Oxford Poverty & Human Development Initiative (OPHI)
Oxford Department of International Development
Queen Elizabeth House (QEH), University of Oxford

OPHI *WORKING PAPER NO. 129*

The Welfare Effects of India's Rural Employment Guarantee

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October 2019

Abstract
We examine the welfare effects of India's workfare program NREGA using a novel, almost sharp regression discontinuity design. We find large seasonal consumption increases in states implementing the program intensely, which are a multiple of the direct income gains. We also find increases in adolescents' school attendance. Our results imply substantial general equilibrium effects. We conclude that, when properly implemented, the public employment program holds significant potential for reducing poverty and insuring households against various adverse implications of seasonal income shortfalls.

Keywords: public works, employment program, social welfare programs, poverty alleviation, safety net, labor markets, poverty, schooling, child labor, India

JEL Classification: J68, I38, O15

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This study has been prepared within the OPHI theme on multidimensional measurement.

Acknowledgments
We thank Jean Drèze, Abhiroop Mukhopadhyay and Laura Zimmermann for helpful comments at an early stage of this research, and Rinku Murgai for sharing information about the implementation of the National Rural Employment Guarantee Act. Manish Chauhan, Indian Institute of Technology, Kanpur, provided invaluable research assistance.

Citation: Klonner, S. and Oldiges, C. (2019): 'The Welfare Effects of India's Rural Employment Guarantee' *OPHI Working Paper* 129, University of Oxford.

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ISSN 2040-8188 ISBN 978-19-1229-120-5

1 Introduction

Poverty around the globe is concentrated in rural areas. According to Chen and Ravallion (2007), three quarters of those living on less than a dollar per day reside in rural areas while the rural population share is less than 60 percent. Well-known poverty alleviation programs have involved cash transfers, pensions, free or subsidized food provision – including school meals, subsidized credit and directed lending, asset creation, and various kinds of agricultural subsidies and extension work. A fundamental problem of all these initiatives is targeting – reaching out to the most needy. When benefits come at no cost for the recipients and administrative capacities for ensuring proper targeting are limited, the benefits from welfare programs have often been found to be captured by wealthy and politically well-connected households (Basu, 1991). An additional key challenge of programs that aim at the mitigation of risks faced by poor households is that they have to be flexible and able to deliver immediate benefits when a household experiences an income shock (World Bank, 2013).

It is primarily on these grounds that public works programs have been popular with governments around the globe. According to the *World Development Report 2014* (World Bank, 2013), in sub-Saharan Africa alone, around 150 public works programs were active around 2010, and Subbarao (2003) enumerates several large-scale public works programs in Asia and Latin America from the 1980s and 1990s. While the mandatory labor effort may reduce the net benefits accruing to program participants (Murgai et al., 2015), workfare has the potential to ensure proper targeting (Besley and Coate, 1992), and households have the flexibility to decide whether to supply their labor and receive benefits. In addition, public works programs have the potential to build growth-enhancing local public goods (Gehrke and Hartwig, 2018).

Ethiopia's Productive Safety Net Program (PSNP) appears to have been the most costly (relative to GDP) recent public employment program in low- and middle-income countries, consuming 2 percent of the country's GDP in 2007 (Lal et al., 2010). India's Mahatma Gandhi National Rural Employment Guarantee Act (NREGA) has been the largest public works program ever in terms of absolute outreach and cost, accruing 2.2 billion workdays and providing employment to nearly one quarter of rural households during the fiscal year 2013–14 (Desai et al., 2015). In the financial year 2012–13, it accrued a cost of 397 billion Indian Rupees (about $7.5 billion), close to 0.5 percent of India's GDP in that year.[1] Introduced

[1] In comparison, China's largest integrated rural development program, the 8-7 Plan, accrued expenditures of close to $4 billion in 2004, of which 18.3 percent went into a food-for-work component (Park et al., 2002).

in 2006, the NREGA guarantees 100 person days of employment to every rural household whose adult members are willing to perform unskilled manual labor at a statutory minimum wage. As with most other recent public employment programs, the main purposes stated by the creators of the NREGA are to alleviate poverty and to protect vulnerable households from economic shocks (Subbarao, 2013).

In contrast to other public employment programs in the global South, the NREGA has attracted a great deal of attention from academic economists. To name just a few studies of the NREGA's early effects, outcomes that have been studied are wages (Berg et al., 2018; Imbert and Papp, 2015; Merfeld, 2019; Zimmermann, 2018), migration (Imbert and Papp, 2019), consumption (Bose, 2017; Deininger and Liu, 2019; Ravi and Engler, 2015), agricultural decisions (Gehrke and Hartwig, 2018), and violent conflict (Fetzer, 2014; Khanna and Zimmermann, 2017).[2]

Our contribution to this literature is empirical: while two recent evaluations of technological e-governance additions to the NREGA, each implemented in a single state, are based on randomized controlled trials (Banerjee et al., 2019; Muralidharan et al., 2016) nearly all of the just-cited papers use district pseudo panels and perform simple difference-in-differences (DID) estimations for identifying the program's intent-to-treat effects, where variation in program status comes from the staggered district-wise rollout of the NREGA in three phases between 2006 and 2008.[3] Two exceptions are Khanna and Zimmermann (2017) and Zimmermann (2018), who develop a fuzzy regression discontinuity design.

In this paper we revisit the rollout of the NREGA and develop a novel empirical approach for estimating its effects. We build on the rules by which different sets of districts were allocated to the different rollout phases. We focus on districts not specifically prioritized for development programs by India's central government for reasons of Maoist conflict, low human development, or agrarian distress and demonstrate that an almost sharp state-wise regression discontinuity design (RDD) obtains for the remaining half of 'non-priority' districts in 14 of India's most populous states for the fiscal year 2007–08, the second phase of the program's rollout. A second innovation is that we combine administrative program expenditure data with three National Sample Surveys conducted in that year that contain consumption data and basic information on occupational activities. We also analyze in detail data on workfare employment and agricultural wages from one of these surveys. Guided by the finding

[2] Another strand of papers deals with corruption (e.g. Niehaus and Sukhtankar, 2013a,b), and political incentives (e.g. Gulzar and Pasquale, 2017; Gupta and Mukhopadhyay, 2016).

[3] Deininger and Liu (2019) as well as Ravi and Engler (2015) study 'treatment effects on the treated' by comparing households that participate in the program actively with households that don't.

that a reasonable amount of workfare employment was generated in only six of these states ('star states') and that this employment was almost exclusively concentrated in the agricultural lean season in spring, we study program effects separately by agricultural season and implementation intensity.

We find increases in per capita income from workfare employment equal to about 7 percent of the national poverty line and no leakage of NREGA wage funds in the star states during the agricultural lean season in spring, while there is substantial leakage and no (not even small) effects on workfare income during the fall season or in other ('non-star') states in any season. Mirroring the income effects, we find large gains in consumption in the star states during the spring season. They equal about three times the income gains and are accompanied by similarly large decreases in poverty. Moreover, households' self-reported principal occupation is shifted by the NREGA with the share of the modal occupation, agricultural labor, decreasing by almost one third. We also find seasonal increases in school attendance and decreases in adolescent labor, implying that the positive welfare effects of the program also include gains in schooling.

Our results illustrate that workfare programs in developing countries can successfully reduce poverty and insure households against seasonal drops in employment and consumption. Through this insurance function, public employment also appears to mitigate failures in the credit market regarding households' ability to smooth income fluctuations, which can generate positive spillovers on adolescents' school attendance. The heterogeneous effects for both leakage and welfare by implementation intensity across states and season demonstrate, however, that an effective and sufficiently intense implementation is crucial. The pattern of our results implies substantial non-linearities in both the implementation effectiveness of and the social returns to this large welfare program: when poorly implemented, leakage is excessive and welfare effects are smaller than program outlays, whereas leakage is small and the welfare effects close to the outlays when the implementation intensity is high. Our findings also suggest sizable general equilibrium effects of the employment program, even in the relatively short run. Due to data limitations, however, we cannot fully make precise the channels through which these effects run. For example, we find only limited evidence for short-term agricultural wage increases or decreases in seasonal migration.

The pattern of our results is similar to Muralidharan et al. (2016), who study the income and wage effects of the addition of biometric smartcards to the NREGA in one Indian state in 2011. They find substantial decreases in leakage and increases in household incomes that outmatch significantly, by a factor of 10, the direct income gains from NREGA earnings, suggesting "a complex set of feedback loops, multipliers, and interactions between several channels operating in general equilibrium."

Our estimation procedure for the employment program's effects improves on previous NREGA impact studies in several dimensions. First, our identification approach is cross-sectional, which does not require the parallel trend assumptions made in the previously cited DID analyses. While all DID studies of the NREGA acknowledge better outcomes for their control districts before the NREGA was rolled out, all of them claim parallel trends before the program. In contrast, we show that there are large and significant departures from parallel trends for consumption and poverty pre-NREGA when all Indian districts are pooled. More generally, given that early coverage by the NREGA was targeted at less developed districts, any DID strategy cannot convincingly disentangle program effects from an accelerated secular convergence trend, which we think is difficult to rule out given India's aggregate growth rate of 8 percent between 2005 and 2007. Second, we take seriously the confounding of NREGA's effects with two other similarly budgeted rural development programs rolled out in parallel. While about 80 percent of NREGA districts – but none of the control districts – in the DID and RDD studies cited above also have at least one of these programs, this share is just 30 percent in our sample, and we show that our estimates are robust to the exclusion of such districts. Third, DID estimates suffer from neglecting at least two earlier programs that were active in none of the control districts but were active in 60 percent of the other authors' treatment districts in 2005 (when pre-NREGA data was typically collected) but which were phased out before 2007, the year of the DID studies' midline or endline data. Our cross-sectional approach, in contrast, is immune to this concern. Finally, we think there are good reasons to believe that there are heterogeneous program effects depending on a district's initial characteristics relating to violent political conflict, agrarian distress, or dismal human development indicators. Our approach makes explicit for which district characteristics the estimated treatment effects are externally valid. The extant DID approaches, in contrast, compare a set of early NREGA districts, most of which are faced with at least one such challenge, to a set of control districts not confronted with these challenges to a comparable extent.

The remainder of this paper is structured as follows. In Section 2, we introduce the NREGA and discuss in detail its rollout between 2006 and 2008, which sets the stage for our identification strategy. Section 3 describes the various data sources that we use. Section 4 contains the results, several robustness checks and extensions. Section 5 concludes.

2 Background and Research Design

2.1 The National Rural Employment Guarantee Act

Under the NREGA, enacted in 2005, every rural household is entitled to 100 days of work at the statutory minimum wage, which is set by the respective state government. The NREGA as a policy instrument is remarkable in two ways: its rights-based approach and its provisions for transparency and accountability (Khera, 2011). It also draws strongly on the spirit of the Right to Information Act, enacted in 2006, by defining provisions for enabling transparent and easily accessible administrative records, as well as processes for public scrutiny and accountability of officials toward beneficiaries. As a result, since its implementation in 2006, it has been closely monitored by both researchers and civil society, which has helped to expose several instances of leakage and corruption (Niehaus and Sukhtankar, 2013a).

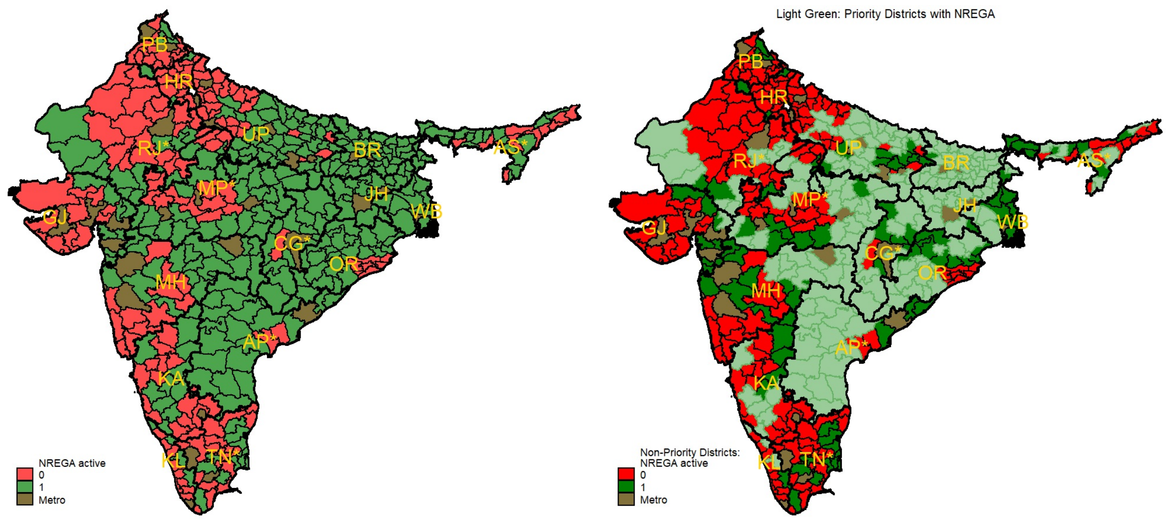
The NREGA is not the first public works program in post-independence India. The National Food for Work Programme (NFFWP), implemented between 2004 and 2006, is viewed as the predecessor of the NREGA. Of the several earlier state-level programs, the Maharashtra Employment Guarantee Scheme, enacted in 1977 and active until the inception of the NREGA, has received some interest by researchers in the past (Basu, 1981; Drèze, 1990; Ravallion et al., 1993). The nature of assets created is varied and comprises roads, bridges, public and private irrigation facilities, and improvement of marginal farm land, as well as construction of schools and health centers.

At 0.6 percent, the central government's expenditures on the NREGA as a share of the country's GDP reached a peak in the fiscal year 2009–10 (Drèze and Khera, 2017). It is India's second largest welfare program, only outmatched by the country's public food distribution system (PDS), on which the central government spent about 1 percent of GDP around the same time (World Bank, 2011). More details on the particulars of this program can be found in the excellent literature summary by Sukhtankar (2016).

2.2 NREGA Rollout and Research Design

The NREGA started in 200 districts, which we will refer to as phase I districts, in the fiscal year April 2006 to March 2007. In April 2007, another 130 districts were added (phase II), and in April 2008 the remaining 295 districts were covered under phase III. Only a handful of metropolitan districts were not implementing the program by 2009. We identify phase I, phase II, and phase III districts as published on the official website of the Ministry of Rural Development in a document dated December 2010. In our econometric analysis, where we approach the NREGA rollout as a natural experiment, we focus on the fiscal year 2007–08. The left panel of Figure 1 maps districts' program status in India's 17 major states, which are home to about 92 percent of the country's population, for that year. It also flags 35 districts with a major city, which we exclude from all our analyses.[4] The relative frequency of program districts varies considerably across states. Notably the NREGA was active, at least in principle, in all non-metropolitan districts of the relatively poor northeastern states Bihar and Jharkhand, as well as in West Bengal.

Figure 1: Districts' NREGA and Priority Status in 2007-08, Major States



In this section we only sketch our research design, aided by maps and diagrams. The details, which are based on an extensive analysis of government documents, conversations with experts, and a detailed replication exercise of how official rules governing the assignment of the 479 districts in India's 17 major states to the three NREGA phases were implemented, are relegated to an appendix. Our key insight is that the allocation of districts to phases I and II was largely driven by two targeting rules, one strict and one soft. The strict rule is based on three priority lists of districts plagued by Maoist insurgency,[5] agrarian distress, and low human development, all of which had been compiled earlier, during the first half of the

[4]We have adopted this list from the Planning Commission report *Identification of Districts for Wage and Self Employment Programmes* (Government of India, 2003), which we will discuss in more detail shortly.

[5]The effect of the NREGA on Maoist conflict is the subject of Dasgupta et al. (2017), who find large pacifying effects concentrated in "red belt" states with effective program implementation. They use a difference-in-differences estimation strategy.

21st century's first decade, by India's Planning Commission. In accordance with the Government of India (2007a), all 182 districts on these three lists in India's 17 major states had the NREGA by 2007–08.

The choice of most of the remaining major states' 93 NREGA districts in 2007-08 is linked to a backwardness ranking published in the 2003 Planning Commission report *Identification of Districts for Wage and Self Employment Programmes* (Government of India, 2003). Districts that are relatively backward according to this ranking, which is based on agricultural wages and productivity as well as the population share of disadvantaged social groups around the turn of the millennium, were also targeted by the first two phases of the NREGA (Government of India, 2007a), albeit not in a manner as stringent as the districts appearing on the three just-mentioned priority lists.

Figure 2: District Backwardness Ranks and NREGA Status in 07/08, by State

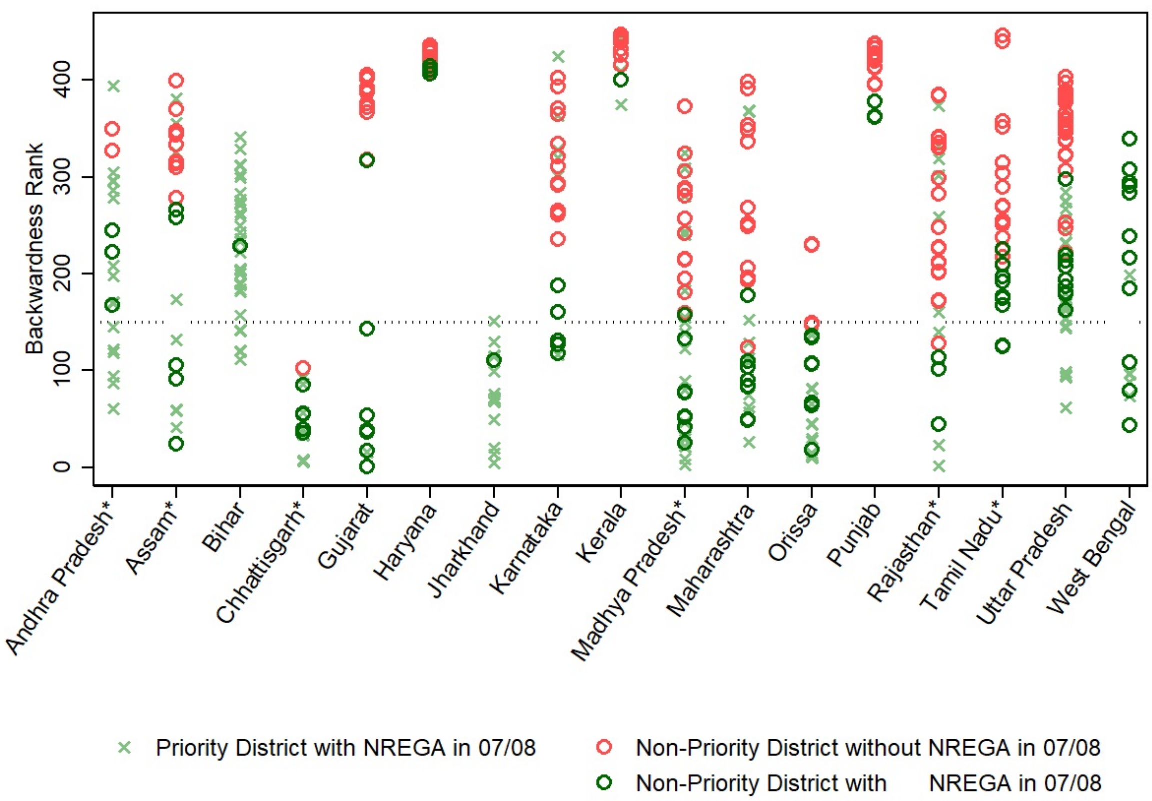


Figure 2 plots the backwardness ranks of all districts by state, separately by priority (light green x's) and other ('non-priority') districts (red and dark green circles). It is evident that, according to these backwardness ranks, priority districts largely overlap with phase III districts (labeled 'non-priority district without NREGA in 07/08' in Figure 2), even within states. To assess impacts of the NREGA in 2007–08, it is not obvious how to identify a valid control group for the priority districts if selection into the priority lists correlates with development outcomes and trends across districts absent the program.[6] On the other hand, for the majority of major states, non-priority 2007–08 program districts (green circles) are sharply separated from phase III districts (red circles) on this backwardness scale, at least within states. This is consistent with a press release of the Government of India (Government of India, 2007a), according to which backward districts from the Government of India (2003) were added to phase II after including priority districts (see Figure A.2 in the Appendix).

As Figure 2 demonstrates, the backwardness ranking was not processed from bottom to top during phases I and II. Instead, consistent with the objective of an "equitable distribution" of program districts across states (Government of India, 2007a), all major states received some program districts in addition to the priority districts, including the relatively well-to-do states Haryana, Kerala, and Punjab. Within each state, however, the most backward districts according to the Government of India (2003) were selected, at least in most cases.[7] This is the point of departure of our research design, which is following an RDD in spirit. We exclude priority districts, take a district's within-state backwardness rank and center it by subtracting the respective state's number of non-priority phase I and II districts less one half. The resulting running variable is a measure of district backwardness within state, where a higher value means less backward.

[6]The DID identification approaches, such as Imbert and Papp (2015), as well as the fuzzy RD approach of Khanna and Zimmermann (2017) assume, at least implicitly, no such selection effects.

[7]The set of 150 districts explicitly recommended for wage employment programs by the authors of Government of India (2003) was not comprehensively included into the first two phases of the NREGA. Those districts are all located on or below the dotted horizontal line in Figure 2 and comprise four phase III districts, in the states Chhattisgarh, Maharashtra, Orissa, and Rajasthan.

Figure 3: RDD Running Variable and NREGA Status in 2007-08, non-Priority Districts

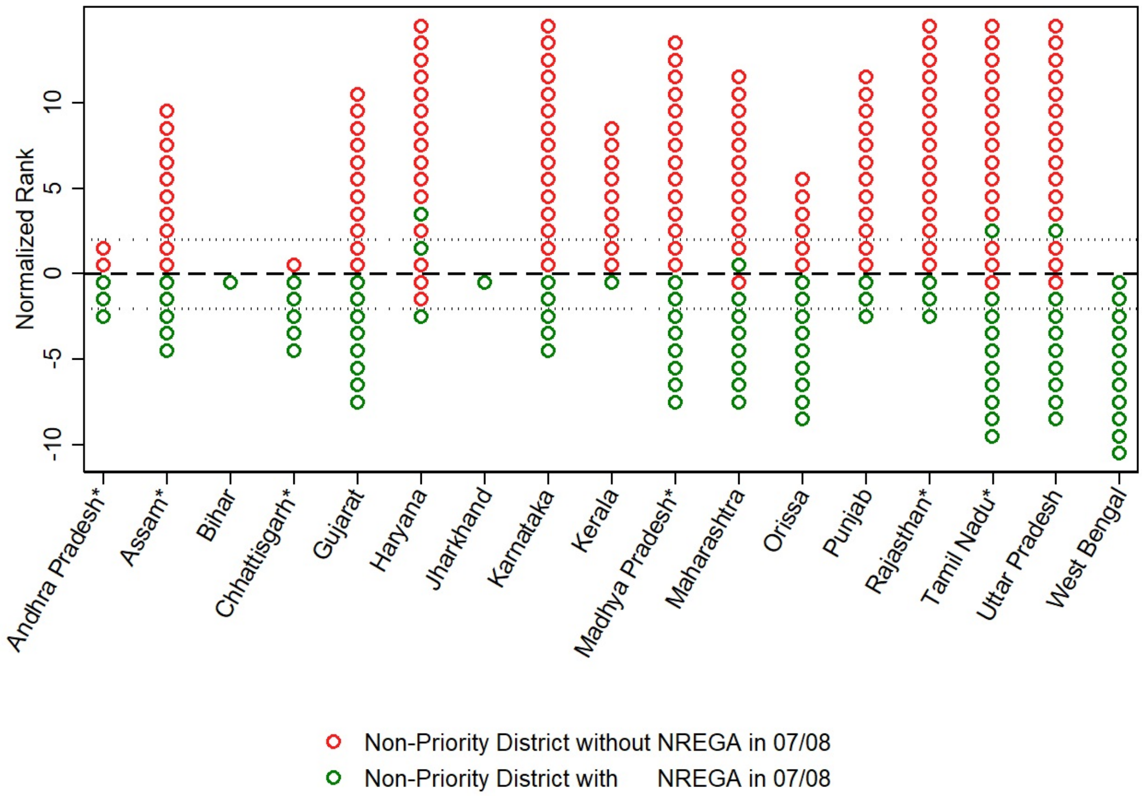


Figure 3 plots the resulting *normalized rank* for non-priority districts, state by state. Evidently, 10 of the 14 resulting state-wise RDDs are sharp. We drop the states Bihar, Jharkhand, and West Bengal, which do not have a single non-metropolitan district without the NREGA in 2007–08. For reasons that will become clear shortly, our focus will be on the six states flagged with a star in Figures 1 to 4.

Our empirical design faces the challenge that there are only three star states with more than three districts below and no more than four star states with more than two districts above the threshold. This lack of density in the running variable around the threshold as well as the fact that it only takes integer values distinguishes our scenario from a standard RDD, where continuity in the running variable and a sufficient density around the threshold are basic requirements, at least for nonparametric identification (Lee and Lemieux, 2010). While we are aware of these differences, we will continue to refer to our empirical design as RD for terminological simplicity.

Regarding the choice of bandwidth in the estimations, we desire as much similarity as possible regarding the backwardness ranks of districts within each state while achieving a sample size, which is the number of districts, that yields reliable statistical inference. The latter concern requires a minimum of two for the (one-sided) bandwidth, which leaves us with 23 districts in the star states, five states with four and one, Chhattisgarh, with three districts. Regarding the former objective, Figure 2 shows that increasing the bandwidth beyond two more than doubles the sample range of the backwardness ranks for three of the four star states with more than two non-priority districts on either side of the RD threshold. For our main analyses, we therefore choose a bandwidth of two and a piecewise constant regression function since even a first-order polynomial, or local linear regression, would be no more than just identified while cutting into the degrees of freedom considerably.[8]

Figure 4: RDD Sample Districts

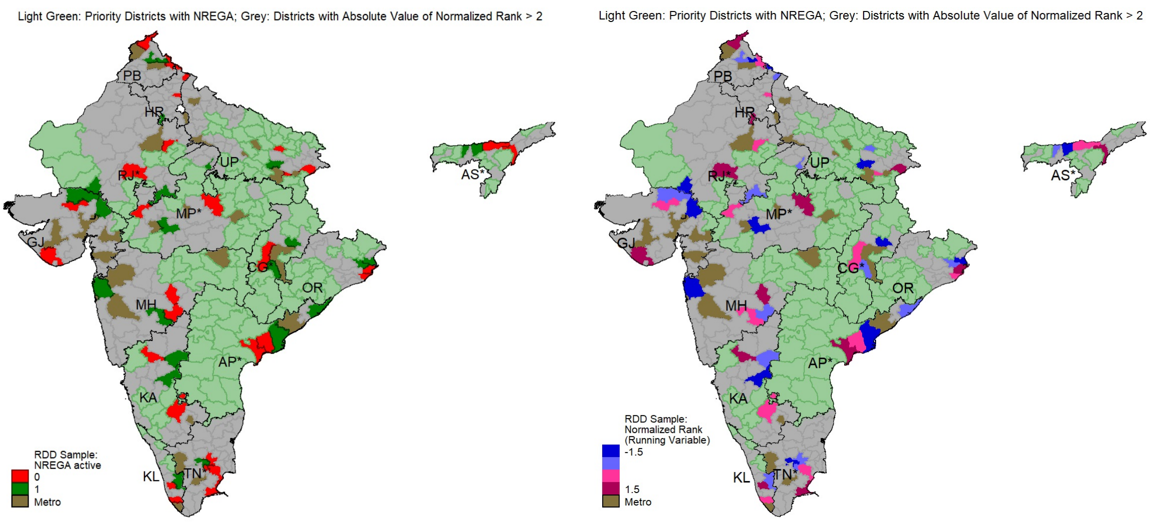


Figure 4 maps all districts from Figure 3 whose normalized rank does not exceed two in absolute value. The right panel is a heat map depicting the normalized rank of threshold districts in different shades of pink and blue. The left map illustrates that, in each of the 10 states where our RDD is sharp, two of these four districts are covered by the NREGA in 2007–08 and two are not.[9]

We can make precise for which subset of districts the treatment effects estimated with this design are externally valid: our RD estimates refer to districts that are not plagued by Maoist

[8]We explore local linear regression as a robustness check.
[9]A slight exception is Chhattisgarh (CG), where our RDD is sharp but there is only one non-metropolitan district not covered by the NREGA in 2007–08.

insurgence, excessive agrarian distress, or especially low human development. Moreover, at least for the six states flagged with a star in Figures 1 to 4, they refer to districts that are just slightly more backward by the Planning Commission's 2003 backwardness ranking than the average across the Indian union. While this could be seen as a limitation of our analysis, we view it as a strength: our RD estimates are valid, roughly, for an average usual district in India's major states, where 'average' refers to agricultural and social group characteristics and 'usual' means not challenged by any of the complications captured by the three priority lists.

Our research design contrasts the RDD approach by Khanna and Zimmermann (2017), which builds on the same district backwardness ranking. The main difference to our approach is that it does not exclude priority districts. This results, first, in much greater fuzziness (this fact is immediately evident from Figure 2, where it is unclear how to determine thresholds between green and red markers on the backwardness scale within each state). Second, for identification of causal treatment effects, it requires that, conditional on backwardness ranks, levels of outcomes are the same in priority and non-priority districts absent the NREGA – which does not hold in our data from before the NREGA. Third, it confounds NREGA program effects with those of other geographically targeted major welfare programs, most notably the Backward Regions Grand Fund (BRGF), a district development program, and the Prime Minister's Rehabilitation Package for Farmers in Suicide Prone Districts, at least one of which had been rolled out concurrent to the NREGA in each priority district – about 80 percent of all NREGA 2007–08 program districts. Finally, it is difficult to make precise for what type of districts the estimated effects are externally valid. Our state-wise RDDs, instead, are mostly sharp and immune to assumptions regarding different outcome levels or trends in priority versus non-priority districts. Moreover, no more than one third of NREGA 2007–08 program districts in our RDD sample are covered by the BRGF and, at 27 percent, this figure is even smaller for the six star states.

Our research design has similar advantages over previous difference-in-differences (DID) approaches exploiting the NREGA's rollout (e.g. Imbert and Papp, 2015; Berg et al., 2018). While these authors typically acknowledge different levels of program and control districts pre-NREGA, they all rely on parallel trend assumptions for priority versus non-priority districts. We show below that this assumption is grossly violated in consumption data from before the NREGA. More generally, given that the NREGA's early phases were targeted at less developed districts, any DID strategy cannot convincingly disentangle program effects from an accelerated convergence trend, which is not unlikely given India's rapid aggregate growth between 2005 and 2007. Moreover, confounding NREGA's effects with the effects resulting from the other two programs rolled out concurrently is as big a threat as discussed in the previous paragraph.

The main challenge facing our approach is the exogeneity of the state-wise RD thresholds. While obvious from Figure 2, at least for most states, identification of causal effects of the NREGA requires that they involve no sorting of districts around the threshold based on outcomes of interest absent the program. We will discuss this issue in detail in Section 4.2.

3 Data

3.1 Administrative Data

We have collected district- and month-wise program expenditures from the NREGA website hosted by the Ministry of Rural Development. For the major states with at least one phase III district, Figure 5 depicts district means of NREGA wage expenditures per rural inhabitant during the agricultural year 2007–08.

Two facts stand out. First, in accordance with Drèze and Oldiges (2009), we find ample variation across states. Expenditures are much higher in six states identified as NREGA high performers by these authors (with the exception of Karnataka). Borrowing Imbert and Papp's (2015) term for NREGA high performers, we will refer to them as 'star states'. According to the sample means set out in panel C of Table 1, NREGA wage expenditures average Rs. 21 per rural inhabitant and month in these six star states during 2007–08, which compares to less than Rs. 6 in the other eight states in our RDD sample.

Second, wage expenditures follow a marked seasonal pattern. In all star states, expenditures are concentrated in the *rabi* season of 2008, the months January to June, when labor demand in agriculture is at a low. In the second quarter of 2008, the records show monthly expenditures of close to Rs. 120 per rural inhabitant in phase I and II districts of Andhra Pradesh, Chhattisgarh and, Rajasthan, which is more than a quarter of India's rural consumption poverty line of Rs. 440 per person and month in that year (see Table 1, panel D).

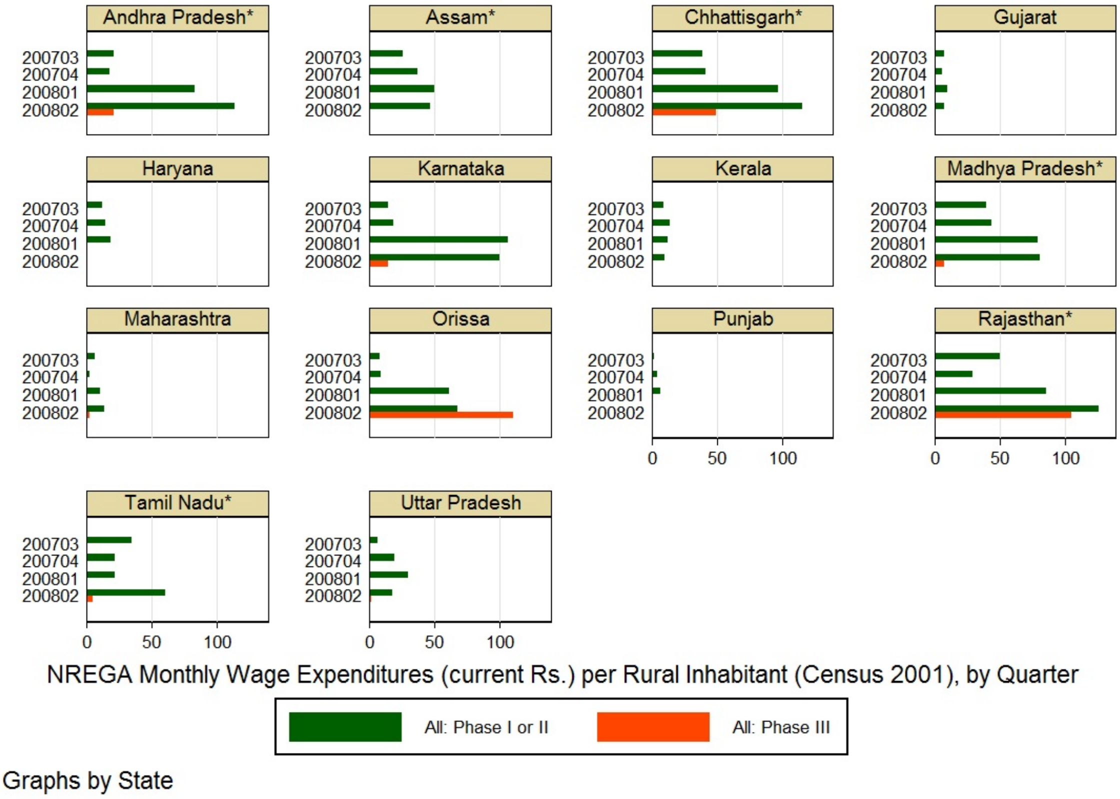
With the onset of the financial year 2008–09 in the second quarter of 2008, all star states except Assam started implementing the NREGA and paying wages in phase III districts according to these data. This has implications for our RDD, where the control group becomes 'contaminated' in the second quarter of 2008 in some of the states. We will revisit this issue

Table 1: Descriptive Statistics

	(1) All Major States	(2) RDD Sample Star States	(3) RDD Sample Other States
A. NSS Data, Household Level			
MPCE (Current Rs.)	665.87	715.95	739.40
	(461.43)	(398.44)	(585.81)
Poverty Headcount Ratio	0.46	0.33	0.44
	(0.50)	(0.47)	(0.50)
Principal Occupation: Agricultural Laborer	0.27	0.31	0.25
	(0.44)	(0.46)	(0.43)
Household Size	5.81	5.02	5.89
	(2.66)	(2.27)	(2.65)
Scheduled Caste or Tribe	0.32	0.33	0.31
	(0.47)	(0.47)	(0.46)
Observations	142303	6595	8549
B. NSS Data, Individual Level			
Agricultural Wage Rate (Current Rs./day)	59.21	60.96	62.65
	(27.77)	(28.73)	(32.11)
Observations	31663	1552	1637
Agricultural Wage Rate (Males)	65.62	71.55	70.03
	(29.45)	(31.22)	(35.42)
Observations	20359	855	980
Agricultural Wage Rate (Females)	47.93	48.62	51.31
	(20.05)	(19.22)	(21.85)
Observations	11304	697	657
Wage Income from Public Works	8.06	11.98	1.76
(Current Rs. per Month, per rural Inhabitant)	(130.68)	(166.57)	(67.02)
Observations	292416	12512	18249
C. Government of India, Administrative Records			
NREGA Wage Expenditures	19.66	21.11	5.60
(Current Rs. per Month, per rural Inhabitant)	(51.87)	(41.87)	(32.85)
Observations	5304	276	336
D. Other Sources			
Poverty Line (State-wise, current 2007-08 Rs.)	449.13	439.92	473.30
	(36.43)	(27.50)	(48.64)
Observations	17	6	8
Rainfall in 2007, relative to district long-term avg.	1.11	0.99	1.14
	(0.25)	(0.13)	(0.17)
Observations	419	23	30

Notes: Means, standard deviations in parentheses. Data sources: NSS 64th round schedules 1, 10 and 25 (panel A), NSS 64th round, schedule 10 (panel B), NREGA website maintained by the Ministry of Rural Development (panel C), Government of India (2009), Government of India (2013), India Water Portal (online source, various years) (panel D).

Figure 5: NREGA Wage Expenditures in 2007–08



Data Source: Ministry of Rural Development, Administrative Records in the next subsection when we turn to NREGA wage incomes.

3.2 Survey Data

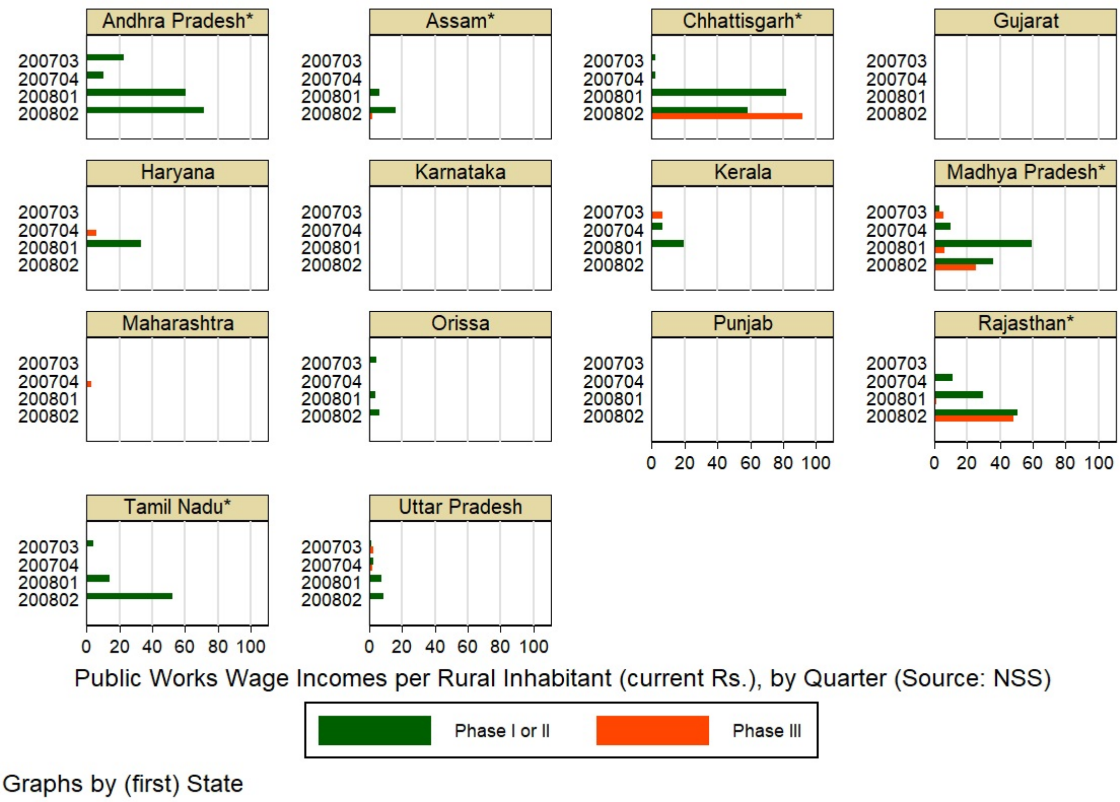
In our empirical analysis, we use primarily the 61st and 64th round of India's National Sample Survey (NSS) covering the agricultural years July 2004 to June 2005 and 2007–08, respectively. For placebo experiments and in an extension we use, in addition, data from the 55th and 66th round canvassed in 1999–00 and 2009–10, respectively. For calculating representative district averages, we use the sampling weights provided with the NSS data.[10] In all our regression analyses, district sample means are the unit of observation.

Our focus is on household welfare as captured by consumer expenditures in 2007–08. While there is a consumption module with 368 items as part of the 64th NSS round, called schedule 1, we choose to also involve consumption data from the same round's employment survey, schedule 10, as well as an education expenditure survey, schedule 25. Schedules 10 and 25 contain short consumption questionnaires with 19 and five expenditure categories, respectively. We include these latter two data sources for the following reasons. First, the 64th round's employment and education expenditure surveys contain large numbers of observation – 125,578 and 100,581 households, respectively – for India as a whole. Large numbers of unit-level observations are essential for our analysis of the NREGA in the six star states, which relies on district means for only the rural sector in just two dozen districts. Moreover, India's National Sample Survey Organization (NSSO) points out that district-level survey means of the "thin" consumption survey administered in 2007–08 are not representative due to the total sample size of 50,297 households, which is small by NSS standards (Chaudhuri and Gupta, 2009). Second, the sampling methodology in the employment and education expenditure modules of the 64th round is identical to the one used in schedules 1 and 10 of the "thick" 61st round, which covers the agricultural year 2004–05 and will deliver lagged dependent variables as well as placebo estimates in our econometric analyses. On the other hand, the sampling methodology is markedly different in the 64th round's consumption survey from both the employment module of the same round as well as the 2004–05 consumption survey, which makes researchers generally reluctant to trust "thin" NSS survey rounds (Deaton and Kozel, 2005). In data from the 61st round, where both the employment and the consumption survey are "thick", rural mean (median) per capita consumption expenditures in the employment survey falls short of the average in the consumption survey by merely

[10] The data is provided with household-level inverse probability sampling weights, which we multiply by the household size to make all figures representative for the population of individual rural inhabitants.

4.2 (2.5) percent. When applying the updated national (or Tendulkar) poverty line used by India's Planning Commission for the NSSO consumption survey to the consumption data in the employment survey, the poverty headcount ratio is overestimated by a moderate 2.3 percentage points or 6.3 percent.

Figure 6: Monthly NREGA Wage Incomes in 2007–08



Data Source: NSS 64th Round, Schedule 10

For the major states with at least one phase III district, Figure 6 depicts district means of public works wage incomes per rural inhabitant by quarter during the agricultural year 2007–08 reported by respondents to the employment survey. Sample means are set out in panel B of Table 1. As in the administrative data, we find ample variation across states. Consistent with the figures reported in Imbert and Papp (2011), these incomes are close to zero in all non-star states. Remarkably, of Karnataka's 8,650 rural respondents to this survey aged 18 and older (metropolitan districts excluded), only a single one reported some public works activity during the week preceding the interview, while Karnataka's administrative wage expenditures are on par with those of the star states (see Figure 5). Figure 6 confirms our classification of star and non-star states, but, even in the former, reported wage incomes fall short of the levels documented in the administrative data by more than one third on average. It is beyond the scope of this paper to assess the sources of this difference, but leakage on the supply side as well as underreporting of wages on the demand side likely contribute to this gap (Niehaus and Sukhtankar, 2013a).

Since, according to the survey data, there was virtually no NREGA activity in the non-star states, we conduct all regression analyses separately for the six star and the other eight states. We view the former as examples of a scenario where the employment program actually reached out to rural populations, while we view the latter as similar to a control group where the program was barely accessible. While Kerala shows a similar level of public works wage incomes as Assam in the survey data, we choose to not include it in the group of star states. First, in Drèze and Oldiges (2009), it is ranked only 11th among the 14 states that we consider, while Assam is ranked fourth. Second, Kerala is an outlier with respect to our RDD's marginal districts, which are both in the most affluent decile of the backwardness ranking plotted in Figure 2. Since our main interest is in the NREGA's effect on poverty reduction, however, including Kerala's districts is not meaningful.[11]

Consistent with the administrative data, households in phase III districts report substantive NREGA wage incomes in Madhya Pradesh, Chhattisgarh, and Rajasthan during the second quarter of 2008. We deal with this issue by excluding from our RD analyses unit-level observations from these three states where the survey interview took part during the second quarter of 2008.[12]

Sample means of our outcomes of interest are set out in panels A and B of Table 1. Monthly per capita consumption expenditures (MPCE) are 7 to 8 percent higher in our RDD samples than in the major states as a whole, a consequence of the fact that districts prioritized for development programs by the central government are excluded. A corresponding pattern is obtained for the poverty rate, which stands at 46 percent in the major states, and 33 and 44 percent in the star and non-star state districts of our RDD samples.[13] In contrast, the shares of agricultural laborers and disadvantaged social groups (scheduled castes and tribes) as well

[11] With the exception of Assam, our classification of star states is congruent with Imbert and Papp (2015), who use 1 percent of adult work days in public works as a cutoff. An important reason why we choose to include Assam among the set of star states is sample size: with the inclusion of Assam, there are six star states in our RDD and eight other states. Reducing the number of star states would bring down the RDD sample size for star states to less than 20 districts; see the next section.

[12] We explore departures from this strategy as a robustness check.

[13] As pointed out previously, these poverty rates are somewhat overstated relative to the official figures because we use a poverty line corresponding to a comprehensive consumption questionnaire, while 80 percent of households in our consumption sample have been administered only a short questionnaire.

as agricultural wage rates in our RDD samples are fairly representative of the major states as a whole.

4 Econometric Analysis

4.1 Empirical Approach

Implementing the empirical strategy outlined above, our main estimating equation is

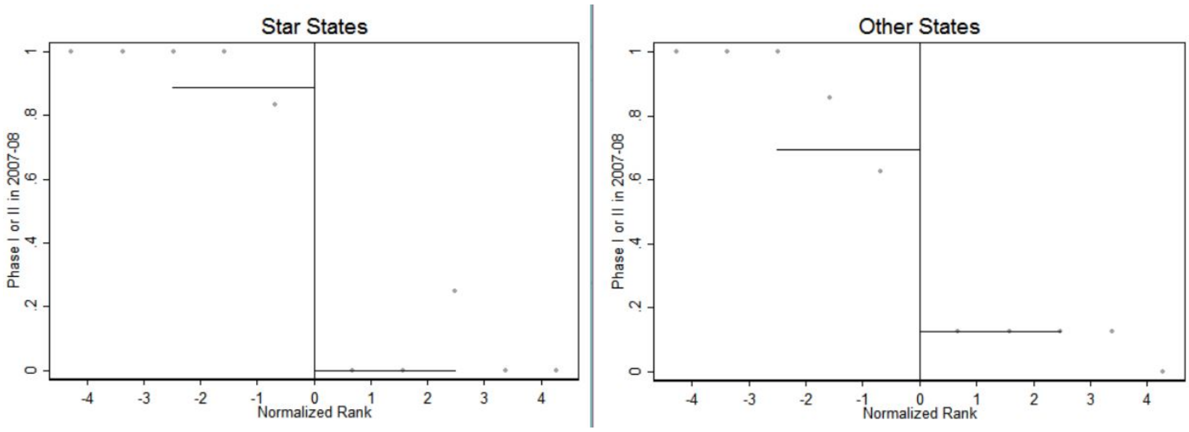
$$y_{sd} = \alpha_s + \beta\, 1\{nrank_{sd} \leq 0\} + \gamma x_{sd} + u_{sd}, \tag{1}$$

where y_{sd} is an outcome of interest in district d of state s, α_s is a state fixed effect, $nrank_{sd}$ denotes the normalized rank of district sd, x_{sd} is a vector of controls, in particular rainfall and the lagged value of the dependent variable, and u_{sd} is a stochastic error term. We include state fixed effects because our RDD is state-wise. We include the lagged dependent variable and rainfall to reduce residual variance. This is essential given the small number of districts in our research design.

The coefficient β captures the intent-to-treat (ITT) effect of the NREGA. We use a triangular kernel with a bandwidth of 2.5 (one-sided), implying that districts immediately neighboring the threshold of the running variable ($nrank = +/-0.5$) get twice the weight as districts with a normalized rank of 1.5 in absolute value. We use standard, parametric statistical inference as it generally leads to more conservative decisions in our application than the nonparametric confidence intervals of Calonico et al. (2014).

The left panel of Figure 7 plots the relative frequency of NREGA program status in 2007–08 for the six star states in our sample over the running variable $nrank$ together with a piecewise constant regression function as given by the right-hand side of Equation (1) and using the specifications laid out in the previous paragraph. According to these plots, the probability of being a program district in 2007–08 drops at the threshold by 89 percent in the star states and by 57 percent in the other seven states that we consider (see also columns 1 and 2 of Table 5). The former figure implies that our RDD is almost sharp for the star states and hence ITT effects obtained from estimating Equation (1) will be fairly tight lower bounds for average causal treatment effects of the NREGA for districts close to the threshold.

Figure 7: Probability of Being Covered by the NREGA in 2007–08, Non-Priority Districts in 14 Major States



4.2 Validation

In this section we discuss the identifying assumptions of our RDD and perform some econometric tests of these assumptions. First, there must be no manipulation of the value of the running variable in a way that leads to "precise sorting" around the eligibility threshold (Lee and Lemieux, 2010). No manipulation of the value of the running variable, which is a district's within-state backwardness rank among non-priority districts, is implied by no manipulation of the original, India-wide backwardness ranking. The said score was calculated in the early 2000s by an expert group from publicly available data and hence manipulation to favor certain districts can safely be ruled out.

Second, there must be no manipulation of the eligibility threshold, which in our application is equivalent to no manipulation of the distribution of non-priority phase I and II districts across states for the sake of including or excluding particular districts that are in the vicinity of the RD threshold. With respect to this issue, our research design faces the challenge that there is no ex-ante specified threshold for 'treatment' within each state in Figure 2. Rather, we infer the thresholds from combining three priority lists of districts with districts' observed NREGA status in 2007–08. Support for our identifying assumption comes from conversations with a former member of the Planning Commission, who related that state quotas for non-priority phase I and II districts were determined regardless of the identity of the districts that would be included or excluded as a consequence.[14] Since we cannot ultimately ascertain

[14]Zimmermann (2018) and Khanna and Zimmermann (2017) point out that the total number of phase I and II districts in each state may have been chosen by the planners according to the state-wise distribution of the

whether the requirement of no manipulation of the threshold holds, we will conduct a number of validity tests exploring observable similarity of districts just above and below our RD threshold absent the NREGA. Specifically, we use placebo estimations with lagged data and balancing tests with contemporary realizations of arguably unaffected covariates.[15]

We start out with placebo regressions employing data from schedules 1 and 10 of the 55th and 61st NSS rounds. To assess how our RDD performs relative to other authors' approaches (e.g. Imbert and Papp, 2015, 2019; Berg et al., 2018) in pre-NREGA data, we modify the regressor of interest in Equation (1) to an indicator for phase I or II status.[16] We conduct these regressions for three sets of districts: all districts identified by the NSS in the 17 major states (453 districts), all districts that are not on one of the three priority lists (261 districts; see Figure A.3 in the Appendix), as well as our narrower RD sample with two districts above and below the threshold (58 districts). The results for the dependent variables MPCE (logarithmic) and poverty using India's updated national or Tendulkar poverty line are set out in the upper two panels, columns 1 through 6, of Table 2.[17] According to column 1, there are vast differences in both MPCE and poverty in phase I and II ('early') relative to phase III ('late') districts in 1999–00, of 13 percent and 13 percentage points, respectively. According to column 4, both of these gaps have narrowed by one-third five years later. According to the figures in column 7, which contains DID estimates for these two NSS rounds, the null hypothesis of parallel trends in early and late NREGA districts is clearly rejected for both welfare measures at the 95 percent significance level.

Columns 2 and 5 show that excluding priority-list districts does not alter this pattern: there are similar gaps in consumption and poverty between early and late NREGA districts around the turn of the millennium, which shrink, however, even more, by about two-thirds, during the following five years. Column 3, in contrast, shows that early and late NREGA districts are much more similar in our RD sample. Moreover, according to column 6, these differences have completely vanished by 2004–05, three years before our main analysis. Hence the null

poor across the Indian union during the early 1990s. In contrast, our source pointed out that the distribution of non-priority districts across states was perhaps loosely guided by state poverty headcounts but eventually decided by the planners in consultation with politicians rather ad-hoc.

[15] McCrary tests for the continuity of the running variable's density at the cutoff are not a meaningful option in our setting as our running variable is a within-state rank – which is uniformly distributed around the threshold by construction.

[16] We report literal placebo estimations of Equation (1) in the robustness section below.

[17] As pointed out earlier, the poverty regression estimates are not valid for official poverty figures as poverty is slightly overestimated in the employment survey data, where the consumption questionnaire is short. Nonetheless, we view the poverty estimates as an important indication for changes in the lower part of the consumption distribution.

Table 2: Levels and Trends pre-NREGA: Consumption and Poverty

Panel A						
	(1)	(2)	(3)	(4)	(5)	(6)
Year:		1999/2000			2004/2005	
Districts:	All	N-P	RD	All	N-P	RD
Dependent Variable: MPCE (logarithmic)						
PI/II	-0.130***	-0.115***	-0.064**	-0.082***	-0.042*	0.001
	(0.016)	(0.019)	(0.030)	(0.018)	(0.023)	(0.041)
Dependent Variable: Poverty Headcount Ratio						
PI/II	0.131***	0.119***	0.055	0.073***	0.032	-0.031
	(0.017)	(0.021)	(0.036)	(0.019)	(0.024)	(0.045)
Dependent Variable: Princ. Occ. Agric. Labor						
PI/II	0.033***	0.024*	0.008	0.010	-0.002	-0.032
	(0.011)	(0.013)	(0.027)	(0.012)	(0.017)	(0.029)
Observations	448	260	58	453	261	58
R-squared	0.550	0.478	0.630	0.336	0.278	0.589
Unit-level obs.	138534	81858	19210	126986	75932	17073

Panel B			
	(7)	(8)	(9)
Year:		1999/2000 - 2004/2005	
Districts:	All	N-P	RD
Dependent Variable: MPCE (logarithmic)			
PI/II X Y04-05	0.051**	0.078***	0.065
	(0.020)	(0.024)	(0.066)
Dependent Variable: Poverty Headcount Ratio			
PI/II X Y04-05	-0.060**	-0.092***	-0.086
	(0.024)	(0.029)	(0.079)
Dependent Variable: Princ. Occ. Agric. Labor			
PI/II X Y04-05	-0.023	-0.024	-0.041
	(0.015)	(0.021)	(0.042)
Observations	922	542	116
R-squared	0.862	0.868	0.853
Unit-level obs.	265520	157790	36283

Notes: Robust standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ Standard errors clustered at the district level (cols. 7-9). An observation is a district mean in a given year. Additional control variables not reported: state fixed effects (cols. 1-6); district fixed effects, state - year 2004-05 interactions (cols. 7-9). Data source: NSS 55th round (cols. 1-3 and 7-9) and 61st round (cols. 4-9), schedules 1 and 10, rural households in 17 major states. N-P stands for Non-Priority District, RD for Regression Discontinuity Sample, PI/II is a Phase I or II District Dummy, and Y04-05 is a Year 2004-05 Dummy.

hypothesis of identical levels of consumption and poverty right before the inception of the NREGA cannot be rejected at any common test size.

Table 3: Levels and Trends pre-NREGA: Agricultural Wages

Panel A						
	(1)	(2)	(3)	(4)	(5)	(6)
Year:	1999/2000			2004/2005		
Districts:	All	N-P	RD	All	N-P	RD
PI/II	-0.190***	-0.160***	-0.020	-0.134***	-0.132***	0.007
	(0.042)	(0.054)	(0.104)	(0.026)	(0.033)	(0.052)
Observations	437	256	58	436	256	57
R-squared	0.070	0.104	0.183	0.059	0.120	0.457
Unit-level obs.	35839	20187	4919	19481	11595	2903

Panel B			
	(7)	(8)	(9)
Year:	1999/2000 - 2004/2005		
Districts:	All	N-P	RD
PI/II X Y0405	0.065	0.060	0.121
	(0.056)	(0.067)	(0.097)
Observations	873	512	115
R-squared	0.629	0.640	0.746
Unit-level obs.	55320	31782	7822

Notes: Dependent Variable: Agricultural Wages (logarithmic). Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ Standard errors clustered at the district level (cols. 7-9). An observation is a district mean in a given year. Dependent variable is the residual from a regression of agricultural wages on state-year-season-gender-task dummies (fully interacted). N-P stands for Non-Priority District, RD for Regression Discontinuity Sample, PI/II is a Phase I or II District Dummy, and Y04-05 is a Year 2004-05 Dummy.

Table 4.2 and the lower panel of Table 2 contain results of analogous estimations for basic indicators of the agricultural labor market. In Table 4.2 the dependent variable is a wage residual. According to columns 1, 2, 4, and 5, real wages differ vastly and in a statistically significant fashion between early and late NREGA districts in both years. Similar to the welfare measures, the initial gap of almost 20 percent in 1999–00 narrows by about one-third by 2004–05. Despite the fact that this pattern closely resembles the one encountered for the consumption measures, the hypothesis of parallel wage trends cannot be rejected for either subsample, because of lower estimation precision (columns 7 and 8).[18] This failure to reject

[18]One contributing factor is the sample size: the numbers of unit-level observations for the two consumption measures in Table 2 are about five times as large as the sample sizes reported for agricultural wages in Table 4.2.

parallel wage trends pre-NREGA mirrors the main finding of the placebo analyses of Imbert and Papp (2015) and Berg et al. (2018).

A labor market indicator for which more data is available is the fraction of households that report agricultural labor as their principal occupation. Results for this variable are set out in the lower panel of Table 2. According to the figures there, agricultural labor is significantly more common in early NREGA districts during 1999–00, but this difference has disappeared by 2004–05. As expected, early and late NREGA districts are very similar in both years within our RD sample.

Table 4: RDD Balancing Tests

	(1) Househ. size	(2) SC/ST (fraction)	(3) Land holdings (size class)	(4) Rural population (millions)	(5) Rainfall (dev. from average)
Rank (Dummy)	-0.138	0.019	-0.092	-0.111	-0.004
	(0.139)	(0.032)	(0.149)	(0.154)	(0.039)
Observations	54	54	54	54	53
R-squared	0.774	0.435	0.732	0.611	0.500
Unit-level obs.	7583	7583	7583	7583	7583

Notes: Robust standard errors in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Estimation sample: all non-priority districts from major states with at least one phase III district (14 states). All estimations include state fixed effects. Data source: NSS 64th round, schedules 1, 10 and 25, rural households. Estimation method: weighted least squares, triangular kernel, bandwidth (one-sided): 2.5. Rank (Dummy) is a dummy for the normalized rank being negative.

For the balancing tests, we estimate Equation (1) with alternative dependent variables available from the household surveys as well as rainfall. We include all non-priority districts from major states with at least one phase III district in these estimations. According to the results set out in Table 4, we find a discontinuity for none of the five covariates, in line with the RDD's identifying assumptions. To summarize, while our research design departs from a canonical RDD because the thresholds are inferred rather than exogenously given, we find no evidence against the hypothesis that assignment of program status around the implied thresholds is as good as random – which is in line with qualitative information supplied by one of the decision-makers in that process. We therefore think that our approach – while imperfect – is at least a substantial improvement over all existing studies of the NREGA's effects based on the program's rollout.

Table 5: NREGA Status and Public Works Wage Income per Rural Inhabitant

Panel A						
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.:	Program District		Wage Expenditures			
Data Source:			*Gov. of India, Admin. Records*			
			Fall 2007		Spring 2008	
States:	Star	Other	Star	Other	Star	Other
Rank (Dummy)	0.885***	0.573***	9.48***	1.99	34.07***	13.28*
	(0.102)	(0.167)	(2.39)	(1.30)	(7.33)	(7.06)
Observations	23	31	138	168	105	168
R-squared	0.86	0.42	0.35	0.89	0.49	0.16

Panel B				
	(7)	(8)	(9)	(10)
Dep. Var.:	Wage Earnings			
Data Source:	*NSS, Sch. 10*			
	Fall 2007		Spring 2008	
States:	Star	Other	Star	Other
Rank (Dummy)	-0.14	0.00	31.37*	3.03
	(0.12)	(0.00)	(17.51)	(2.82)
Observations	23	31	23	31
R-squared	0.35	1.00	0.48	0.16
Unit-level obs.	6332	9115	4832	9134

Notes: Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ State fixed effects included in all specifications. Estimation method: weighted least squares, triangular kernel, bandwidth (one-sided): 2.5. An observation is a district in cols. 1,2 and 7-10, and a district in a month in cols. 3-6. A unit-level observation is an individual in columns 7 through 10. (Unit-level) observations from Chhattisgarh, Madhya Pradesh and Rajasthan during April-June, 2008 are excluded in cols. 3-10. Wage Expenditures and Wage Earnings are Public Works Wages (monthly, current Rs. per rural inhabitant). Rank (Dummy) is a dummy for the normalized rank being negative.

4.3 Main Results

Table 5 contains estimation results for public works wage incomes in 2007–08 from two sources. Columns 3 to 6 contain estimates for the administrative records. In accordance with Figure 5, the difference in wage expenses per rural inhabitant is greatest between early and late NREGA districts in the spring of 2008. According to the intent-to-treat estimate in column 5, in the six star states, Rs. 34 more were spent in monthly wages per rural inhabitant in districts where the NREGA was active. That figure is a multiple of the effect in the low-performing eight states (column 6). Moreover, because of overall low levels of spending (see Figure 5), there are only small differences during the fall of 2007, of Rs. 9 and Rs. 2 in star and other states, respectively.

Columns 7 to 10 contain estimates for public works wage incomes from the NSS employment survey (schedule 10). While Figures 5 and 6 show that self-reported workfare wage incomes are on average substantially smaller than the corresponding outlays in the administrative data when all, including priority districts, are used, the intent-to-treat estimate for our RD sample in column 9 says that, before the onset of phase III, rural inhabitants in districts where the NREGA was active report Rs. 31 higher public works wages. This amount equals about 8 percent of the national (monthly) poverty line (see Table 2). Moreover, it almost equals the estimate obtained from the administrative data in column 5, implying only minimal leakage when the program is intensely implemented in an average "ordinary" (as opposed to a priority) district in the star states.[19] On the other hand, the RD estimates for the fall season and the other states in both seasons are virtually zero (columns 7, 8, and 10), implying a discrepancy with the administrative figures of around Rs. 10 for columns 7 and 10. Accordingly, leakage has been greater in both absolute and relative terms in instances where the program has been implemented half-heartedly: in star states during the fall season and other states during the spring season.

This nonlinear pattern implies a negative marginal rate of leakage with respect to implementation intensity, at least at high intensity levels and in the star states. This finding adds to the evidence on corruption in the NREGA. For example, Niehaus and Sukhtankar (2013b) find substantial leakage on the margin of an NREGA wage increase in the (non-star) state Orissa.

Table 6 contains results for the two labor market outcomes that we consider. According to columns 1 through 4, there is no instant effect of the NREGA on agricultural wages. While

[19] Given that we have normalized total administrative district expenditures by 2001 district populations, our per capita administrative figures can be expected to be somewhat overstated, perhaps by up to 8 percent – because they disregard the district populations' growth between 2001 and 2008.

Table 6: Agricultural Wage Rates and Occupational Pattern

Panel A				
	(1)	(2)	(3)	(4)
Dep. Var.:	Agricultural Wages (logarithmic)			
	Fall 2007		Spring 2008	
States:	Star	Other	Star	Other
Rank (Dummy)	-0.006	-0.048	-0.001	-0.116
	(0.050)	(0.082)	(0.093)	(0.087)
Observations	23	29	21	28
R-squared	0.521	0.298	0.386	0.430
Unit-level observations	819	812	669	825

Panel B				
	(1)	(2)	(3)	(4)
Dep. Var.:	Princ. Occupation: Agric. Laborer			
	Fall 2007		Spring 2008	
States:	Star	Other	Star	Other
Rank (Dummy)	-0.012	0.069	-0.137***	-0.025
	(0.049)	(0.047)	(0.042)	(0.047)
Observations	23	30	23	30
R-squared	0.740	0.581	0.819	0.647
Unit-level observations	3298	4285	2704	4264

Notes: Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.. Additional control variables not reported: state fixed effects, lagged dependent variable (from 61st round) and normalized 2007 district rainfall. Dependent variable in cols. 1-4: residuals from a unit-level data regression of logarithmic agricultural daily wages on sex and activity dummies (fully interacted). Data source cols 1-4: NSS 64th round, schedule 10. Data source cols 5-8: NSS 64th round, schedules 1, 10 and 25. Estimation method: weighted least squares, triangular kernel, bandwidth (one-sided): 2.5. A unit-level observation is an individual in cols. 1-4 and a household in columns 5-8. Unit-level observations from Chhattisgarh, Madhya Pradesh and Rajasthan during April-June, 2008 are excluded. Rank (Dummy) is a dummy for the normalized rank being negative.

the signs are negative throughout, the point estimates are mostly small in magnitude and imprecisely estimated. At least in part, this is due to the fact that our wage analysis is based on only one of the three NSS surveys fielded in 2007–08 and that agricultural labor market activities are reported by only a subset of respondents.

In contrast, the dependent variable in columns 5 through 8, agricultural labor as the household's principal activity, is available for each household in all three NSS surveys conducted in 2007–08. Consequently there are about four times as many unit-level observations in columns 5 through 8. Given a sample mean of 30 percent, the point estimate of -13.7 percentage points for the star states during the spring of 2008 implies a large effect of the NREGA on households' principal economic activity. Given that rainfall has been very similar in early and late NREGA districts (see Table 4), it is unlikely that this effect is driven by agro-climatic conditions. We have also explored to which activities households switch when reducing agricultural labor. We find small, similarly sized positive but individually insignificant effects for farming, non-agricultural labor as well as "other" activities.

Table 7 contains results for the two welfare measures, consumption and poverty. Scatter plots corresponding to columns 1 through 4 of this table are in Figure B.1 of the Appendix. We find large gains in consumption and reductions in poverty in early NREGA districts in the star states during the spring of 2008. According to the point estimate in column 3, consumption increases by 16 percent and this estimate is highly significant with a p-value of 0.011. Given mean consumption expenditures of Rs. 679 in the star states, this point estimate implies average consumption gains of close to Rs. 100. We have also explored food and non-food consumption items separately and found similarly sized increases in both categories, which we do not report in the tables. Poor households benefit disproportionately as poverty during the spring season decreases by 16 percentage points (p-value 0.018), more than one-third of the sample average of 38 percent (see Table 2). Parallel to the changes in principal occupation, these welfare gains accrue exclusively in star states during the spring season. The consumption gains implied by the point estimate in column 3 are a multiple of the wage outlays and income effects reported in columns 5 and 9 of Table 5. They are, on the other hand, of a similar magnitude as the wage expenditures reported in the administrative data for early NREGA districts in the star states during the spring of 2008, displayed in Figure 5, which average around Rs. 80.

As in Muralidharan et al. (2017), the magnitude of our welfare estimates suggests sizable general equilibrium effects of the NREGA in addition to the modest direct income effects reported in Table 5. While we find no sufficiently precise evidence for spillovers on agricultural

Table 7: Consumption and Poverty

Panel A				
	(1)	(2)	(3)	(4)
Dep. Var.:	MPCE (logarithmic)			
	Fall 2007		Spring 2008	
States:	Star	Other	Star	Other
Rank (Dummy)	-0.066	-0.033	0.160**	-0.050
	(0.068)	(0.049)	(0.054)	(0.048)
Observations	23	30	23	30
R-squared	0.675	0.907	0.774	0.893
Unit-level observations	3298	4285	2704	4264

Panel B				
	(1)	(2)	(3)	(4)
Dep. Var.:	Poverty Headcount Ratio			
	Fall 2007		Spring 2008	
States:	Star	Other	Star	Other
Rank (Dummy)	0.035	0.050	-0.163**	0.052
	(0.062)	(0.050)	(0.061)	(0.047)
Observations	23	30	23	30
R-squared	0.627	0.749	0.614	0.815
Unit-level observations	3298	4285	2704	4264

Notes: Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Additional control variables not reported: state fixed effects, lagged dependent variable (from 61st round) and normalized 2007 district rainfall. Data source: NSS 64th round, schedules 1, 10 and 25. Estimation method: weighted least squares, triangular kernel, bandwidth (one-sided): 2.5. An unit-level observation is a household. Unit-level observations from Chhattisgarh, Madhya Pradesh and Rajasthan during April-June, 2008 are excluded. Rank (Dummy) is a dummy for the normalized rank being negative.

wage rates, higher earnings from other than agricultural labor market activities or self employment, reductions in savings or expansions in borrowing (as in Kaboski and Townsend, 2012), and additional productivity of the assets created by the public works (Gehrke and Hartwig, 2018) are possible channels.

4.4 Robustness

To assess the internal validity and robustness of our main results, we conduct several validity and robustness checks. Appendix Tables B.1 and B.2 contain results of placebo experiments for labor and welfare outcomes for the 'thick' NSS round preceding the 64th, the 61st fielded in 2004–05. Scatter plots for consumption in the star states corresponding to columns 1 and 3 of Table B.2 are in Figure B.2 of the Appendix. The sample sizes and estimation precision are similar to those of our main analysis. All point estimates are small and statistically far from significant.

In a second robustness check, we cease to eliminate observations from the three star states Chhattisgarh, Madhya Pradesh, and Rajasthan, where the NREGA's phase III commenced intensively in the second quarter of 2008, hence contaminating the control group of our research design. The results for the 2008 spring season are set out in Table B.3. Relative to columns 3 and 7 of Tables 6 and 7, the number of observations increases by about one-fifth. In accordance with the hypothesis that outcomes in the phase III districts are now more similar to those in the early NREGA districts, the point estimates for principal occupation, MPCE, and poverty are all attenuated by about one-third. They remain statistically significant at conventional levels nonetheless.

We have criticized previous authors' empirical approaches to the rollout of the NREGA for ignoring other major welfare programs that were rolled out in 2006 and 2007, concurrent to the NREGA: (1) the Backward Regions Grant Fund (BRGF), active from the fiscal year 2007–08 onward with a central budget allocation of around Rs. 200 per year and rural inhabitant in program districts (Government of India, 2014) and (2) the Prime Minister's Rehabilitation Package for Farmers in Suicide Prone Districts with a central budget allocation of around Rs. 1,000 per inhabitant in program districts per year, active from the fiscal year 2006–07 onward (Bhende and Thippaiah, 2010). For comparison, the NREGA's central budget allocation per inhabitant in program districts stood around Rs. 200 in 2007–08. The BRGF covered 250 and the Rehabilitation Package 32 districts. Together, the two programs covered 265 of the 330 NREGA phase I and II districts in 2007–08.

Our estimation samples also contain a few districts where the BGRF was active. Hence, in a third robustness check, we eliminate all BRGF districts from our RDD samples. This concerns two districts in Rajasthan and one in Chhattisgarh among the star states and a total of five districts in the other states. The welfare results for these samples are set out in Table B.4. The point estimates for the spring season in the star states are slightly smaller, consistent with the hypothesis that the BRGF had some additional positive effect on consumption. The magnitude of the change in these coefficients relative to Table 7 is, however, minimal.

Another concern is the possibility of spillovers between districts (Muralidharan et al., 2016), which may run in either direction (Merfeld, 2019). As a fourth robustness check, we include as a control variable the fraction of a district's border abutting a district where the NREGA is active. For the star states the mean of this variable is 50.4 percent with a standard deviation of 24 percentage points. The results for the star states in spring 2008 are set out in Table B.5. The point estimate for this spillover proxy variable is negative for consumption and positive for the headcount ratio. The point estimates of the NREGA's effect become a little smaller for consumption and poverty but remain significant. We have also estimated specifications where we interact the spillover measure with the *Normalized Rank negative* dummy. The results remain qualitatively unchanged. We take this as evidence against significant spillovers of the NREGA in its early stages across district boundaries – at least with respect to consumption and occupational choice.

In a fifth robustness check, we explore nonparametric local linear regression with automatic bandwidth choice (Calonico et al., 2014). We are aware that this is a slight abuse of their methodology, which requires a continuous running variable. Be that as it may, the results for the star states, set out in Table B.6, are almost identical to the ones reported in Tables 6 and 7. Remarkably, at least for consumption and poverty in spring (columns 6 and 8), their algorithm chooses a bandwidth that is very similar to the one we have used for our main results. The only mentionable difference occurs for agricultural wage rates, which increase significantly by 9.5 percent (column 1). While this estimate mirrors – both qualitatively and quantitatively – the ones obtained by Imbert and Papp (2015) and Merfeld (2019) in DID estimations, we are inclined to discount it somewhat given that the RD bandwidth is less than 1.5 and hence only the 12 districts immediately neighboring the threshold are included by the estimation algorithm.

Our sixth robustness check uses time differences in district means between 2004–05 and 2007–08 as dependent variables. According to Table B.7, where the results for the star states are set out, the welfare effects are virtually identical to the ones reported in Table 6. A difference regarding statistical significance occurs for the principal occupation in the spring of 2008 (column 4). While the large negative point estimate of -10.2 percentage points is similar to the one in column 7 of Table 6 (-13.7), the estimation precision is almost halved (standard error of 8.0 in comparison to 4.2 percentage points). Accordingly, we do not view this result as contradicting our main finding regarding the NREGA's effect on households' principal occupation.

In a seventh robustness check, we use the NSS's urban rather than the rural subsample. The welfare results are set out in Table B.8.[20] In accordance with the hypothesis that the NREGA should affect only the rural economy, at least in the short run, the point estimates are all very small for the star states in spring 2008 (columns 3 and 7) and statistically far from significant.

Finally, we estimate Equation (1) with data from the 66th NSS round fielded in 2009–10, the second year in which the NREGA covered all of India's districts. As for the 61st NSS round, we use schedules 1 and 10, which gives sample sizes similar to our main analyses. According to the results set out in Table B.9 for the star states, there is no significant discontinuity for any of the four outcomes that we consider – consistent with the pattern that the control districts of our main analysis enjoyed similar benefits as the treatment districts two years later. Still, the large positive, albeit imprecisely measured, agricultural wage rate increase of 15.7 percentage points in column 1, which is for the fall 2009 season, stands out. It is qualitatively consistent with the results of Berg et al. (2018), who find persistent increases in agricultural wages in star states during the agricultural peak season due to the NREGA within a DID estimation framework using all of India's districts.

4.5 Other Outcomes

The principal purpose of our study has been to assess short-term welfare effects of the NREGA. As an extension, we consider additional outcomes available from the NSS surveys fielded in 2007–08 that have received recent prominent attention (Sukhtankar, 2016), namely migration (Imbert and Papp, 2019), school attendance, and child labor (Shah and Steinberg, 2015; Afridi et al., 2016). A limitation in our approach for studying these outcomes is that all three of them are recorded only in the employment-unemployment survey (schedule 10), which severely decreases the number of unit-level observations relative to our main analysis, where we employ three schedules simultaneously. Another shortcoming of

[20]Since there is no immediately analogous wage rate or modal principal occupation, we do not conduct this analysis for the labor market outcomes.

the NSS data regarding migration is that schedule 10 of the 64th round records only the number of individual temporary migrations during the 365 days preceding the interview. Hence more than half of the recall period of households intensely exposed to the NREGA in spring 2008 covers a period with virtually no NREGA activity. In addition, no migration questions were included in the 61st NSS round's surveys and, thus, no recent lagged values are available as a control.

Table 8: Migration

	(1)	(2)	(3)	(4)
	Fall 2007		Spring 2008	
	Star States	Other States	Star States	Other States
Normalized rank negative (dummy)	-0.109	0.020	-0.231	0.019
	(0.124)	(0.050)	(0.189)	(0.041)
Observations	23	30	23	30
R-squared	0.291	0.335	0.424	0.618
Unit-level observations	1498	1944	1228	1939

Notes: Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ Additional control variables not reported: state fixed effects and normalized 2007 district rainfall. Data source: NSS 64th round, schedule 10, rural households. Dependent variable: number of migrations during the 365 days preceding the interview. Estimation method: weighted least squares. An observation's weight is the product of the district's cumulative sample weight and the triangular kernel weight, bandwidth (one-sided): 2.5. A unit-level observation is a household.

Be that as it may, sample means for migration and regression results are set out in Tables A8 and 8, respectively. According to the former, rural households report 0.22 temporary migrations per year on average. The point estimates in Table 8 show a pattern that is qualitatively similar to the welfare results: the negative estimate for the star states in spring stands out. Qualitatively consistent with the findings of Imbert and Papp (2019), there is a decrease in migration with a point estimate that is similar in magnitude to the sample mean. It is, however, imprecisely measured and therefore not statistically significant.

Jacoby and Skoufias (1997) have shown for villages in South India that poor rural households increase child labor at the expense of schooling to mitigate income shortfalls during the agricultural slack season, with no significant gender asymmetries. Given that the NREGA can compensate seasonal consumption shortfalls, their model of credit market imperfections predicts that, in the star states in spring, child labor should decrease and schooling increase. Guided by a number of recent studies on the NREGA's effect on schooling and child labor, which all stress the importance of heterogeneous effects by age (see the next paragraph for citations), we partition school-aged children and adolescents into two brackets, 5–12 and 13–18 year olds, and use as a dependent variable the number of days an educational institution was attended in the month preceding the interview.[21] According to Table B.10, about 25 and 17 days are spent in school by children and adolescents, respectively, while work activities are performed one day and 10 days on average, respectively. Boys and girls attend school at similar rates, but female adolescents attend school about 17 percent less often than their male counterparts and instead report 50 percent more workdays, mostly devoted to domestic chores.

Table 9: School Attendance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Children (5-12 years)				Adolescents (13-18 years)			
	Fall 2007		Spring 2008		Fall 2007		Spring 2008	
	Star States	Other States	Star States	Other States	Star States	Other States	Star States	Other States
Rank (Dummy)	1.84	-1.10	0.89	-1.61	2.50	-0.23	5.54***	0.92
	(1.05)	(1.02)	(1.16)	(1.51)	(2.24)	(1.42)	(1.77)	(1.49)
Observations	23	30	23	30	23	30	23	30
R-squared	0.71	0.45	0.70	0.35	0.34	0.67	0.70	0.81
Unit-level obs.	1031	1538	747	1509	790	1193	531	1172

Notes: Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Additional control variables not reported: state fixed effects, lagged dependent variable (from 61st round) and normalized 2007 district rainfall. Data source: NSS 64th round, schedule 10, rural households. A unit-level observation is an individual. Dependent variable: days attended educational institution during the month preceding the interview. Estimation method: weighted least squares, triangular kernel, bandwidth (one-sided): 2.5. Rank (Dummy) is a dummy for the normalized rank being negative.

According to the results set out in Tables 9 and 10, the NREGA has a large and significant positive effect on adolescents' school attendance and a similar-sized negative effect on adolescent labor in the star states during the spring season. The additional heterogeneous effects by sex and activity reported in Table 11 suggest that male and female adolescents each reduce the work activities in which they are usually most strongly engaged, productive work for males and domestic chores for females, and that both sexes enjoy gains in school attendance. Due to small samples and limited precision, only the school attendance and productive work estimates for males are significant at conventional levels, however. Given that adolescents are more productive in work activities than children, this pattern is precisely in line with the predictions of Jacoby and Skoufias' (1997) model. Put differently, through its insurance function

[21]The individual-level data of the employment survey records activities during the seven days preceding the interview for all household members aged five years and older.

Table 10: Child and Adolescent Labor

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Children (5-12 years)				Adolescents (13-18 years)			
	Fall 2007		Spring 2008		Fall 2007		Spring 2008	
	Star States	Other States	Star States	Other States	Star States	Other States	Star States	Other States
Rank (Dummy)	-0.03	-0.13	-0.74	0.75	-2.66	-0.19	-4.13**	-0.65
	(0.37)	(0.66)	(0.81)	(0.47)	(2.11)	(1.26)	(1.60)	(1.49)
Observations	23	30	23	30	23	30	23	30
R-squared	0.54	0.19	0.43	0.44	0.43	0.71	0.66	0.79
Unit-level obs.	1031	1538	747	1509	790	1193	531	1172

Notes: Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Additional control variables not reported: state fixed effects, lagged dependent variable (from 61st round) and normalized 2007 district rainfall. Data source: NSS 64th round, schedule 10, rural households. A unit-level observation is an individual. Dependent variable: days with labor activity during the month preceding the interview. Estimation method: weighted least squares, triangular kernel, bandwidth (one-sided): 2.5. Rank (Dummy) is a dummy for the normalized rank being negative.

Table 11: Schooling and Labor of Adolescents, Star States, Spring 2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimation sample:	Male Adolescents (13-18 years)				Female Adolescents (13-18 years)			
Dependent variable:	School	Work			School	Work		
	Att.	All	Prod.	Dom.	Att.	All	Prod.	Dom.
Rank (Dummy)	7.88**	-4.78	-4.75*	0.11	3.58	-4.16	-0.60	-3.44
	(3.51)	(2.87)	(2.38)	(0.18)	(3.41)	(3.39)	(2.17)	(3.38)
Observations	23	23	23	23	23	23	23	23
R-squared	0.54	0.47	0.56	0.47	0.42	0.42	0.28	0.51
Unit-level obs.	281	281	281	281	250	250	250	250

Notes: Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Additional control variables not reported: state fixed effects, lagged dependent variable (from 61st round) and normalized 2007 district rainfall. Data source: NSS 64th round, schedule 10, rural households. A unit-level observation is an individual. Data source: NSS 64th round, schedule 10, rural households. Dependent variables: days attended educational institution (col. 1, 5), days with labor activity (col. 2, 6), days with productive labor activity (col. 3, 7), days with domestic chores (col. 4, 8) during the month preceding the interview. Estimation method: weighted least squares, triangular kernel, bandwidth (one-sided): 2.5. Rank (Dummy) is a dummy for the normalized rank being negative. Dependent variables are School Attendance (Att.), Work (All, Productive, Domestic).

the NREGA appears to also mitigate failures in the credit market regarding the smoothing of households' income fluctuations. Our results support, moreover, the view that the income effect of the NREGA dominates an opposite substitution effect, which works through an increased opportunity cost of time in school, particularly for older kids (Sukhtankar, 2016).

Our findings are qualitatively in line with Afridi et al. (2016) and Mani et al. (2019), who study six districts in one state around the NREGA's onset. Our findings contrast those of Shah and Steinberg (2015), who estimate schooling and child labor effects of the NREGA's onset. They conduct DID estimations with NSS data from all Indian districts and find patterns precisely opposite to the ones reported in Tables 9 through 11.

It would be desirable to identify in more detail the channels through which the NREGA facilitated the substantial seasonal consumption gains in the star states documented in our main results. In particular, what are the direct and what are indirect or general equilibrium effects of this program? Unfortunately, important variables such as total labor market earnings or days with no employment (see e.g. Muralidharan et al., 2016) are available in only one of the NSS schedules that we use, and the estimates for these outcomes are similarly imprecise as for wage rates or migration previously. Other variables capturing the productivity of the collective and private assets produced by the NREGA, which may contribute indirectly to private gains from the program, are not available in NSS data. The same applies to household borrowings and savings as well as measures of intra-household allocations. Relatedly, limited sample sizes and a large variation in caste composition across our RDD sample districts do not allow the robust identification of heterogeneous effects by caste or social group.

5 Conclusion

In this study of India's rural employment program we have found large, perhaps transformative effects and no leakage for 'ordinary' districts in states where the NREGA was implemented intensely. All effects are concentrated on the agricultural slack season when rural unemployment is high and the program is most active. In contrast, we have found no program effects and much leakage in states that implemented the program half-heartedly.

We think the strengths of our analysis are the combination of several data sources and the novel research design, which takes seriously the rules by which districts were allocated to different phases of the staggered rollout of the program. We have shown how this generates quasi-experimental variation in program status for a subgroup of India's districts. We thereby improve on several studies of this program with regards to three major challenges formulated by Muralidharan et al. (2017): a lack of experimental variation, construct validity, and the extent of effective NREGA presence, as well as spillovers across district boundaries, in our context.

A limitation of our empirical analysis is the precision of the estimated program effects. It is rooted in the small number of high-program-intensity districts in our research design, which are home to less than 5 percent of India's entire population. Moreover, guided by the seasonality of the program's implementation, we conduct disaggregated analyses by agricultural season, which further cuts sample sizes. A second limitation relates to the scope of our analysis. Driven by the objective of identifying causal program effects, our research design is focused on only a subgroup of 'ordinary' districts, ones that are not plagued by violent political extremism, high incidences of farmer suicides, or dismal human development indicators.

We have documented that, when rigorously implemented, households enjoy seasonal consumption gains that are a multiple of the direct wage earnings from the NREGA. Moreover, older children's school attendance increases significantly. This suggests that there are substantial general equilibrium effects of the program, even in the short term, as was by Muralidharan et al. (2016) who studied the introduction of smartcards as an implementation improvement in the NREGA. Our heterogeneous findings for star versus other states, moreover, support the view that general equilibrium effects only kick in when the program is implemented with sufficient intensity, implying hugely increasing marginal social returns to program outlays. We conclude that rural employment programs hold significant potential for not only increasing consumption levels but also for insuring households against various adverse implications of seasonal drops in employment and income.

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Appendix (for Online Publication)

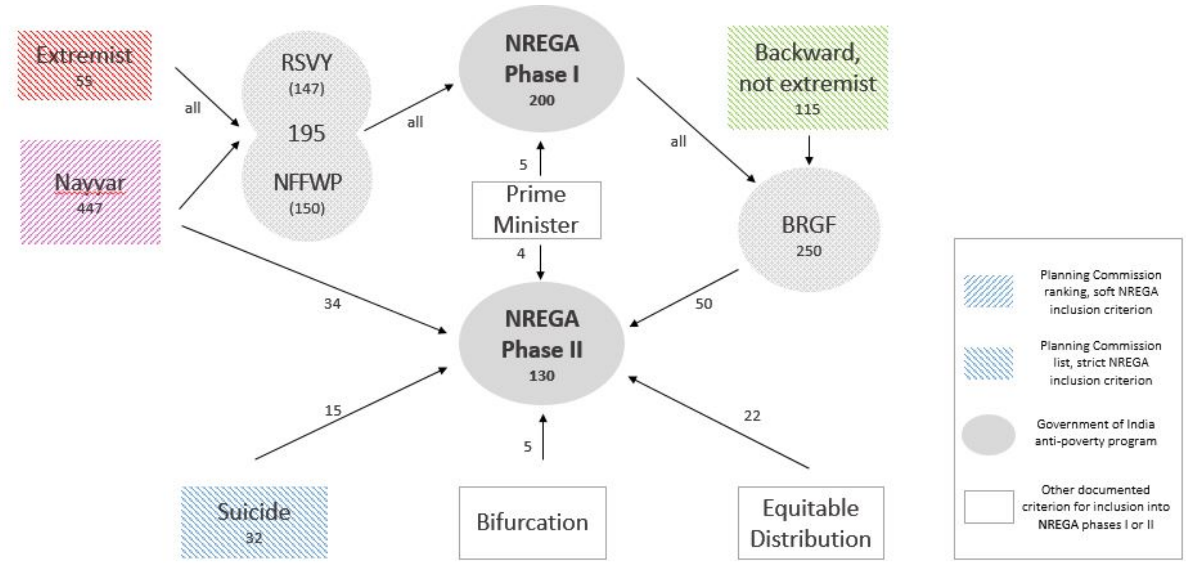
A Details of the Research Design

Districts in the first NREGA phase comprise all districts covered by two earlier centrally sponsored programs, RSVY (Rashtriya Sam Vikas Yojana) with 147 districts and NFFWP (National Food for Work Programme) with 150 districts, 195 districts in total, plus five districts that were added at the discretion of the Planning Commission's chairman, India's Prime Minister Manmohan Singh. For the selection of both RSVY and NFFWP districts, two criteria were of central importance. First, all 55 districts identified by the Planning Commission as being affected by extremist (mostly Maoist) conflict (Planning Commission, 2005) were included in at least one of the two programs. The remaining 140 districts were added to the two programs according to state-wise poverty rates, a concern for equitable distribution across states, and, within states, according to the "backwardness" of districts, where backwardness was evaluated according to a Planning Commission ranking published in the 2003 report *Identification of Districts for Wage and Self Employment Programmes* (Government of India, 2003).[22]

While no specific criteria were mentioned to us regarding the choice of the additional five districts chosen by the Planning Commission chairman, the data shows that all of them are ranked low on the said backwardness ranking in their respective states. The upper left section of Figure A.1 illustrates the selection criteria for NREGA phase I districts.

[22]To the best of our knowledge, these rules guiding the choice of NREGA's phase I districts are not available from publicly accessible documents. They were kindly shared with us by Dr. Rinku Murgai from the World Bank's New Delhi office.

Figure A.1: Selection Criteria for NREGA Phase I and II Districts



Notes: All numbers are numbers of districts. RSVY: Rashtriya Sam Vikas Yojana; NFFWP: National Food for Work Programme; NREGA: National Rural Employment Guarantee Act; BRGF: Backward Regions Grant Fund.

Since this backwardness ranking plays a crucial role in our research design, we will briefly digress and provide some background on it. In 2001, the Planning Commission appointed a task force headed by Dr. Rohini Nayyar with the goal of identifying districts for the targeting of future employment programs. The final report of this task force (Government of India, 2003) contains a backwardness ranking for 447 districts in India's 17 most populous states,[23] which is calculated from the percentage of SC/ST (scheduled caste and scheduled tribe) population, agricultural output per worker, and the agricultural wage rate around the year 2000. We will refer to a district's rank on that list as its backwardness rank in the sequel.

Returning to the 200 phase I districts, we record that 55 of them are chosen according to a strict rule, violent political extremism, while for the remaining 145 the soft rule of a low Nayyar rank applies, at least within state. For phase II, which commenced in the Indian fiscal year 2007-08, we rely on a press release from the Minstry of Rural Development from March 2007, which lists six criteria according to which 130 additional program districts were selected (see Figure A.2). These are (i) poor outcomes regarding human development indicators published in Government of India (2005) (50 districts), (ii) high incidence of farmer suicides (15), (iii) partitions of phase I districts (5), (iv) a low rank on the Nayyar district

[23] These are Andhra Pradesh, Assam, Bihar, Chhattisgarh, Gujarat, Harayana, Jharkhand, Kerala, Karnataka, Madhya Pradesh, Maharashtra, Odisha, Punjab, Rajasthan, Tamil Nadu Uttar Pradesh and West Bengal.

backwardness ranking (34), (v) equitable distribution across states (22), (vi) discretion of the Planning Commission chairman (4).

Figure A.2: Press Release on NREGA Phase II Districts by India's Ministry of Rural Development (Government of India, 2007a)

Press Information Bureau
Government of India
Ministry of Rural Development

09-March-2007 16:49 IST

130 Additional Districts included under NREGA

The coverage under National Rural Employment Guarantee Act (NREGA) has further been extended to 130 additional districts in 27 States of the country. Names of the districts to be added are given in the **Annexure.** The List of Uttar Pradesh districts will be issued later.

These additional districts have been identified on the following criteria:

(i) 50 districts from the list of BREG districts,

(ii) 15 additional districts with the incidence of farmers suicide,

(iii) 5 additional districts that were bifurcated and created with the district with original name already included in the NFFWP/RSVY/BREG. The newly created districts with new names which were left out of these programmes are now included.

(iv) 34 districts have been selected from the Planning Commission Ranking of Backwardness used for identifying NFFWP districts,viz, SC/ST population, inverse of agricultural productivity, and inverse of agricultural wages.

(v) In order to make an equitable distribution, at least two districts in addition have been selected from each special category state as well and from States whose minimum number is not covered in items (i) to (iv) above. This covers 22 districts.

(vi) 4 districts have been taken up as Learning Impact Districts.

Initially, NREGA was notified in 200 districts of the country on February 2, 2006 after it was passed as The National Rural Employment Guarantee Act, 2005 with an unanimous consent. In these districts 1.66 crore households have been provided employment, generating 64 crore person days of employment on 6 lakh works. As per the provisions of this act NREGA guarantees local employment in the form of unskilled manual work up to 100 days in a financial year to every rural household if it demands such employment.

While we were not able to retrieve lists of districts that were included under each of these criteria, our own analysis of phase I and II districts reveals that the numbers of districts mentioned under the first two criteria in the press release are precisely consistent with including all districts that appear on two pre-existing NREGA lists by 2007–08. While several of these districts had already been included in phase I, the first two criteria essentially serve to ensure that all districts on these two lists are covered by the NREGA by 2007–08. For the first criterion, backwardness according to human development, there are precisely 50 districts on the list "115 Most Backward Districts excluding 55 Extremist Affected Districts" published in Government of India (2005) that are not already covered in phase I. For the second one, farmer suicides, there are precisely 15 districts mentioned in the Government of India's press release "Rehabilitation Package for the Farmers in Suicide Prone Districts of Andhra Pradesh, Karnataka, Kerala and Maharashtra", published in September 2006, that are not already covered in phase I and do not come under the previous criterion.[24] Regarding the third criterion, we were able to identify precisely five districts that were carved out of phase I districts between 2001 and 2003.

The remaining 60 phase II districts were apparently not selected following a strict rule derived from a pre-existing priority list. Instead, item iv of Figure A.2 implies that a low rank in Government of India (2003) was used as criterion for inclusion into phase II of the NREGA for at least 34 districts. Figure A.1 illustrates these selection criteria for NREGA phase II districts.

Figure A.3: Euler Diagram of the 479 Census 2001 Districts in India's 17 Major States

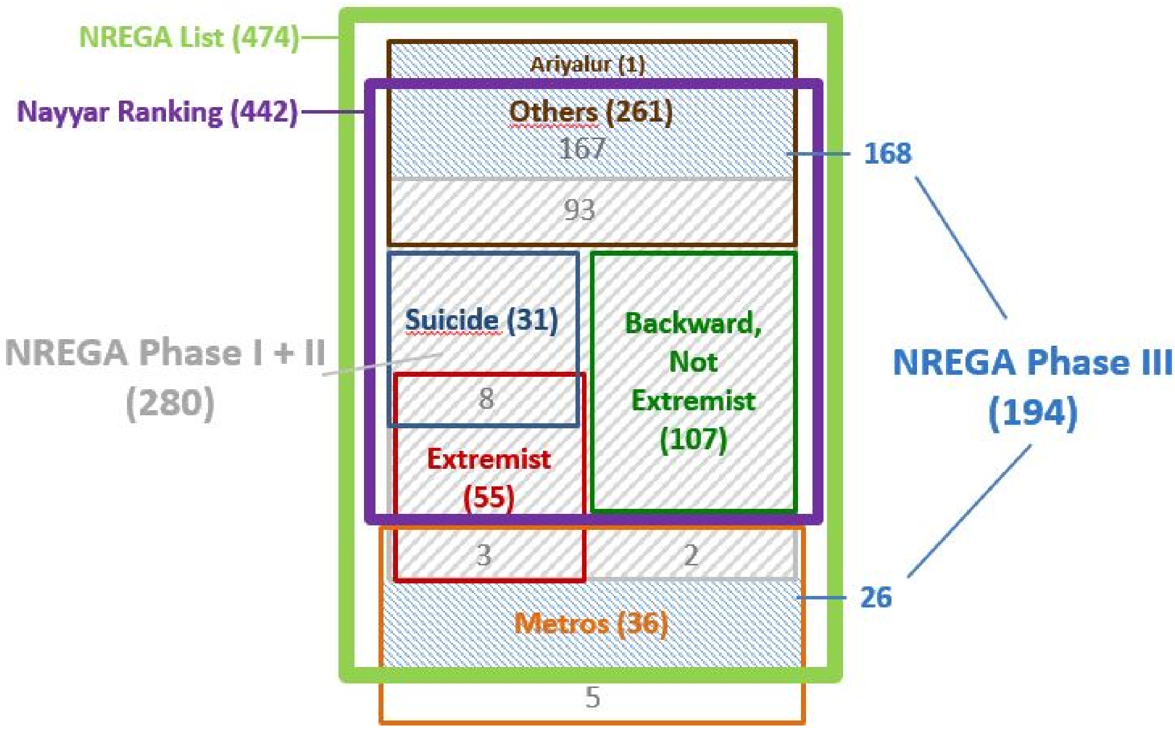


Figure A.3 depicts the resulting sets of districts for India's 17 major states. Accordingly, from the union set of 280 phase I and II districts, 185 can be singled out that are included in at least one of the three priority lists, Extremist-Affected Districts (55), Most Backward Excluding

[24] This press release mentions 32 districts, 31 on a list published in Government of India (2007b) (see Annexure A), and mentions one additional district, Idukki in Kerala.

Extremist (115), and Farmer Suicides (32). While the first two lists are disjoint by construction, the intersections of the Farmer Suicides list with the first two comprise 17 districts, hence resulting in a union set of the three lists of 185 districts. For the remaining 145 phase I and II districts, it is suggestive that a low Nayyar rank is a key predictor, at least within each state.

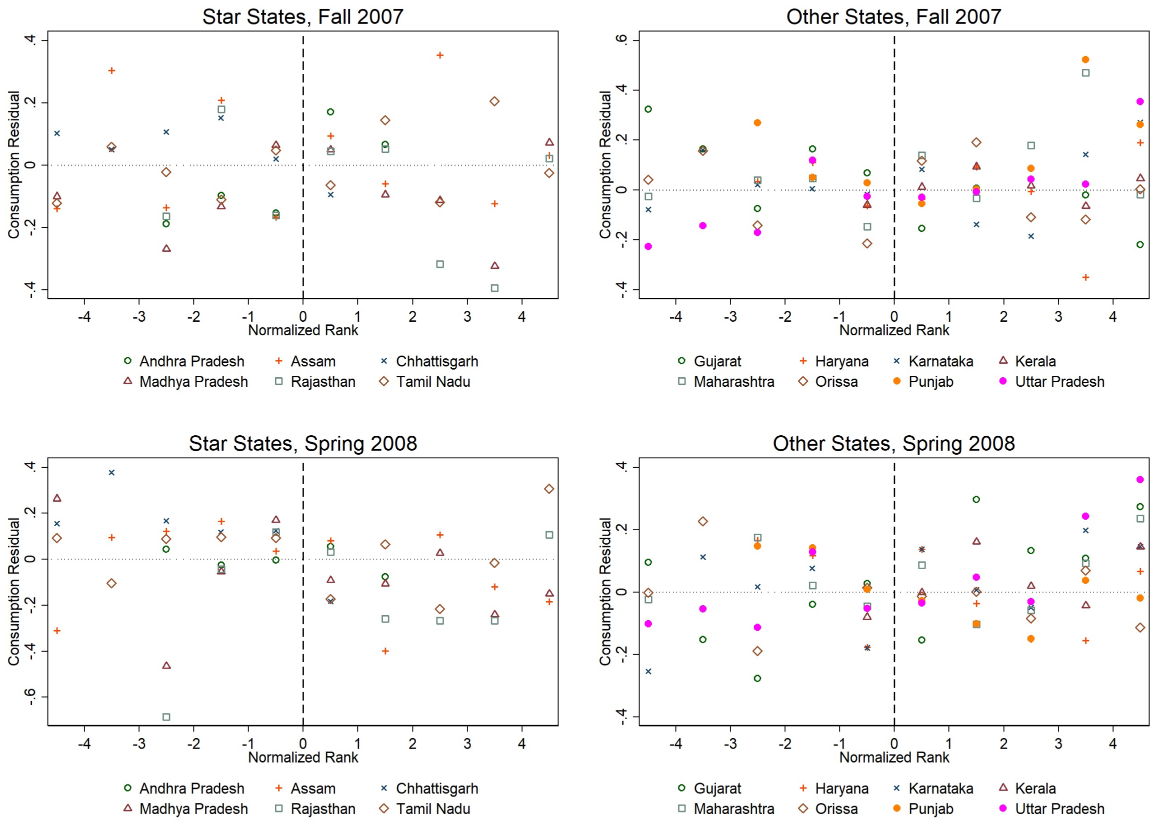
To explore this hypothesis, we start out with all 479 districts in these 17 states included in the 2001 Census of India, which provides the sampling frame for the National Sample Surveys until 2011, the sources of outcome data in our empirical analysis. Of these 479 districts, 442 are included in the Nayyar ranking (the purple-bordered set in Figure A.3). The difference is due to the deliberate omission of 36 primarily metropolitan districts from the Nayyar ranking and one (non-metropolitan) district in Tamil Nadu.[25] On the other hand, there are five districts that were created between April 2001 and January 2002 and included in the Nayyar ranking, which comprises 447 districts, but which were not in the 2001 Census.

Since in our impact analysis we are exclusively concerned with rural areas, we disregard the 36 metropolitan districts (the orange-bordered set in Figure A.3), which leaves us with 443 districts. When we also sideline all districts on the three priority lists, we are left with 261 districts (the 'Others' set in Figure A.3), of which 260 are included in the Nayyar ranking. These 260 districts form our subject pool (the 'Others' set less the Ariyalur district in Figure A.3), plotted as circles in Figures 2 and 3. Of these, 93 belong to phases I and II, while 167 were covered by the NREGA only in phase III.

[25]This is the district of Ariyalur, which was newly created in January 2001 and included in the 2001 census but not in the Nayyar ranking, probably because it was merged with another district and ceased to exist in April 2002.

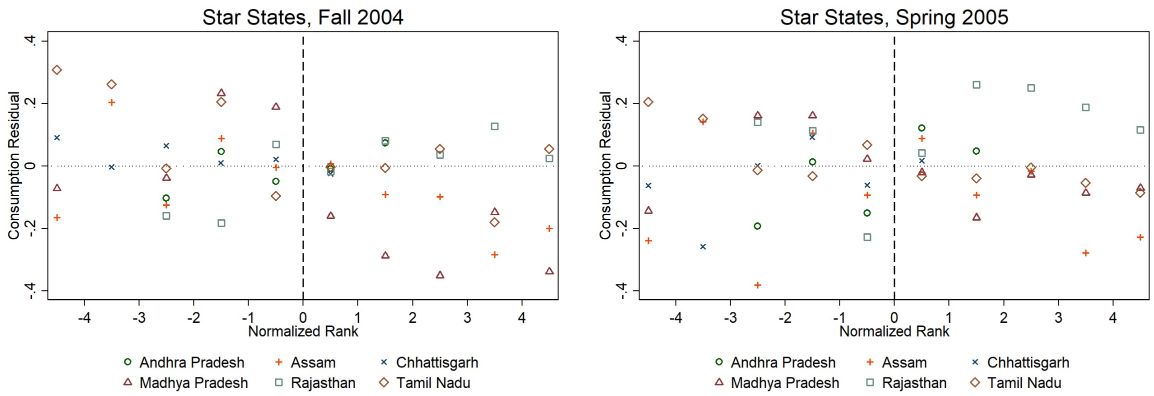
B Figures and Tables

Figure B.1: Mean Consumption (logarithmic) Residuals, RDD Sample Districts, 2007–08



Notes: Residuals are obtained from a regression of district means on state fixed effects, the lagged dependent variable and district rainfall using a triangular kernel with bandwidth 2.5 (one-sided).

Figure B.2: Mean Consumption (logarithmic) Residuals, RDD Sample Districts, 2004–05 (Placebo)



Notes: See Appendix Figure B.1

Table B.1: Agricultural Wage Rates and Occupational Pattern, Placebo

Dep. Var.:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Agric. Wages (log.)				Princ. Occ.: Agric. Laborer			
	Fall 2007		Spring 2008		Fall 2007		Spring 2008	
States:	Star	Other	Star	Other	Star	Other	Star	Other
Rank (Dummy)	0.049	-0.029	0.166	-0.066	-0.040	0.032	-0.022	0.009
	(0.060)	(0.118)	(0.156)	(0.070)	(0.042)	(0.046)	(0.051)	(0.043)
Observations	22	26	23	28	23	30	23	30
R-squared	0.596	0.464	0.225	0.539	0.836	0.650	0.775	0.713
Unit-level obs.	737	618	608	654	3472	4255	3476	4270

Notes: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Additional control variables not reported: state fixed effects, lagged dependent variable and rainfall. DV: residuals from a unit-level data regression of logarithmic agricultural daily wages on sex and activity dummies. Data source: NSS 61st round, schedule 10 (cols 1-4), schedules 1 and 10 (cols. 5-8); NSS 55th round, sch. 1 (cols 1-4), sch. 1 and 10 (cols 5-8) for lagged dependent variables. Estimation method: weighted least squares, triangular kernel, bandwidth (one-sided): 2.5. Rank (Dummy) is a dummy for the normalized rank being negative.

Table B.2: Consumption and Poverty, Placebo

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.:	MPCE (logarithmic)				Poverty Headcount Ratio			
	Fall 2007		Spring 2008		Fall 2007		Spring 2008	
States:	Star	Other	Star	Other	Star	Other	Star	Other
Rank (Dummy)	0.076	-0.008	-0.048	0.079	-0.080	-0.025	0.032	-0.092
	(0.059)	(0.076)	(0.055)	(0.058)	(0.073)	(0.089)	(0.045)	(0.070)
Observations	23	30	23	30	23	30	23	30
R-squared	0.552	0.812	0.674	0.875	0.536	0.537	0.722	0.635
Unit-level obs.	3472	4255	3476	4270	3472	4255	3476	4270

Notes: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ Additional control variables not reported: state fixed effects, lagged dependent variable (from 55th round) and normalized 2004 district rainfall. Data source: NSS 61st round, schedules 1 and 10. Estimation method: weighted least squares, triangular kernel, bandwidth (one-sided): 2.5. Rank (Dummy) is a dummy for the normalized rank being negative.

Table B.3: Agricultural Wage Rates, Occupational Pattern, Consumption and Poverty, Star States, Spring 2008, with All Observations from 2008

Dep. Var.:	(1) Ag. Wages (log.)	(2) Princ. Occ.: Ag. Laborer	(3) MPCE (log.)	(4) Poverty HCR
Rank (Dummy)	0.075	-0.103**	0.119**	-0.115*
	(0.106)	(0.041)	(0.051)	(0.060)
Observations	22	23	23	23
R-squared	0.400	0.784	0.790	0.618
Unit-level obs.	733	3297	3297	3297

Notes: See Tables 6 and 7. Rank (Dummy) is a dummy for the normalized rank being negative.

Table B.4: Consumption and Poverty without BRGF Districts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.:	MPCE (logarithmic)				Poverty Headcount Ratio			
	Fall 2007		Spring 2008		Fall 2007		Spring 2008	
States:	Star	Other	Star	Other	Star	Other	Star	Other
Rank (Dummy)	-0.090	-0.086*	0.146**	-0.052	0.056	0.070	-0.155**	0.039
	(0.079)	(0.049)	(0.057)	(0.044)	(0.059)	(0.048)	(0.062)	(0.038)
Observations	20	25	20	25	20	25	20	25
R-squared	0.730	0.932	0.803	0.909	0.761	0.782	0.685	0.883
Unit-level obs.	3034	3581	2572	3561	3034	3581	2572	3561

Notes: Districts excluded where the Backward Regions Grant Fund was active in 2007-08. Rank (Dummy) is a dummy for the normalized rank being negative. See Table 7 for further notes.

Table B.5: Occupational Pattern, Consumption and Poverty, Star States, Spring 2008, Controlling for Spillovers from Neighboring Districts

Dependent Variable:	(1) Princ. Occ.: Ag. Laborer	(2) MPCE (log.)	(3) Poverty HCR
Normalized rank negative (dummy)	-0.144***	0.129**	-0.135**
	(0.041)	(0.049)	(0.062)
Fraction of district border with NREGA district	-0.061	-0.217	0.198
	(0.118)	(0.139)	(0.138)
Observations	23	23	23
R-squared	0.821	0.806	0.650
Unit-level observations	2704	2704	2704

Notes: Rank (Dummy) is a dummy for the normalized rank being negative. See Tables 6 and 7 for further notes.

Table B.6: Agricultural Wage Rates, Occupational Pattern, Consumption and Poverty, Nonparametric RD Estimation with Automatic Bandwidth Choice, Star States

Panel A

	(1)	(2)	(3)	(4)
	Agric. Wages (log.)		Princ. Occ.: Ag. Lab.	
	Fall 2007	Spring 2008	Fall 2007	Spring 2008
Rank (Dummy)	0.095*	-0.126	-0.084	-0.136**
	(0.056)	(0.088)	(0.063)	(0.056)
Bandwidth (one-sided)	1.096	2.017	1.245	1.870
Obs. left	6	10	6	11
Obs. right	6	10	6	12

Panel B

	(5)	(6)	(7)	(8)
	MPCE (log.)		Poverty HCR	
	Fall 2007	Spring 2008	Fall 2007	Spring 2008
Rank (Dummy)	-0.115	0.181***	0.056	-0.183***
	(0.073)	(0.069)	(0.067)	(0.068)
Bandwidth (one-sided)	1.947	2.054	1.759	2.412
Obs. left	11	11	11	11
Obs. right	12	12	12	12

Notes: Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Additional control variables not reported: state fixed effects, lagged dependent variable (from 61st round) and normalized 2007 district rainfall. Dependent variables: residuals from a unit-level data regression of logarithmic agricultural daily wage on sex and activity dummies, fully interacted (cols 1-2). Data source: NSS 64th round, schedule 10 (cols. 1-2), sch. 1, 10 and 25 (cols. 3-8). Estimation method: bias-corrected robust local regression point estimates and robust variance estimators, polynomial of order zero, triangular kernel, automatic bandwidth; implemented with the Stata procedure rdrobust. See Calonico et al. (2014). Rank (Dummy) is a dummy for the normalized rank being negative.

Table B.7: Agricultural Wage Rates, Occupational Pattern, Consumption and Poverty, Star States, Longitudinal Differences 2004-05 to 2007-08

Panel A

	(1)	(2)	(3)	(4)
	Agricultural Wages (log.)		Princ. Occ.: Ag. Laborer	
	Fall 2007	Spring 2008	Fall 2007	Spring 2008
Rank (Dummy)	-0.007	-0.110	0.008	-0.102
	(0.134)	(0.087)	(0.049)	(0.080)
Observations	22	20	23	23
R-squared	0.500	0.353	0.335	0.403
Unit-level obs.	1570	1195	6770	6180

Panel B

	(5)	(6)	(7)	(8)
	MPCE (log.)		Poverty HCR	
	Fall 2007	Spring 2008	Fall 2007	Spring 2008
Rank (Dummy)	-0.097	0.166**	0.077	-0.171**
	(0.060)	(0.065)	(0.059)	(0.075)
Observations	23	23	23	23
R-squared	0.356	0.395	0.371	0.354
Unit-level obs.	6770	6180	6770	6180

Notes: Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Additional control variables not reported: state fixed effects, lagged dependent variable (from 61st round) and normalized 2007 district rainfall. Dependent variables: district means of residuals from a unit-level data regression of logarithmic agricultural daily wages on sex and activity dummies, fully interacted, time difference (cols. 1-2); dummy for agricultural labor as a household's principal occupation, time difference of district means (cols. 3-4), MPCE (log), time difference of district means (cols. 5-6), dummy for MPCE below the poverty line, time difference of district means (cols. 7-8), Data source: NSS 61st and 64th round, schedule 10 (cols. 1-2), sch. 1, 10 and 25 (cols. 3-8). Estimation method: weighted least squares, triangular kernel, bandwidth (one-sided): 2.5.

Table B.8: Consumption and Poverty, Urban Households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	MPCE (logarithmic)				Poverty Headcount Ratio			
	Fall 2007		Spring 2008		Fall 2007		Spring 2008	
	Star States	Other States	Star States	Other States	Star States	Other States	Star States	Other States
Rank (Dummy)	-0.084	0.136	0.011	0.085	0.028	-0.042	-0.016	0.003
	(0.077)	(0.084)	(0.083)	(0.108)	(0.062)	(0.051)	(0.048)	(0.053)
Observations	23	30	23	30	23	30	23	30
R-squared	0.549	0.716	0.475	0.400	0.219	0.398	0.485	0.120
Unit-level obs.	1758	2612	1429	2919	1758	2612	1429	2919

Notes: Estimation sample contains only urban households; see Table 7.

Table B.9: Agricultural Wage Rates, Occupational Pattern, Consumption and Poverty, Star States in 2009-10

Panel A

	(1)	(2)	(3)	(4)
	Agricultural Wages (log.)		Princ. Occ.: Ag. Laborer	
	Fall 2007	Spring 2008	Fall 2007	Spring 2008
Rank (Dummy)	0.157	-0.041	0.012	-0.013
	(0.094)	(0.116)	(0.052)	(0.054)
Obs.	20	18	23	23
R-squared	0.653	0.514	0.747	0.852
Unit-level obs.	405	322	2600	2631

Panel B

	(5)	(6)	(7)	(8)
	MPCE (log.)		Poverty HCR	
	Fall 2007	Spring 2008	Fall 2007	Spring 2008
Rank (Dummy)	-0.045	0.045	0.018	-0.004
	(0.070)	(0.052)	(0.063)	(0.070)
Obs.	23	23	23	23
R-squared	0.792	0.784	0.729	0.732
Unit-level obs.	2600	2631	2600	2631

Notes: Data source: NSS 66th round (61st round for lagged values), schedules 1 and 10. Rank (Dummy) is a dummy for the normalized rank being negative. See Tables 6 and 7.

Table B.10: Descriptive Statistics of Additional Variables

	(1) All Major	(2) RDD Sample	(3)
	States	Star States	Other States
A. NSS Data (Schedule 10), Household Level			
Temporary Migrations (count; 365 day recall)	0.22	0.25	0.09
	(1.17)	(1.37)	(0.71)
Observations	64648	2995	3883
B. NSS Data (Schedule 10), Individual Level			
Children (5-12 years of age)			
School Days (one month recall)	25.34	26.92	26.63
	(11.29)	(9.64)	(9.99)
Work Days (one month recall)	1.00	0.96	0.86
	(5.35)	(5.22)	(5.02)
Observations	54667	2043	3047
Boys (5-12 years of age)			
School Days	25.74	26.92	27.05
	(10.92)	(9.60)	(9.49)
Work Days	0.65	0.64	0.44
	(4.31)	(4.15)	(3.60)
Observations	29260	1037	1616
Girls (5-12 years of age)			
School Days	24.87	26.92	26.16
	(11.69)	(9.69)	(10.50)
Work Days	1.40	1.30	1.33
	(6.31)	(6.12)	(6.22)
Observations	25407	1006	1431
Adolescents (13-18 years of age)			
School Days	17.74	16.81	19.24
	(14.94)	(15.11)	(14.63)
Work Days	10.57	10.97	9.43
	(14.18)	(14.29)	(13.83)
Observations	36517	1491	2365
Male Adolescents (13-18 years of age)			
School Days	19.06	18.08	19.99
	(14.63)	(14.93)	(14.40)
Work Days	8.37	9.02	7.86
	(13.14)	(13.53)	(12.96)
Productive Work (days)	7.68	8.42	7.26
	(12.69)	(13.08)	(12.49)
Domestic Work	0.59	0.47	0.55
	(4.02)	(3.39)	(3.78)
Observations	19795	789	1239
Female Adolescents (13-18 years of age)			
School Days	16.18	15.52	18.31
	(15.14)	(15.19)	(14.87)
Work Days	13.15	12.93	11.36
	(14.92)	(14.77)	(14.60)
Productive Work (days)	3.21	6.24	2.78
	(8.76)	(11.72)	(8.28)
Domestic Work	9.91	6.65	8.56
	(14.00)	(12.36)	(13.48)
Observations	16722	702	1126

Notes: Means, standard deviations in parentheses; see Table 2.