts received the program in April 2007, and all remaining rural districts started NREGS in April 2008 (Ministry of Rural Development, 2010).¹⁷ I will refer to the district phases as Phases 1, 2, and 3, respectively.

This phasing-in allows the use of a regression-discontinuity (RD) design since the government used a two-stage assignment algorithm to allocate districts to implementation phases: In the first step, each Indian state receives a quota of treatment districts proportional to the percentage of India's poor living in that state. In the second step, the quota in each state is filled with the poorest districts according to a development ranking. This setup is designed to ensure inter-state fairness in the allocation of districts in the first step and intra-state fairness in the second step.

The prevalence of poverty across states is measured by the headcount ratio times the rural state population. The calculations used headcount ratio data from the 1993-1994 nationally

¹⁷Districts with a 100 percent urban population were excluded. The scheme operates in 99 percent of all Indian districts.

representative National Sample Survey (NSS) and population data from the 2001 Indian Census.¹⁸ The development ranking was created in a 2003 report by the Indian Planning Commission, which combined information on agricultural wages, agricultural productivity and the district population of low-caste individuals (Scheduled Castes and Scheduled Tribes) to create an index of economic underdevelopment (Planning Commission, 2003).¹⁹ Districts were then ranked on these values.²⁰

The algorithm creates state-specific treatment cutoffs between implementation phases. 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. Since the dataset that I will be using in my empirical analysis was collected 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 empirical analysis.

For the empirical estimation ranks are made state-specific and are re-centered so that a district with a normalized state-specific rank of zero is the last eligible district in a state. Negative numbers are assigned to districts with lower ranks and positive numbers to richer districts.

There is no reason to believe that the algorithm used in the analysis is not the correct algorithm. The official government documents mentioned above explicitly provide details on parts of it, e.g. by providing the raw data and all of the information needed to construct

¹⁸I use the state headcount ratios from Planning Commission (2009), since the original headcount ratio calculations do not have estimates for new states that had been created by then. As official Planning Commission estimates, they are likely to be closest to the information the Indian government would have had access to at the time of NREGS implementation.

¹⁹Data on the outcome variables was unavailable for the remaining Indian states, and it is unclear whether a comparable algorithm using different outcome variables was used for them. I therefore restrict the empirical analysis to these 17 states.

²⁰In addition to the algorithm, the government had a separate list of 32 districts heavily affected by Maoist violence. See e.g. Planning Commission (2005). These districts were not subject to the algorithm and all received NREGS in the first implementation phase. The analysis in this paper focuses on Phase 2 and Phase 3 districts.

the index values for the district development ranking. I use this information to construct my benefit calculator. The actual use of the algorithm for NREGS district allocation was also confirmed by some experts, including a former member of the Planning Commission of India.

Table 1 provides an overview of how well the algorithm predicts actual NREGS receipt for 17 major Indian states.²¹ 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. Columns 4 and 5 give the success rate of the algorithm, which is calculated as the percent of treated districts for which actual equals predicted treatment status.

Table 1 shows that the overall prediction success rate of the algorithm is about 84 percent in Phase 1 and 82 percent in Phase 2. There is some slippage in the algorithm use and heterogeneity in the performance of the algorithm across states. Nevertheless, the algorithm performs quite well in almost all states and the prediction success rates are considerably higher than those expected from random assignment, which are 40.27 percent for Phase 1 and 37.45 percent for Phase 2. Table 1 implies that the empirical identification strategy is a fuzzy RD design.

The main assumption in an RD design is that treatment areas were unable to perfectly manipulate their treatment status (Lee, 2008). In the context of NREGS, this assumption fails if districts were able to receive early access to NREGS by manipulating the underlying algorithm. That manipulating the algorithm was possible is unlikely. First, development index values and state headcount ratios were constructed based on data from the early to mid-1990s, which had long been publicly available at the time of NREGS district allocation. This eliminates the possibility to strategically misreport information once it became clear which metrics would determine early access to the program. This information was combined with 2001 Census information on the states' rural population, which was also widely publicly

²¹Rank data is complete for all rural districts in these states. See appendix for further information.

Table 1: Predictive Success of Algorithm for Major Indian States

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

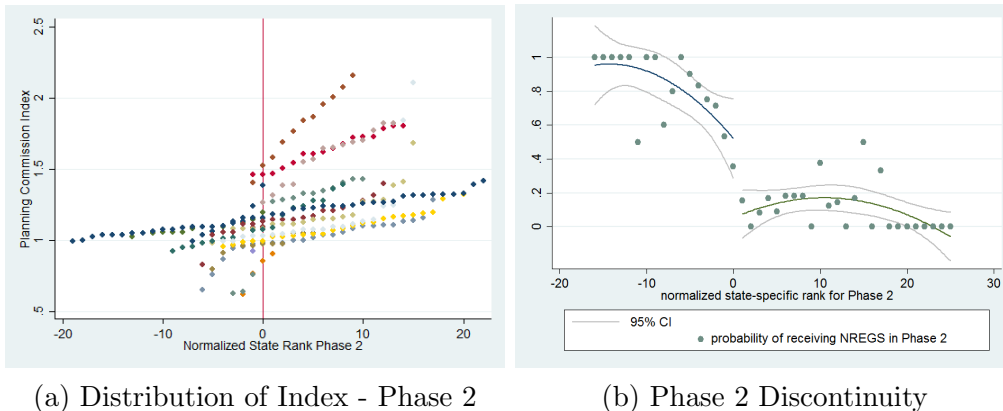
Note: Table includes all districts with non-missing development index value for 17 major Indian states (the only missing districts in these states are urban districts according to the Planning Commission report definition from 2003 and therefore include either the state capital or an urban agglomeration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

available. Second, the creation of the development index from the raw data is done in a transparent way. The Planning Commission report outlines the exact procedure and also lists the raw data for all districts, so that the composite index and the district ranking can be perfectly replicated. Third, a research group at the Planning Commission of India had created the algorithm long before NREGS became a law and it had only been used for a few much smaller, temporary anti-poverty programs before NREGS. This means that even if manipulation of the running variable had been possible, the much lower cutoffs for earlier programs would have led to manipulation at very different parts of the distribution.²²

²²Cutoffs for these earlier programs were at 100 and 150 districts, whereas the cutoff that my empirical analysis focuses on is for the 330 poorest districts.

Figure 1a shows that the distribution of index values over ranks is smooth, with no suspicious bunching of algorithm values in the vicinity of the cutoff. This suggests that manipulation is not a big concern.²³

Figure 1: Distribution of Index and Discontinuity



Note: First figure plots the distribution of the index by state. Second figure shows the treatment discontinuity for Phase 2, dropping the phase far away from the cutoff (Phase 1). Negative and zero normalized state rank numbers are districts that should have received NREGS based on the government algorithm, whereas positive numbers are assigned to districts that should have been ineligible.

While it is common in the RD literature to analyze potential manipulation using the McCrary test, this test is inconsistent for a discrete running variable as in the case of NREGS.²⁴ Another way of analyzing whether manipulation is likely to be a problem is to test whether there are discontinuities at the cutoff in the baseline data: if the RD specification is valid, we would expect baseline outcomes to be smooth at the cutoff. Appendix Table B.1 reports the results of such tests for the outcome variables used in this paper as well as for five other outcomes for all parametric specifications of the RD estimator. The estimates show that only one out of the 40 coefficients is statistically significant. Again, widespread manipulation of treatment assignment seems unlikely based on these results. To soak up residual variance, the parametric estimation results in this paper control for the baseline outcome variable, but the estimated coefficients are not substantially affected by the exclusion of the baseline

²³Appendix figure B.1 shows the general distribution of index values over ranks before they are made state-specific. Appendix Figure B.2 provides a larger version of Figure 1a.

²⁴See e.g. Frandsen (2017).

controls. The Online Appendix reports the results from additional baseline tests using a number of demographic, geographic and political variables, and again finds no evidence of systematic differences between the implementation phases at baseline.

Even if districts were unable to directly manipulate the algorithm, a related threat to internal validity occurs if the misclassification is introduced in the construction of the algorithm values. This concern does not apply to the second step of the algorithm, since raw data and development index values are directly available from the Planning Commission report, so I use exactly the same information that the government had access to when constructing the benefit calculator. For the first step of the calculator, I use data published by the Planning Commission in 2009, based on information that was available at the time of NREGS introduction. India had created three new states in the early 2000s, which did not exist when the original data used for the algorithm was collected in the 1990s. While the 2009 data on state poverty headcount ratios is very likely to be the information that was used by the Planning Commission in the first step of the algorithm, the Online Appendix provides additional details on this point and its potential consequences.

With a fuzzy RD design, we need to verify that there is indeed a discontinuity in the probability of receiving NREGS at the cutoff, and Figure 1b shows that this holds. It plots the probability of receiving NREGS for each bin, as well as fitted quadratic regression curves and corresponding 95 percent confidence intervals on either side of the cutoff. The graph shows that the average probability of receiving NREGS jumps down at the cutoff and that the magnitude of the discontinuity is estimated to be around 50 percent. This suggests that there is indeed a discontinuity in the probability of being treated.

While not a concern for the internal validity of the results, the prediction success rate of about 82 percent could be a concern for the representativeness of the results if deviations from the algorithm are correlated with other district characteristics. The Online Appendix provides detailed information on the question why the prediction success rate for the government algorithm was not higher. Deviations in the algorithm are most likely driven by

the realities of the political negotiation process, which necessitated a tradeoff between the advantages of a clear-cut rule and the benefits from tailoring the district allocation to political considerations. As the Online Appendix also shows, however, there is no discontinuity in the political party in power at the cutoff at baseline, so this does not undermine the internal validity of the RD approach.

4 Data and Empirical Specification

4.1 Data and Variable Creation

The data used in this paper comes from household surveys collected by the National Sample Survey (NSS) Organisation. These surveys are representative of the Indian population, and drawn from the population in a two-stage stratified sample design. The dataset used for the analysis is the 64th round (July 2007-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 of three months. In April 2007, NREGS had just been rolled out to Phase 2 districts. 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.²⁵ To analyze the labor market impacts of NREGS, I 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 socio-demographic characteristics. Additionally, a representative 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 the dataset is a sub-round panel with up to four observations per round at the district level.

I restrict my sample to prime-age men (18-60 years) with at most secondary education

²⁵See e.g. Imbert and Papp (2015). The results are qualitatively the same when these potentially contaminated control group observations are excluded.

living in rural areas. The education restriction is used to make the results in the paper more easily comparable to other papers such as Imbert and Papp (2015) that have also used this restriction, but the results are robust to not restricting the sample on education. The employment module asks detailed questions about an individual’s work status in the last 7 days, which I use to create employment variables, focusing on casual jobs. Employment measures are dummy variables equal to 1 if an individual worked in a public-sector job, a private-sector casual job or in family employment, and 0 otherwise. Casual private-sector employment and family employment combined add up to total private-sector employment. 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 variables up to the district-sub-round level using sampling weights.²⁶ Data from the 61st round (July 2004-June 2005) is used as baseline information.

4.2 Empirical Specification

The main results report estimates from two different RD approaches: the standard parametric estimation and a meta-analysis approach. Both approaches have distinct advantages in the NREGS case and at the same time test the robustness of the estimates to different empirical specifications. Ideally, the estimation just uses observations in the vicinity of the cutoff and estimates the discontinuity using local linear regressions. In the case of NREGS, the number of observations close to the cutoff is limited, however: One observation corresponds to a district rather than to an individual or household, and the running variable is discrete due to the poverty ranking. This leads to a tradeoff between precision and bias: The larger the window of observations used in the regressions, the more precise the estimates are likely to be, but this implies that observations further away from the cutoff are used, which may bias the estimate at the cutoff (Lee and Lemieux, 2010). The main results use parametric polynomial estimations with all available relevant observations after dropping

²⁶The results are also robust to collapsing the data to the district-year level instead.

Phase 1 districts (that by design are far away from the analyzed cutoff between Phase 2 and Phase 3 districts).

Concerns about potentially biased estimates are addressed in two ways that are well established in the parametric RD literature: First, figures non-parametrically plot the relationship between the running variable and outcome variables of interest. The graphs in Figures 2a to 2d use the method suggested in Calonico et al. (2015) to optimally undersmooth bins and show quadratic regression functions on both sides of the cutoff. Regressions are run through all of the underlying data points. For easier comparison with the table results, the figures plot the residualized relationship between the running variable and the outcome variable of interest after controlling for the outcome variable at baseline and state fixed effects. In general, a quadratic function fits the data quite well. These patterns suggest that using the whole sample of Phase 2 and Phase 3 districts and estimating the treatment effect at the cutoff using linear and quadratic functions of the running variable is not a bad approximation of the underlying data.

Second, in addition to local linear and quadratic regressions (where the slope of the regressions lines is allowed to differ on both sides of the cutoff), the appendix shows the robustness of the results for two other parametric specifications (linear, quadratic). Using F-tests I cannot reject the null hypothesis that other higher-order polynomial terms are irrelevant, and the estimated coefficients are usually qualitatively similar. Additionally, Gelman and Imbens (2014) discourage the use of higher-order polynomials.

The parametric approach estimates the following regression equation:

$$y_{ijk} = \beta_0 + \beta_1 nregs_{ik} + \beta_2 baseline_{ijk} + f(rank, nregs) + \eta_k + \epsilon_{ijk}$$

y_{ijk} is an outcome variable of interest in district i and season j and state k , and the coefficient of interest is β_1 . $f(\cdot)$ is a function of predicted NREGS receipt $nregs$, and the district's rank based on the state-specific normalized index $rank$.²⁷ To soak up residual

²⁷The reported results are therefore intent-to-treat results. The alternative is to instrument actual treatment receipt with predicted NREGS receipt, which leads qualitatively similar results.

Table 2: Summary Statistics for Districts at Baseline by Phase (Men)

	Men			
	phase 2		phase 3	
		N		N
private employment	0.2975	396	0.2938	668
family employment	0.5810	396	0.5271	668
public employment	0.0038	396	0.0015	668
daily wage (total)	52.75	387	65.71	645
daily wage (private)	52.77	386	65.78	645
daily wage (public)	53.44	18	63.54	22

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

variance, the regression also controls for the baseline outcome variable and state fixed effects η_k . Standard errors are clustered at the district level.

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

4.3 Summary Statistics

Table 2 presents baseline employment and wage summary statistics for districts separately by phase. 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 wage is about 66 rupees in Phase 3 districts. Private-sector daily wages are very similar to overall casual daily wages, and there is no substantial difference between public-sector and private-sector wages.

It is very uncommon to work in the public sector in all districts: 0.4 percent of workers

work there in a typical Phase 2 district in the week prior to the survey, and the corresponding number for Phase 3 districts is 0.2 percent. In contrast, in all districts about 30 percent of males work in private casual jobs, and about 58 percent in a family business or on the family farm. The remainder are males who are unemployed or out of the labor force in a given week.

5 Conceptual Framework and Testable Predictions

The summary statistics show that the typical rural man has three employment options: public employment, private casual employment and family employment. Before NREGS was implemented, public employment was not a regular option, and the baseline information reveals that less than one percent of workers spent any time working for the government. Most men therefore work either in a casual job in the private sector or in self-employment in a family business or on the family farm, with almost twice as many men working in family employment than in the private sector.

How NREGS should be expected to influence employment outcomes in these rural areas depends on the labor market structure and on the characteristics of private casual jobs and family employment in terms of availability, desirability and riskiness. Based on the program characteristics, NREGS is a flexible program that can be taken up either as an alternative form of employment or as an insurance tool after negative shocks. Since the program officially guarantees 100 days of public employment at any time of the year, workers may choose to work in a public-works program instead of in a casual private job, for example, or they could take up NREGS when there are few employment opportunities in the private sector or in family employment.

As a new type of employment opportunity, NREGS has two attractive features: First, workers under the program should earn the minimum wage, which in practice is typically higher than the actual equilibrium wage paid in the private casual sector since minimum

wage laws are not well enforced.²⁸ Second, work in a public-works project may be less risky than family or private employment, since duration and wage payments are likely to be known beforehand and do not depend on seasonality or weather conditions.

If NREGS functions as an alternative form of employment, we should therefore expect public employment to increase substantially after the introduction of the program. This may crowd out private or family employment if NREGS is the more attractive job, but does not have to if NREGS functions as a precautionary savings tool that allows workers to earn money from a secure job in less time than before and thereby enables them to spend more time in riskier activities such as family employment. We may therefore expect NREGS to be particularly attractive as an employment opportunity in districts that face a lot of income volatility due to rainfall variation. In addition to looking at the impact of NREGS on employment outcomes during normal times, the empirical analysis will therefore look at whether there is heterogeneity based on the rainfall variance of districts.

The second potential function of NREGS is as an insurance tool after negative economic shocks. Since NREGS is supposed to be available at any point during the year, public-works jobs can be used to earn income after a shock has hit. In the rural areas in which NREGS operates, an important source of negative shocks is likely to be the weather, which influences the employment and income opportunities in agriculture. If NREGS functions as a safety net after a shock, we would expect take-up to increase after negative shocks. This can be tested empirically.

If households know that they have access to the safety net after a shock, this may also lead to a re-optimization of the time spent in various employment opportunities during normal times because NREGS reduces the riskiness of other forms of employment. This could be especially true for family employment. Such an effect would therefore imply that take-up of NREGS should be low in normal times, but that a re-optimization between existing employment opportunities may occur.

²⁸For the majority of individuals in the sample, the reported private casual wage is below the official minimum wage in that area.

All of these potential impacts of NREGS can be tested empirically with the available data. The appendix also describes a simple household optimization model that works through the potential impacts of the two functions of NREGS and their empirical predictions more formally. Implementation problems with NREGS in the form of rationing or underpayment of wages due to corruption will attenuate the impacts of the program since they make NREGS less attractive as an employment opportunity and as an insurance tool.

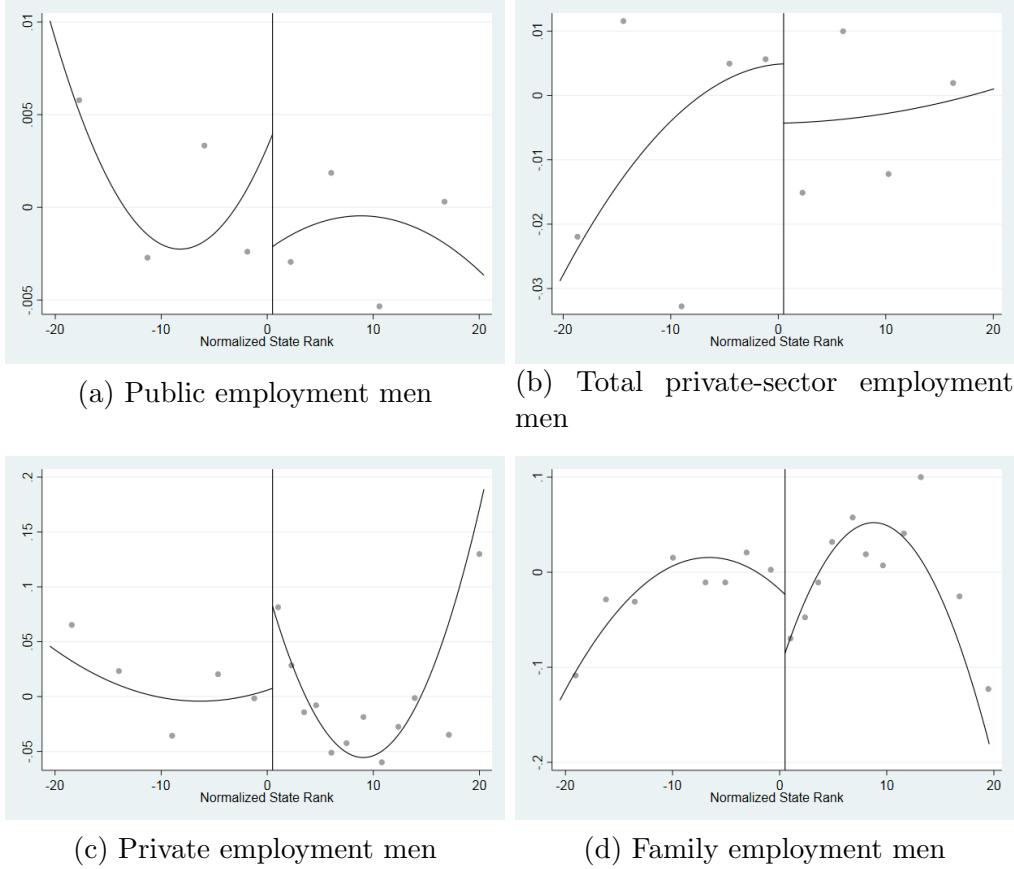
6 Results

6.1 Main Results

Figures 2a to 2d focus on four main outcome variables: public employment, private employment, family employment, and total private-sector employment, which is the sum of private and family employment. Figures 2a and 2b provide visual tests for the take up of public employment after the introduction of NREGS as well as for a crowding out of total private-sector employment. While public employment appears to be slightly higher in Phase 2 districts than in Phase 3 districts at the cutoff, the magnitude of this effect is extremely small and it is difficult to spot a discontinuity in the scatter points at the cutoff. A similar picture emerges for total employment. These results suggest that there is no economically meaningful take-up of NREGS after the program goes into effect, and no crowding out of private-sector employment as a whole after the introduction of the safety net.

In contrast, graphs 2c and 2d show larger impacts of NREGS on employment in the two sub-categories of total employment, private casual employment and family employment. The probability for the average man in a district to have worked in private employment last week drops in early NREGS treatment districts compared to similar non-treated districts, whereas the effect on family employment works in the opposite direction. Despite low actual take-up of the program, NREGS therefore seems to have had an effect on the kind of private-sector job rural workers choose.

Figure 2: RD Results



Note: The graphs show a scatter plot of binned rank observations as well as quadratic regression lines, which are estimated on the underlying data points rather than on the bins. The bins are created using Calonico et al. (2015)’s approach to optimally undersmooth bins.

Table 3 presents the main results in table format using two different approaches to estimating the RD effects. Each cell presents the impact of NREGS on an outcome variable of interest. Panel A reports the results using the parametric RD approach with linear and quadratic polynomials. As in the figures, the impact of NREGS on public employment, which directly includes NREGS employment, is very small and imprecisely estimated. Being in a NREGS district increases the average rural prime-aged man’s probability of having had a public-works job in the last 7 days by 0.16 percentage points in the linear specification and 0.22 percentage points in the quadratic specification. While this translates into an increase of about 20 percent relative to the outcome mean of 0.76 percent, this is not a large effect. Sim-

ilarly, there is no evidence of a large impact on total private-sector employment. Again, this suggests that there is no crowding out of private-sector employment opportunities because of NREGS.

Table 3: NREGS Impact: Employment (Men)

Specification	Panel A: Parametric			
	public	total	private	family
Linear flexible	0.0016 (0.0043)	0.0064 (0.0194)	-0.0420* (0.0227)	0.0465* (0.0263)
Quadratic flexible	0.0022 (0.0053)	0.0015 (0.0251)	-0.0665** (0.0309)	0.0646* (0.0369)
N	863	863	863	863
outcome mean	0.0076	0.8136	0.3173	0.4963
Specification	Panel B: Meta-Analysis			
	public	total	private	family
Linear	-0.0016 (0.5661)	0.0075 (0.6085)	-0.0299* (0.0632)	0.0374* (0.0606)
Quadratic	-0.0003 (0.9298)	0.0115 (0.4581)	-0.0501*** (0.0051)	0.0616*** (0.0067)
N	863	863	863	863

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ An observation is a district in a given season. An employment outcome is the proportion of working-age adults in rural areas working in a given type of employment in the last 7 days. Parametric regressions with different levels of flexibility are reported in Panel A, using clustered standard errors at the district level. For the meta-analysis specification, treatment effects at the cutoff are estimated separately by state and then combined through a population-weighted average, with p-values of the null hypothesis test reported in parentheses.

Columns 3 and 4 of Panel A show, however, that the introduction of the safety net program seems to lead of a re-optimization of the jobs taken up by male workers. In the quadratic specification, the probability of an average male worker in a district having worked in private casual employment in the last week drops by 6.65 percentage points, whereas the probability of engaging in family employment increases by about the same magnitude.

Panel B shows the results for a meta-analysis approach, where the treatment effect for each outcome variable is first estimated separately for each state, and those estimates are then combined by using population weights. Estimates obtained by a simple average of the results are reported in Appendix Table B.2. Since the number of observations for an individual state is often small and more flexible specifications are often highly collinear with the treatment variable, Panel B reports the results for polynomial specifications that constrain the slope to be the same on both sides of the cutoff, with p-values for the significance of the population-weighted average reported in parentheses. The results of this estimation technique are qualitatively very similar to the parametric approach. They again point to men leaving the private casual employment sector to take up family employment, while the impacts on public employment and on the sum of private causal and family employment are again imprecisely estimated.

Overall, the results in Table 3 therefore show that the employment impacts of NREGS are robust across two different RD approaches. There is no economically significant increase in public employment after the introduction of NREGS, but male workers leave private casual employment mostly in favor of family employment.

Table 4: NREGS Impact and Risk: Employment (Men)

Specification	Panel A: Parametric			
	public	total	private	family
Linear flexible	-0.0057 (0.0049)	0.0132 (0.0233)	-0.0511* (0.0270)	0.0613** (0.0299)
NREGS*high risk	0.0128*** (0.0046)	-0.0119 (0.0175)	0.0152 (0.0226)	-0.0252 (0.0258)
Quadratic flexible	-0.0049 (0.0059)	0.0081 (0.0290)	-0.0760** (0.0330)	0.0796** (0.0393)
NREGS*high risk	0.0128*** (0.0046)	-0.0120 (0.0175)	0.0149 (0.0226)	-0.0250 (0.0258)
N	863	863	863	863
outcome mean	0.0076	0.8136	0.3173	0.4963

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ *high risk* is a dummy variable equal to 1 if a district has a higher than median variance of rainfall at baseline. An observation is a district in a given season. An employment outcome is the proportion of working-age adults in rural areas working in a given type of employment in the last 7 days. Parametric regressions with different levels of flexibility are reported using clustered standard errors at the district level.

Table 4 analyzes whether NREGS take-up is higher in districts with more unpredictable rainfall patterns. If NREGS allows workers to move to family employment because the program provides partial insurance after a negative rainfall shock, this safety net compensates more fully for income losses in less risky districts. Since households know that they live in a riskier area, their demand for a buffer stock should be higher. High risk districts are defined as those with a higher than median variance in rainfall at baseline, which proxies for income volatility. Column 1 of Panel A shows that public employment is statistically significantly higher in high-variance districts, whereas there is not much take-up (and even some evidence of a public employment decrease) in low-risk treatment districts relative to control districts. Qualitatively, private casual employment is higher and family employment lower in high-risk districts, although the effect is not statistically significant. This suggests that workers in high-risk and low-risk districts are willing to take on more risk by moving to family employment after the introduction of the program, but that workers in high-risk districts also have a higher take-up of NREGS. This is consistent with using the scheme to build up a buffer stock for bad shocks.

Table 5 tests whether the employment guarantee scheme actually functions as a safety net after a negative income shock. The specifications focus on districts during the agricultural off-season (January to June), but consider rainfall shocks that occurred at the beginning of the previous agricultural season in the months of July to September, which roughly corresponds to the monsoon season. This gives the rainfall shock some time to feed through to household incomes. The main treatment variable is interacted with an indicator variable equal to one if a district experienced a negative rainfall shock (lower than expected rainfall) in the agricultural main season.

Table 5: NREGS Impacts and Safety Net (Men)

Specification	Panel A: Parametric			
	public	total	private	family
Linear flexible	-0.0048 (0.0101)	0.0334 (0.0270)	-0.0462 (0.0317)	0.0782** (0.0347)
NREGS*negative shock	0.0309* (0.0157)	-0.0420 (0.0344)	-0.0219 (0.0350)	-0.0206 (0.0432)
Quadratic flexible	-0.0059 (0.0127)	0.0278 (0.0374)	-0.0708 (0.0442)	0.0973** (0.0494)
NREGS*negative shock	0.0320* (0.0163)	-0.0408 (0.0349)	-0.0195 (0.0349)	-0.0216 (0.0431)
N	432	432	432	432
outcome mean	0.0130	0.8069	0.3282	0.4787

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ *negative shock* is a dummy variable equal to 1 if there was a negative deviation of rainfall from expected rainfall during the last monsoon season. Sample is restricted to agricultural off-season. An observation is a district in a given season. An employment outcome is the proportion of working-age adults in rural areas working in a given type of employment in the last 7 days. Parametric regressions with different levels of flexibility are reported using clustered standard errors at the district level.

As column 1 shows, take-up is indeed substantially higher after adverse shocks, rising by about 3 percentage points, whereas the effect is small and statistically insignificantly different from zero after positive rainfall shocks. The sum of the main effect and the interaction effect with the negative shock is always statistically significantly different from zero at conventional levels.

Taken at face value, the increase in public employment after a bad rainfall shock in treatment districts comes at the cost of total private-sector employment, although the coefficients are imprecisely estimated. More likely, however, there are just fewer private-sector jobs available after a bad rainfall shock than during good times.

6.2 Discussion and Extensions

As mentioned above, NREGS can fulfill two functions in rural areas: First, it provides an alternative form of employment, since NREGS work should be offered at any time of the year. Working for the public-works program therefore offers new job opportunities in addition to private casual and family employment and should be less risky in terms of employment availability and wages than the existing alternatives. In many areas, the minimum wage stipulated under NREGS is higher than the equilibrium wage in the private casual sector at baseline, which should make NREGS employment especially attractive. This would suggest that take-up of NREGS should be high and at least partially crowd out private casual employment, while the effect on family employment is ambiguous.

Second, NREGS may work predominantly as a safety net after bad economic shocks. In that case, take-up of NREGS may be low in normal times, but should rise after bad shocks. Since households know that they have access to the safety net, this may lead to a re-optimization of time allocation even in normal times, however: If family employment is preferred to private casual employment by households but not taken up because it is perceived to be too risky, then the introduction of NREGS should lead to a decrease in private casual employment and a corresponding increase in family or self-employment.

In terms of the employment effects, the results from Tables 3 to 5 suggest that NREGS works as an alternative form of employment in certain situations, but in general is more effective as a safety net. The results from Table 3 are consistent with an ex ante safety net effect: There is no large impact of NREGS on public employment during normal times in the typical district. Across two different RD approaches, the estimated coefficients are very small, and increases larger than one percentage point at the cutoff can typically be ruled out even for the imprecisely estimated coefficients. But there is robust evidence that men leave the private casual sector to move into family employment, which is consistent with the idea that the availability of NREGS after bad economic shocks lowers the relative riskiness of family employment and leads men to leave the private casual sector. On the other hand, the estimates provide no support for the idea that NREGS is predominantly taken up as a new alternative form of employment. Table 5 supports the safety net function by showing that the ex post effect works as well: After a bad rainfall shock, take-up of NREGS increases.

The results from Table 4 are consistent with a role of NREGS as alternative employment, however. In high-risk districts, take-up of NREGS is substantially higher than in low-risk areas. Since households know that they live in riskier areas, their demand for a buffer stock should be higher, especially if NREGS cannot provide full insurance after bad shocks and if there is rationing in NREGS or private-sector employment so that the buffer-stock demand for work cannot be met by just working in the higher-paying alternative. The results from Table 4 also suggest, however, that as in the overall sample male workers re-optimize their time since they have access to NREGS in case of a negative shock.

Overall, the employment effects of NREGS therefore support the idea that there is a role for NREGS as an additional employment opportunity in rural areas with high rainfall variability, which proxies for income variability, but that in general NREGS is more effective as a safety net. NREGS only has a limited direct influence on the Indian rural employment in the form of direct take-up. Instead, NREGS seems to work as an insurance tool that reduces the riskiness of family employment relative to casual private-sector work, even though the

risk heterogeneity results suggest that buffer stock considerations are not completely absent, either.

Appendix Table B.3 tests whether the introduction of NREGS affects per-capita expenditures, the total wage or the private wage. Since many workers are self-employed, per-capita expenditures provide a better estimate of household income. The private wage captures the wage in cash or in kind earned by workers in the private casual sector, whereas the total wage is the sum of the private wage any wages from public employment. As the table shows, the safety net does not generate substantial welfare benefits in the form of higher per-capita expenditures, but also does not positively affect workers who remain in the private casual sector through higher wages. If anything, the estimated wage coefficients are typically negative, although imprecisely estimated. Appendix Table B.4 also documents results corresponding to Tables 3 and 5 for women and shows that there is not much evidence for female employment impacts.

One potential explanation for this finding is that such welfare effects may take longer to be realized. The analysis in this paper is limited to the first year of NREGS implementation because of data limitations and since the program is rolled out to control districts afterwards. Medium- to long-term benefits of NREGS can therefore not be captured. Even if there are no household expenditure impacts, however, the program may have substantial welfare implications through the occupational changes and may therefore alter the unobserved utility households derive from employment. Additionally, it is possible that the effectiveness of NREGS is underestimated since the empirical analysis focuses on comparing Phase 2 and Phase 3 districts, which by design are less economically underdeveloped than Phase 1 districts. Unfortunately, it is impossible to compare Phase 1 to Phase 2 districts at the corresponding treatment discontinuity due to data limitations: Both Phase 1 and Phase 2 districts are treated at the time the employment data is collected, and there are no comparable large-scale and high-quality datasets available that would allow an analysis of the employment effects of this earlier implementation phase. It is therefore possible that NREGS

functions more heavily as an alternative form of employment in Phase 1 districts, but that the insurance function of NREGS is more important in the richer districts considered in this paper.

Maybe most surprising about the empirical patterns in this paper is the fact that a large-scale public-works program like NREGS does not seem to significantly increase the working-age population's probability of having held a public-works job in the past 7 days. Mean public employment is only 0.69 percent for men in Phase 2 and Phase 3 districts. So while some of the estimated coefficients are equivalent to large increases in public employment in percentage terms, the empirical analysis can usually rule out increases in public employment above 1 percentage point even in the less precisely estimated specifications.

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

According to administrative records, the employment guarantee scheme provided 1.4 billion person-days of employment in 1.78 million projects in the 330 Phase 1 and Phase 2 NREGS districts in 2007-2008.²⁹ 61.15 percent of this employment was given to women. The average daily wage paid was 75 rupees (about \$1.8). This means that in a typical week, the scheme generated 83677 workdays of employment in 104 projects in the average district. With an average prime-aged district population of 1.10 million people, this translates into

²⁹The 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.

0.0764 NREGS workdays per week per person. In the NSS data, the number of public-works workdays in Phase 1 and Phase 2 districts are 0.0789 for prime-aged adults, or about 4 days of public employment per person per year. This means that the NREGS employment generated for the chosen sample of prime-aged adults in this paper is in the same ballpark as that suggested by administrative sources, and is low at the local level: the implied weekly number of NREGS workdays per prime-aged adult in the average district would be 0.9615, for example, if we assume that 50 percent of workers have a NREGS job for 100 days per year. These back-of-the-envelope calculations therefore support the public employment results in this paper in that generated employment opportunities seem to be relatively modest at the local level.³⁰

For the safety net explanation of the results to be plausible, workers must have believed that they would really benefit from increased public employment opportunities after a negative shock and to have been willing to take on more risk after the introduction of NREGS. The Online Appendix provides qualitative and circumstantial evidence that NREGS was likely perceived very differently from previous schemes: Social activists, NGOs and worker organizations had campaigned for the introduction of an employment guarantee program and had been heavily involved in shaping its details. NREGS differs from previous government anti-poverty initiatives because of the legal character of the scheme and its massive scope, and NGOs and social activists were heavily involved in monitoring the program from the beginning. One of the main arguments by social activists for a program like NREGS was exactly the point that what rural households needed was a flexible program that would be available to workers in a predictable manner. All of this means that workers are likely to have perceived NREGS as very different from previous failed development initiatives.³¹

³⁰Another way of scaling the public-employment impacts is to calculate the annual increase in NREGS employment implied by the regression results. Taking the RD estimates for public employment from the Phase 2 vs Phase 3 regressions of Table 3 literally, they imply a 6 percentage point increase in public employment per year. According to administrative data, the average person worked 42 days in that year. This implies that about 1 percent of a district's population had a NREGS job at some point over the course of the year.

³¹Please see Online Appendix for additional details.

While it is difficult to establish with existing data sources whether workers are really taking on more risk after the introduction of NREGS, Hari and Raghunathan (2015) use the RD approach developed in this paper and show that the introduction of NREGS increases the riskiness of the portfolio of crops grown, which is consistent with the time-allocation effects found in this paper and the willingness of households to take on more risk since NREGS functions as a safety net.

6.3 Robustness checks

A couple of alternative specifications can be used to test the robustness of the main results. Appendix Table B.2 reports the results for varying specifications for the two approaches used in Table 3: Panel A shows the results using different parametric specifications than those used in Table 3.³² Panel B weights the state-specific treatment effect equally in the meta-analysis approach, in contrast to the population weights applied in Table 3. Appendix Table B.2 reveals that the main results are robust to all of these changes.

Another alternative RD approach is the local randomization approach suggested by Cattaneo et al. (2016a). In contrast to the more traditional approaches, which assume that all variables other than the treatment are smooth and monotonic at the cutoff, local randomization assumes that given the appropriate choice of an estimation window close to the cutoff, observations on both sides of the cutoff can be treated as observations where treatment is randomly assigned. Estimation then proceeds by estimation strategies commonly used in experiments. The chosen estimation window is the largest window in which the hypothesis of balanced baseline variables cannot be rejected, which is similar to a balance table test in an experiment. This approach has the advantage that it zooms in on observations close to the cutoff, and test statistics are adjusted to deal with small sample sizes in the chosen windows (Cattaneo et al., 2016a,b).

³²Varying the bandwidth in the parametric approach leads to qualitatively similar results as in Table 3, although consistent with the tradeoff between precision and bias, the coefficients tend to be more imprecisely estimated with lower bandwidth choices.

Figure B.3a shows the minimum p-value for any variable used in the baseline test for different windows around the cutoff.³³ To be conservative, the horizontal dotted line, which corresponds to a p-value of 0.15, is used as a minimum acceptable p-value in the balance test. The graph reveals that the covariates are balanced for small windows around the cutoff, with a suggested window of $[-3,5]$ for the Phase 2 allocation. The cutoff occurs between rank 0 and 1, so a suggested window length of 4, which is slightly above a p-value of 0.15, corresponds to the window of $[-3,5]$. To be more conservative, the analysis uses a window of $[-2,4]$, which corresponds to a window length of 3. To take into account potential clustering, standard errors are obtained by bootstrapping.³⁴ Appendix Table B.5 presents a typical baseline balance table, and shows that predicted Phase 2 and Phase 3 districts are balanced at baseline on labor-market characteristics. Appendix Table B.6 shows that the main results from Table 3 are also robust to using the local randomization approach.

The main results are also robust to a number of other specifications not reported here, like the exclusion of baseline outcome variables in the parametric regressions, the inclusion of additional control variables and the exclusion of potentially contaminated Phase 3 districts due to the timing of data collection.³⁵ The Online Appendix also shows that the results are robust to dropping the education restriction and to collapsing the data to the district-year level rather than to the district-season level. Additionally, the Online Appendix shows the estimates of a lower and upper bound of the NREGS impacts that make different assumptions about the effectiveness of NREGS in districts where actual treatment status deviates from the one predicted by the algorithm. Since manipulation of the algorithm is unlikely, this addresses concerns about the representativeness of the estimates rather than internal validity concerns. Overall, the main results of the paper are therefore robust to a wide range of alternative specifications.

³³The baseline variables include public, family and private casual employment, landholding, log per capita expenditures, the log daily wage and years of education.

³⁴Cluster bootstrapping does not change the p-value of the results in the very large majority of cases, supporting the assumption of random assignment in the chosen assignment window.

³⁵Phase 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.

7 Conclusion

Using a regression-discontinuity design, this paper has analyzed the impacts of the National Rural Employment Guarantee Scheme (NREGS) on rural employment outcomes. The results suggest that the overall direct effects at the treatment discontinuity are small. The general qualitative pattern is robust across a wide range of empirical specifications and different RD estimation techniques: The introduction of the public-works program at best only leads to small increases in public employment. Workers seem to be leaving the private casual employment sector and move into family employment. The NREGS employment impacts are also statistically significantly higher after a negative rainfall shock.

Overall, these results suggest that NREGS works as a safety net, although this does not translate into substantial improvement in other variables like per-capita expenditures, at least in the short run. The results are also consistent with NREGS indirectly subsidizing self-employment activities by making them less risky. NREGS here mainly functions as an insurance tool after bad economic shocks rather than as a way of accumulating precautionary savings. These indirect program effects may have substantial welfare implications that are typically not captured in the debate, and may mean that programs are not ineffective in altering the living situation of the poor despite low actual take-up.

In the presence of widely documented implementation problems like rationing, NREGS may disproportionately benefit the poor who have the option of becoming self-employed rather than the most economically vulnerable households with few employment alternatives. Broader welfare benefits therefore depend heavily on improving implementation quality, although welfare improvements through the safety net may also take more time to materialize than could be analyzed in this paper. In general, the short-run impacts of NREGS analyzed here will underestimate the longer-run effects if implementation quality has increased over the years. On the other hand, the empirical results also suggest that NREGS may be providing important benefits to workers in the form of a safety net without large distortions in the labor market, which potentially makes the program a more attractive tool for distributing

benefits to the poor than some other government interventions.

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AppendixA Appendix (Not for Publication): A Model of the Household Optimization Problem

AppendixA.1 The Baseline Model without NREGS

The model describes a household's optimal time allocation in a one-period setting. Before NREGS is introduced, a household can first choose to allocate the total time of their household members, T , between working for a big landowner as agricultural laborer in the private casual sector, l , and working on the family farm, f .³⁶ After this decision has been made, a weather shock is realized that determines the payoff from farm work. The period ends, and the household earns the fixed wage w in the private sector, and income y for the time spent in farming. The household derives utility both from the time spent working in self-employment on the family farm, and from the total income earned in both activities during the period. The utility function is additively separable in these components, with $u' > 0, u'' < 0, v' > 0, v'' < 0$, and u and v satisfy the Inada conditions. Weight α is the weight given to the utility from self-employment.

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

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

Which leads to the first-order condition

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

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

³⁶Implicit in this setup is the assumption that a household has perfect control over l or, put differently, that the household can always get a job in the private sector at wage w for the desired duration. One period in this framework is thought of as an agricultural year, which includes peak times like planting and harvesting. While 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.

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

Proof. Since u and v are concave and satisfy the Inada conditions, an interior solution exists. The right-hand side of (4) is decreasing in l , whereas the left-hand side of (4) is increasing in l . By the intermediate value theorem, there must therefore be a value of l at which the first-order condition is satisfied. ■

Appendix A.2 The Model with NREGS

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

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

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

Where n_2^* is the best-response function of n_2 given y since the household can optimize the time spent working for NREGS and actually working on the family farm after the weather

shock has occurred and y has been realized. Once a household chooses the fraction of time to spend on NREGS employment after the weather shock has occurred, l , n_1 , and y are fixed. The household therefore chooses n_2 to maximize

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

Leading to the first-order condition

$$\alpha v'(T - l - n_1 - n_2) = (1 - \alpha)u'((T - l - n_1 - n_2)y + n_2\bar{w} + lw + n_1\bar{w})(\bar{w} - y) \quad (\text{A.2})$$

Lemma 2 *There exists a unique optimal amount of time spent in n_2 (NREGS employment as ex-post insurance) for a given y .*

Proof. Since u and v are concave and satisfy the Inada conditions, an interior solution exists. The right-hand side of (5) is decreasing in n_2 , whereas the left-hand side of (5) is increasing in n_2 . By the intermediate value theorem, there must therefore be a value of n_2 at which the first-order condition is satisfied. ■

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

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

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

Lemma 3 *A household will work either in private-sector work l or in ex-ante NREGS employment n_1 , and will work in the job that pays more.*

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

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

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

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

The household maximization problem can therefore be rewritten as

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

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

$$\begin{aligned} & \max_j \int_{y \leq y_0} [\alpha v(T - j - n_2^*) + (1 - \alpha)u((T - j - n_2^*)y + n_2^*\bar{w} + j\tilde{w})]g(y)dy \\ & + \int_{y > y_0} [\alpha v(T - j) + (1 - \alpha)u((T - j)y + j\tilde{w})]g(y)dy \end{aligned}$$

This leads to the first-order condition

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

$$\begin{aligned}
& - \int_{y > y_0} u'((T - j)y + j\tilde{w})(\tilde{w} - y)g(y)dy \\
& = \int_{y \leq y_0} u'((T - j - n_2^*)y + n_2^*\bar{w} + j\tilde{w})(\tilde{w} - y + (\bar{w} - y)\frac{\partial n_2^*}{\partial j})g(y)dy \quad (\text{A.3})
\end{aligned}$$

Lemma 4 *A sufficient condition for the existence of a unique optimal amount of time spent in employment j is that agents are sufficiently risk averse.*

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

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

holds for all possible values of y .

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

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

holds for all possible values of y .

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

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

Notice how the sufficient conditions for a unique solution do not depend on the sign of $\frac{\partial n_2^*}{\partial j}$, which is ambiguous. Intuitively, how the time allocated to the ex-post NREGS employment responds to an increase in the time allocated to precautionary activity j depends on

the attractiveness of the wage for j relative to the NREGS wage \bar{w} and y . In other words, j only functions well as a precautionary savings tool if the paid wage in that activity is not too low relative to the payoffs that can be achieved through NREGS employment and farming after the weather shock is realized. A sufficient condition for j and n_2^* being substitutes for shocks $y \leq y_0$ is $\tilde{w} \geq \bar{w}$.

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

Proposition 5 *If the NREGS wage is high relative to the private-sector wage, the introduction of NREGS completely crowds out private-sector employment.*

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

Proposition 6 *Even if the NREGS wage is low relative to the private-sector wage, the introduction of NREGS reduces the amount of time spent in private-sector employment under reasonable assumptions. Workers spend more time in farm work and, after bad income shocks, in NREGS employment instead.*

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

$$\begin{aligned}
& \alpha v'(T - l) \\
= & \\
& (1-\alpha) \int_{y \leq y_0} u'((T - l - n_2^*)y + n_2^*\bar{w} + lw)(w - y + (\bar{w} - y)\frac{\partial n_2^*}{\partial l})g(y)dy \\
& + (1-\alpha) \int_{y > y_0} u'((T - l)y + lw)(w - y)g(y)dy \\
& - \alpha \int_{y \leq y_0} v'(T - l - n_2^*)(1 + \frac{\partial n_2^*}{\partial l})g(y)dy \quad (\text{A.4})
\end{aligned}$$

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

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

Proposition 7 *NREGS take-up is low on average if the program primarily functions as a safety net tool.*

Proof. This follows from Propositions 5 and 6. If NREGS is primarily used as a precautionary savings measure, NREGS employment crowds out private-sector employment and will be high. If NREGS mainly functions as insurance and the wage is low relative to the private-sector wage, then NREGS is only taken up after bad shocks to y , and will therefore be low in the absence of large negative aggregate shocks. ■

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

So far the model assumes that an agent can perfectly choose the amount of NREGS employment that is optimal for him, be it as a precautionary savings measure n_1 or as a safety net measure n_2 . In reality, NREGS employment is officially capped at 100 days per household

per year. This makes NREGS less attractive both as a risk-mitigation tool and as an ex post insurance mechanism, and will therefore attenuate the labor market impacts of NREGS predicted by the model. An implication of this feature is also that Proposition 10 may no longer hold: If the restriction on the maximum time spent in NREGS employment means that there is much less insurance after exceptionally bad weather shocks than in the absence of this rule, then households living in risky districts will reduce their time spent in private employment l less than agents in less risky districts.

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

The model also assumes that the private-sector wage is fixed regardless of the weather shock. If the private-sector wage also depends on the weather, private-sector employment is a less useful tool for risk mitigation than in the model, which increases the negative impacts NREGS has on private-sector employment. How the private-sector wage changes after the introduction of NREGS depends on the industry structure of local labor markets and on the composition of the workforce, but there is little consensus in the existing literature about the best way of modelling the Indian casual private sector.³⁷ In a standard perfectly competitive setup where employers pay workers their marginal product and the marginal product is decreasing in the number of workers employed, for example, a decrease in the supply of labor because of NREGS will lead to a higher marginal product of labor for the remaining workers

³⁷The models in Basu (2002) and Basu (2013), for example, are built on the existence of two types of workers: those with long-run contracts, and those with short-run contracts. While the papers cite some evidence of the existence of such long-run contracts in some parts of India, other papers like Kaur (2012) argue that daily labor contracts are the norm in Indian rural labor markets. Imbert and Papp (2015) focus heavily on small farmers with simultaneous labor supply and demand decisions.

and therefore to higher wages, which in turn attenuates the negative impact NREGS has on private-sector employment. Wages should also rise if the public-works program practically enforces the existing minimum wage laws.

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

AppendixB Additional Tables and Figures

Table B.1: Baseline Tests

Specification	public	employment		family	log private		education		land	log per cap. expenditure
		total	private		wage	prim.	upper prim.	sec.		
Linear	-0.0006 (0.0024)	-0.0094 (0.0157)	-0.0188 (0.0187)	0.0077 (0.0212)	0.0596 (0.0398)	-0.0037 (0.0137)	-0.0231 (0.0151)	-0.0070 (0.0119)	83.97 (123.03)	-0.0015 (0.0314)
Linear Flexible Slope	-0.0007 (0.0024)	-0.0093 (0.0156)	-0.0187 (0.0187)	0.0077 (0.0212)	0.0596 (0.0397)	-0.0037 (0.0137)	-0.0231 (0.0152)	-0.0069 (0.0119)	80.19 (118.21)	-0.0019 (0.0314)
Quadratic	-0.0009 (0.0023)	-0.0063 (0.0151)	-0.0155 (0.0187)	0.0088 (0.0210)	0.0527 (0.0396)	-0.0030 (0.0137)	-0.0263 (0.0160)	-0.0083 (0.0120)	31.01 (118.39)	-0.0116 (0.0315)
Quadratic Flexible Slope	-0.0013 (0.0040)	0.0038 (0.0215)	-0.0365 (0.0265)	0.0297 (0.0278)	0.0805 (0.0542)	0.0319* (0.0171)	-0.0148 (0.0196)	-0.0171 (0.0156)	51.60 (147.20)	-0.0248 (0.0403)
outcome mean	0.0025	0.8155	0.3109	0.5529	4.0352	0.1831	0.2159	0.1326	1099.63	6.34

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ Standard errors clustered at the district level in parentheses. An observation is a district in a given season in the baseline data (July 2004-June 2005). An employment outcome is the proportion of working-age adults in rural areas working in a given type of employment in the last 7 days. Parametric regressions with different levels of flexibility are reported. The log private wage in column 5 is conditional on private employment. prim., upper prim., and sec. refer to the average proportion of individuals with primary, upper primary, and secondary education, respectively.

Table B.2: Robustness to Other Specifications: Employment (Men)

Panel A: Parametric				
Specification	public	total	private	family
Linear	0.0019 (0.0043)	0.0046 (0.0194)	-0.0418* (0.0230)	0.0443 (0.0269)
Quadratic	0.0015 (0.0043)	0.0090 (0.0197)	-0.0444** (0.0222)	0.0520** (0.0262)
N	863	863	863	863
outcome mean	0.0076	0.8111	0.3173	0.4963

Panel B: Meta-Analysis				
Specification	public	total	private	family
Linear (simple average)	-0.0021 (0.4926)	-0.0046 (0.7498)	-0.0348** (0.0283)	0.0302 (0.1113)
Quadratic (simple average)	0.0029 (0.3468)	-0.0045 (0.7672)	-0.0738*** (0.0001)	0.0693*** (0.0037)
N	863	863	863	863

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ An observation is a district in a given season. An employment outcome is the proportion of working-age adults in rural areas working in a given type of employment in the last 7 days. Parametric regressions with different levels of flexibility are reported. Standard errors clustered at the district level in parentheses for Panel A. For the meta-analysis specification, treatment effects at the cutoff are estimated separately by state and then combined through a simple average, with the p-value of the null hypothesis test reported in parentheses.

Table B.3: NREGS Impacts on Other Outcomes: Expenditures, Total Wage (Men)

Specification	log per-capita expenditures	log total wage	log private wage
Panel A: overall sample			
Linear	0.0404 (0.0351)	-0.0178 (0.0394)	-0.0185 (0.0394)
Linear Flexible Slope	0.0436 (0.0351)	-0.0176 (0.0392)	-0.0181 (0.0392)
Quadratic	0.0448 (0.0357)	-0.0238 (0.0391)	-0.0245 (0.0393)
Quadratic Flexible Slope	0.0691 (0.0513)	-0.0207 (0.0545)	-0.0357 (0.0555)
N	863	815	811
outcome mean	6.4747	4.1324	4.1259
Panel B: rainfall shock			
Linear	0.0405 (0.0388)	0.0217 (0.0515)	0.0277 (0.0514)
NREGS*negative shock	-0.0497 (0.0509)	-0.0340 (0.0690)	-0.0560 (0.0718)
Linear Flexible Slope	0.0444 (0.0388)	0.0228 (0.0512)	0.0291 (0.0512)
NREGS*negative shock	-0.0509 (0.0502)	-0.0351 (0.0695)	-0.0582 (0.0726)
Quadratic	0.0487 (0.0390)	0.0190 (0.0512)	0.0255 (0.0515)
NREGS*negative shock	-0.0501 (0.0505)	-0.0333 (0.0693)	-0.0553 (0.0722)
Quadratic Flexible Slope	0.1015* (0.0580)	0.0093 (0.0735)	-0.0124 (0.0734)
NREGS*negative shock	-0.0653 (0.0510)	-0.0403 (0.0711)	-0.0593 (0.0727)
N	432	411	407
outcome mean	6.5148	4.1972	4.1881

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

Table B.4: NREGS Impact: Wages and Employment (Women)

Specification	Panel A: Overall Effects (Parametric)			
	public	total	private	family
Linear flexible	0.0015 (0.0051)	0.0230 (0.0311)	0.0033 (0.0175)	0.0234 (0.0265)
Quadratic flexible	-0.0041 (0.0050)	0.0229 (0.0428)	0.0035 (0.0232)	0.0229 (0.0365)
N	863	863	863	863
outcome mean	0.0064	0.3656	0.1366	0.2290
Specification	Panel B: Safety Net Impacts (Parametric)			
	public	total	private	family
Linear flexible	-0.0062 (0.0094)	0.0218 (0.0358)	0.0158 (0.0246)	0.0097 (0.0300)
NREGS*negative shock	0.0255 (0.0177)	-0.0112 (0.0472)	-0.0262 (0.0301)	0.0105 (0.0396)
Quadratic flexible	-0.0207* (0.0111)	0.0422 (0.0479)	0.0220 (0.0313)	0.0240 (0.0412)
NREGS*negative shock	0.0299 (0.0183)	-0.0153 (0.0477)	-0.0276 (0.0291)	0.0080 (0.0397)
N	432	432	432	432
outcome mean	0.0115	0.3482	0.1356	0.2126

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ Standard errors clustered at the district level in parentheses. An observation is a district in a given season. An employment outcome is the proportion of working-age adults in rural areas working in a given type of employment in the last 7 days. Parametric regressions with different levels of flexibility are reported. In Panel B, *negative shock* is a dummy variable equal to 1 if there was a negative deviation of rainfall from expected rainfall during the last monsoon season. Sample is restricted to agricultural off-season.

Table B.5: Baseline Balance Test Phase 2 vs Phase 3 (Men)

Variable	(1) Phase 2	(2) Phase 3	(3) Difference
public employment	0.003 (0.024)	0.003 (0.024)	-0.000 (0.003)
private employment	0.322 (0.162)	0.321 (0.193)	-0.001 (0.027)
family employment	0.546 (0.201)	0.528 (0.204)	-0.018 (0.032)
log private daily wage	3.997 (0.332)	4.068 (0.481)	0.071 (0.075)
log per-capita expenditures	6.266 (0.254)	6.306 (0.267)	0.040 (0.048)
land	906.245 (705.725)	947.089 (713.502)	40.844 (115.623)
Observations	184	196	380

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ Data source: National Sample Survey of India (2004-5) - Employment and Unemployment Module. Data is restricted to districts with poverty rank values of $[-2,4]$, the main analysis window for the local randomization approach. Baseline variables: District-level averages in 2004/05 for public employment, private casual employment, family employment, log private daily wage. All of these are reported for the last week. Log per-capita expenditures in the last 30 days, land in acres. Difference column tests whether differences in the averages are statistically significant.

Table B.6: NREGS Impact: Employment (Men)

Specification	Panel A: Local randomization			
	public	total	private	family
NREGS	0.0023*** (0.0003)	0.0086*** (0.0002)	-0.0255*** (0.0034)	0.0340*** (0.0024)
N	380	380	380	380
outcome mean	0.0055	0.8126	0.3300	0.4827

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ An observation is a district in a given season. An employment outcome is the proportion of working-age adults in rural areas working in a given type of employment in the last 7 days. The window used for the local randomization approach includes observations with re-centered ranks of -2 to 4, and bootstrapped standard errors (1000 repetitions) are reported in parentheses.

Figure B.1: General Distribution of Index over Ranks

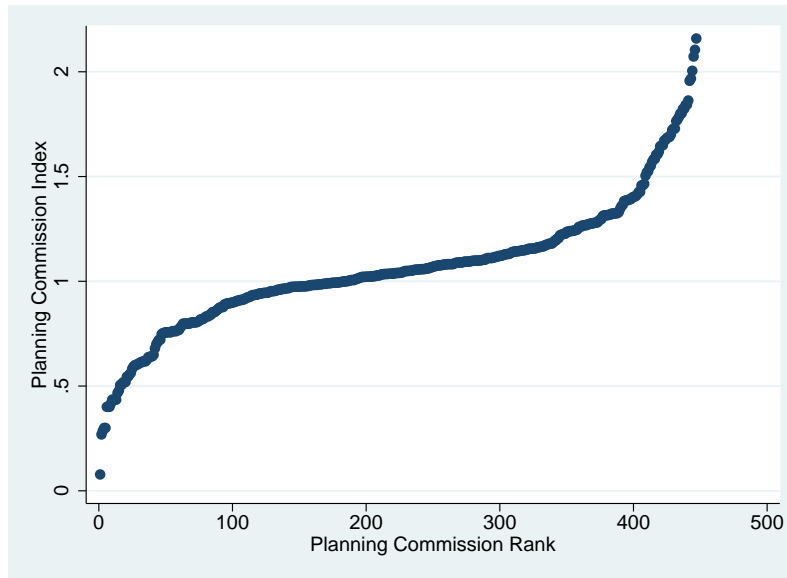
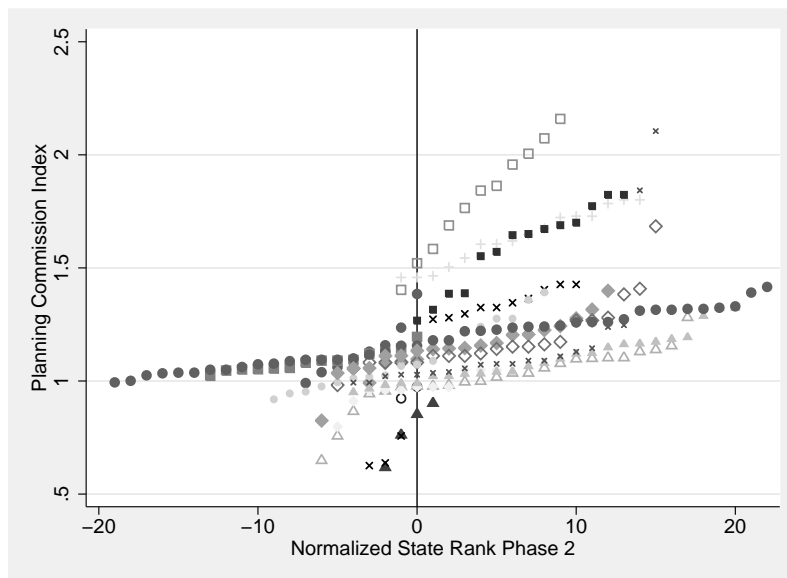
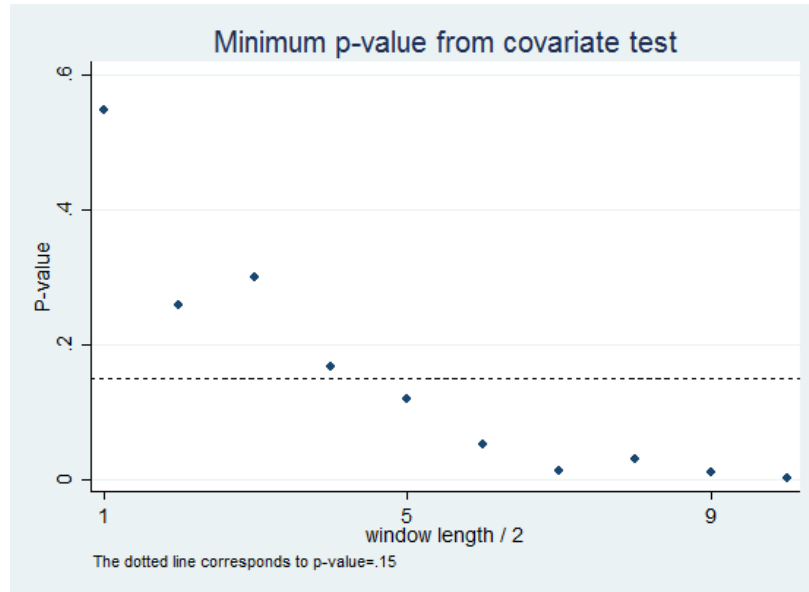


Figure B.2: Distribution of Index over State-Specific Ranks (Phase 2 vs Phase 3)





(a) Phase 2 Minimum P-Value Window Selection

Note: Figure reports the minimum p-value from a balance test of of baseline variables within different windows around the cutoff as well as a horizontal dotted line at 0.15 as a conservative cutoff. Window length 3 corresponds to the window -2 to 4 that is used in the local randomization analysis.