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The welfare effects of India's rural employment guarantee

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ABSTRACT

We assess 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' schooling. Our results imply substantial beneficial indirect effects of this large welfare program. We conclude that public employment programs hold significant potential for reducing poverty and insuring households against various adverse implications of seasonal income shortfalls — when properly implemented.

1. Introduction

Poverty around the globe is concentrated in rural areas. According to World Bank (2020), 80 percent of the world's population living on less than $1.90 a day reside in rural areas, while the rural population share stands at just 50 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, that is 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 are at risk of being captured by wealthy and politically well-connected households (Basu, 1991). An additional key challenge of programs which 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 have been 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 can decide flexibly 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 relatively most costly recent public employment program in low and middle-income countries, consuming two 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 every fourth rural household during the fiscal year 2013–14 (Desai et al., 2015).

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[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% went into a food-for-work component (Park et al., 2002).

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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 in 2006, the NREGA guarantees one hundred person days of employment to every rural household whose adult members are willing to perform unskilled manual labor at a statutory minimum wage. As for 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, 2003).

In contrast to other public employment programs in the global South, the NREGA has attracted a great deal of attention by academic economists. To name just a few studies of the early NREGA, outcomes that have been studied are incomes (Imbert and Papp, 2015), wages (Berg et al., 2018; Merfeld, 2019; Zimmermann, 2018), migration (Imbert and Papp, 2019), consumption (Bose, 2017; Deininger and Liu, 2019; Ravi and Engler, 2015), agricultural decisions (Gehrke, 2019), schooling and child labor (Afridi et al., 2016; Mani et al., 2020; Shah and Steinberg, 2021), and violent conflict (Fetzer, 2020; 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), a lack of sound empirical identification of causal effects of the NREGA has been lamented (Muralidharan et al., 2017; Sukhtankar, 2016). In this paper we revisit the rollout of the NREGA and present a novel approach to estimating its effects. We exploit the rules by which different sets of districts were allocated to the different rollout phases to unmask quasi-experimental variation in districts' program status. 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. We demonstrate that excluding these priority districts generates an almost-sharp state-wise regression discontinuity design (RDD) 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, which 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 that a mentionable amount of workfare employment was generated in only six of these states ('star states') and that this employment was almost exclusively concentrated on 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 seven percent of the national poverty line and no leakage of NREGA wage funds in the star states during the agricultural lean season in spring. In contrast, there is substantial leakage and 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 twice 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.

To assess whether workfare can also mitigate the intergenerational transmission of poverty, we also consider schooling and child labor as outcome variables. We find seasonal increases in schooling and decreases in adolescent labor in the star states, implying that the positive welfare effects of the employment program also include gains in schooling.

Our results illustrate that workfare programs in developing countries can reduce poverty and households' dependence on agriculture, and insure households against various adverse consequences of seasonal income shortfalls. 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 actual implementation intensity across states and season demonstrate, however, that a sufficiently intense and effective implementation is crucial. Our findings also imply sizable indirect effects of the employment program, which are as large as the direct income gains, even in the relatively short run. Due to data limitations, however, we cannot fully make precise the channels through which these effects accrue. For example, we find no significant evidence for short-term agricultural wage increases or decreases in seasonal migration.

The pattern of our results is consistent with Muralidharan et al. (2016), who study income and wage effects of the addition of biometric smartcards to the NREGA in one Indian state in 2011. They find substantial increases in household incomes that outmatch significantly, by a factor of ten, 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 findings make a strong case for big rather than piecemeal government interventions in the welfare policy arena. There are strong amplification effects but only when the program is implemented forcefully and effectively. Moreover, the positive spillovers on school attendance of adolescents suggest that there are additional long-lasting, intergenerational effects.

Our estimation procedure for the employment program's effects is distinguished by the following features. First, our identification approach is cross-sectional, which does not require the parallel trend assumptions essential for difference-in-differences (DID) analyses, on which most of the above-cited literature relies. In the context of the NREGA, parallel trends are a critical assumption given that this program was targeted at less developed districts in its early stages and so DID approaches are susceptible to confounding program effects with an accelerated secular convergence trend, which we think is difficult to rule out given India's aggregate growth rate of eight percent between 2005 and 2007 and the government's various efforts to mitigate regional disparities. Second, we take seriously the confounding of NREGA's effects with two other, similarly budgeted rural development programs rolled out in parallel. About 80 percent of early NREGA districts but none of NREGA's late districts have at least one of these programs. We show that our estimates are robust to the exclusion of such districts with other development programs rolled out in parallel. Third, we avoid the threats to identification of NREGA's effects posed by two earlier programs that were previously active in none of the late but in 60 percent of the early NREGA districts. Our cross-sectional approach 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 regarding 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 remainder of this paper is structured as follows. In Section 2, we shortly 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] 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).

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: first because of its rights-based approach and, second, 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 nature of assets created is varied and comprises roads, bridges, public and private irrigation facilities, improvement of marginal farm land, as well as construction of schools and health centers.

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).

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 Public Distribution System (PDS), on which the central government spent about one percent of GDP around the same time (World Bank, 2011). More details on the particulars of the NREGA can be found in the excellent survey article by Sukhtankar (2016).

2.2. Related literature

The NREGA has attracted a large number of writers. While much of this literature has been concerned with effects on wages and rural labor markets, we focus on the NREGA's welfare effects. We refer the interested reader to the surveys of Drèze and Khera (2017), Gehrke and Hartwig (2018) and Sukhtankar (2016) for findings regarding other outcomes.

Using national employment surveys and exploiting the program's rollout phase, Imbert and Papp (2015) find significant positive effects on rural households' earnings, which equal about two percent of average household consumption. The relative gains are much higher among households in the poorest two quintiles, however, with estimated gains of 5.8 and 2.7 percent. Using national consumption surveys and a similar identification strategy, Bose (2017) finds average consumption gains of around eight percent and increases of 12 percent for traditionally disadvantaged social groups.

Other previous studies of the NREGA's welfare effects have a narrower geographical scope. Applying structural estimation methods to primary panel data collected in one relatively poor state, Bihar, Murgai et al. (2015) project NREGA wage earnings to reduce poverty rates during the agricultural lean season by up to ten percent among participating households and by about three percent in the rural population as a whole. Like the studies employing national data, Deininger and Liu (2019) exploit NREGA's staggered, district-wise rollout in the south-eastern state of Andhra Pradesh. Applying difference-in-differences and matching methods to primary panel data, they find consumption gains of around four percent from program participation, which are concentrated among disadvantaged social groups, where they reach 10 percent. Ravi and Engler (2015) focus on ultra-poor households in one district of Andhra Pradesh and exploit the supply-side rationing of the program for empirical identification of the NREGA's welfare effects. They find consumption increases from program participation of around six percent and food consumption increases of ten percent. As both of the former two studies' focus is on the welfare effects from participation in the NREGA's public works, they do not report effects for the rural population at large.

Finally, through a large-scale randomized controlled trial carried out in Andhra Pradesh, Muralidharan et al. (2017) estimate income and poverty effects of introducing smartcards to the NREGA. They find a decrease in poverty of 26 percent, where the bulk of households' income gains comes from indirect, general equilibrium effects.

2.3. 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 Fig. 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.[3] 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 eastern states Bihar and Jharkhand, as well as in West Bengal.

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 the Online 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,[4] agrarian distress and low human development, all of which had been compiled earlier, during the first half of the 21st century's first decade, by India's Planning Commission. In accordance with Government of India (2007), 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 district backwardness ranking published in the 2003 Planning Commission report *Identification of Districts for Wage and Self Employment Programmes* (Government of India, 2003). With the exception of 36 metropolitan districts, this document ranks all but one district, 442 in total, in the 17 major states. 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, 2007), albeit not in a manner as stringent as the districts appearing on the three just-mentioned priority lists.

[3] 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.

[4] 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.

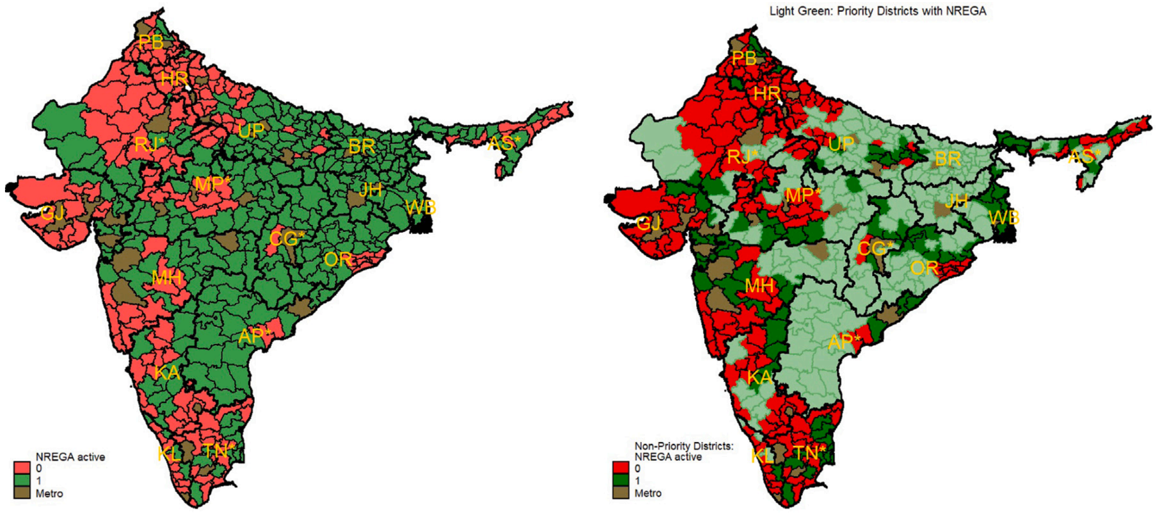

Fig. 1. Districts' NREGA and priority status in 2007–08, major states.

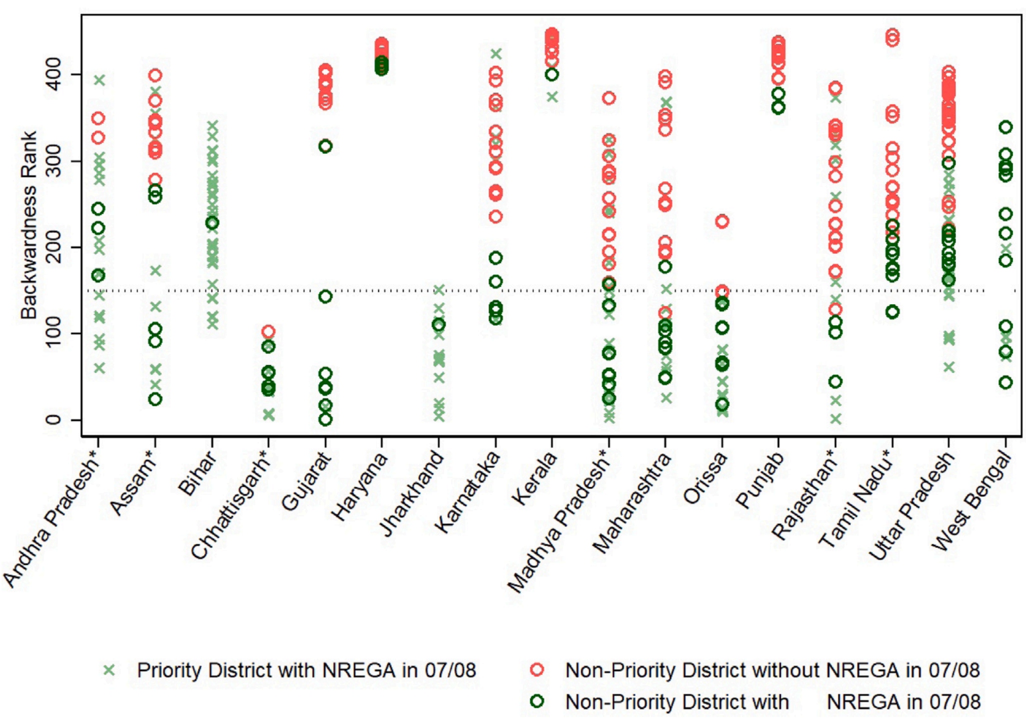

Fig. 2. District Backwardness Ranks and NREGA status in 07/08, by state. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Fig. 2 plots the backwardness ranks of all ranked districts state-wise, separately for 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 Fig. 2), even within states. To assess impacts of the NREGA in 2007–08, it is not obvious how to define 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.[5] 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 state. This is consistent with a press release of the Government of India (Government of India, 2007), according to which backward districts from Government of India (2003) were added to phase II after including priority districts (see Figure A2 in the Appendix).

As Fig. 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, 2007), 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 Government of India (2003) were selected, at least in most cases. This is the point of departure of our research design, which is following an RDD in

[5] 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.

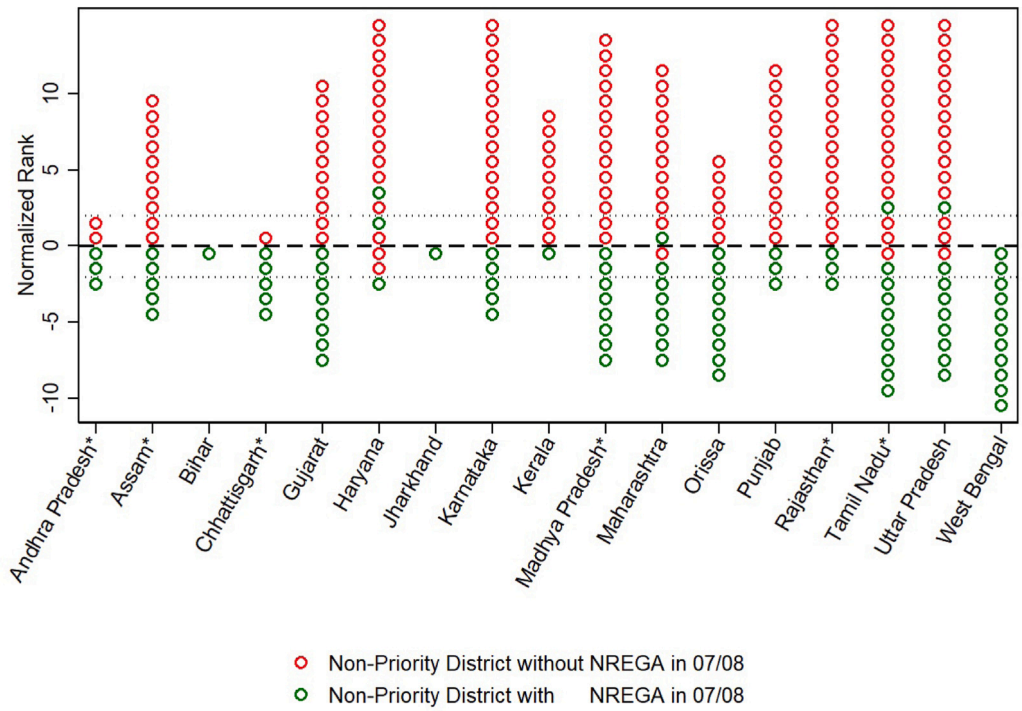

Fig. 3. RDD running variable and NREGA status in 2007–08, non-priority districts.

spirit. We exclude priority districts as well as metropolitan districts not included in the backwardness ranking, 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.

Fig. 3 plots the resulting *normalized rank* for non-priority districts, state by state. Evidently, ten of the fourteen 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 Figs. 1 to 5.

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 of the running variable around the threshold, which is illustrated in Fig. 4, as well as the fact that it only takes integer values distinguishes our scenario from a standard RDD, where continuity of 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, using the data driven-methods discussed in Calonico et al. (2014) is not feasible given the small number of realizations of the running variable. We desire as much similarity as possible regarding the backwardness ranks of districts within each state while achieving a sample size, that is 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, Fig. 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. We will return to this issue in the Robustness section.

Fig. 5 maps all districts from Fig. 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 nine of the ten states where our RDD is sharp,[6] precisely two of the four RD sample districts are covered by the NREGA in 2007–08 and two are not.[7]

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 which are not plagued by Maoist insurgence, excessive agrarian distress or especially low human development. Moreover, at least for the six states flagged with a star in Figs. 1 to 5, 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 is differentiated from the RDD approach in Khanna and Zimmermann (2017), which builds on the same district backwardness ranking, by the exclusion of priority districts, which results in less fuzziness of the RDD. Moreover, identification of causal treatment effects within our framework does not require that, conditional on backwardness ranks, levels of outcomes are the same in priority and non-priority districts absent the NREGA.

The main challenge facing our approach is the exogeneity of the state-wise RD thresholds. While obvious from Fig. 2, at least for most

[6] These ten states are Andhra Pradesh, Assam, Chhattisgarh, Gujarat, Karnataka, Kerala, Madhya Pradesh, Orissa, Punjab and Rajasthan.

[7] The exception is Chhattisgarh (CG) with only one non-metropolitan phase III district.

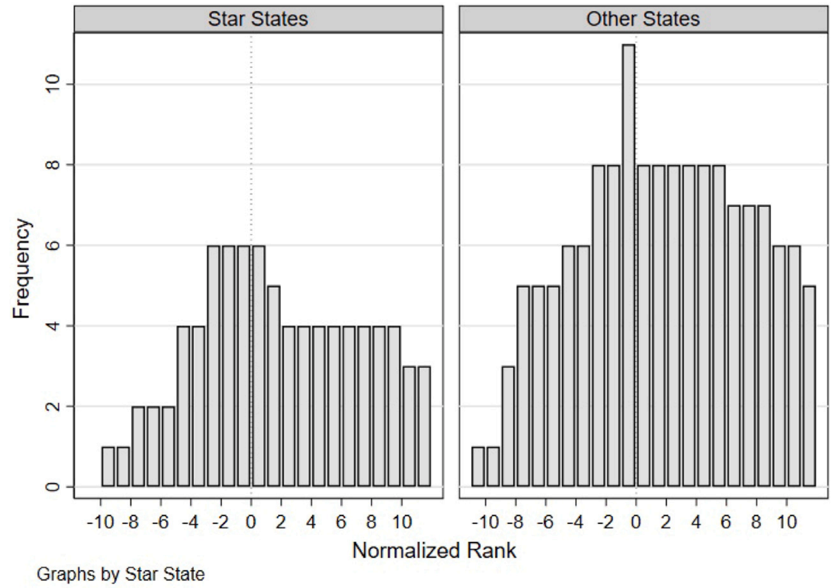

Fig. 4. Distribution of the normalized rank.

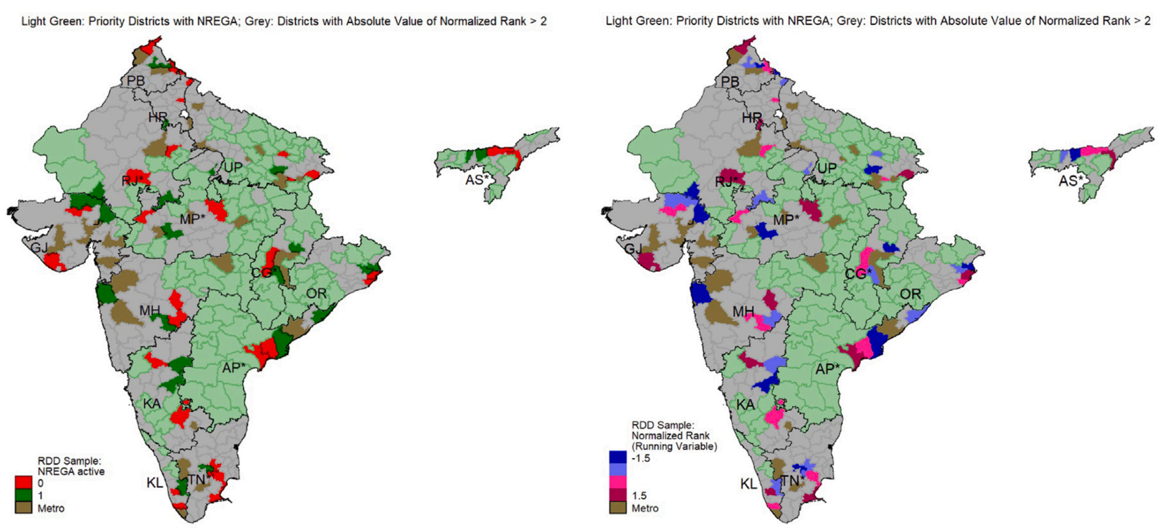

Fig. 5. RDD sample districts. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

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 maintained by the Ministry of Rural Development. For the major states with at least one phase III district, Fig. 6 depicts district means of NREGA wage expenditures per rural inhabitant during the agricultural year 2007–08.

Two facts stand out. First, there is ample variation across states. Second, wage expenditures follow a marked seasonal pattern. In all star states, expenditures are concentrated on 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 (USD 2.80 and 10.10 without and with purchasing-power parity adjustment) 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 empirical analysis, where the control group becomes 'contaminated' in the second quarter of 2008 in some of the states. We will revisit this issue 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 1999–2000 and 2009–10, respectively.

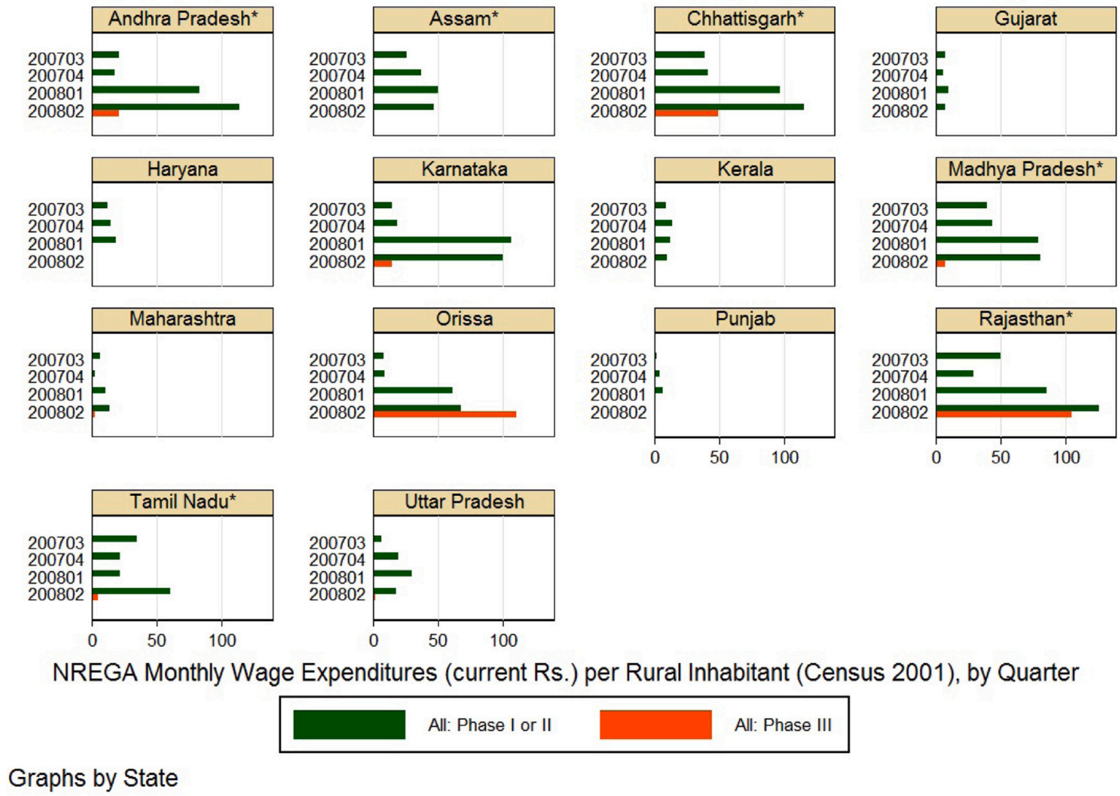

Fig. 6. NREGA Wage Expenditures in 2007-08.
Source: Ministry of Rural Development, Administrative Records.

For calculating representative district averages, we use the sampling weights provided with the NSS data.[8] 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 (2007–08) 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 nineteen and five expenditure categories, respectively. We include these latter two data sources for the following reasons. First, the 64th round employment and education expenditure surveys contain large numbers of observations, 125,578 and 100,581 households 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 (2007–08) round is identical with the one used in schedules 1 (consumption) and 10 (employment) 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 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.

The data scenario regarding labor market outcomes is less favorable, in contrast, as such data are available only in schedule 10 (employment) of the 64th NSS round (2007–08), which covers less than half of the households included in our estimations of consumption and poverty. In addition, the short recall period of only seven days (relative to 30 and 365 for high and low frequency consumption items) adds to an excessive variance for labor market outcomes, perhaps with the exception of school attendance (see Section 4.5). As a consequence, most of our labor market estimates have only a low precision and we therefore generally discount them relative to the results based on data from all three schedules of the NSS's 64th round. For the sake of completeness and comparability with earlier studies we include them nonetheless.

For the major states with at least one phase III district, Fig. 7 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, there is ample variation across states. For the 14 states in our research design, Gujarat, Karnataka, Orissa, Punjab and Utar Pradesh are distinguished by very low numbers while Andhra Pradesh, Chattisgarh, Madhya Pradesh, Rajasthan and Tamil Nadu stand out positively. Moreover, while the wages reported

[8] 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.

Table 1
Descriptive statistics.

	All Major	RDD sample	
	States (1)	Star States (2)	Other States (3)
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	65.62	71.55	70.03
Males	(29.45)	(31.22)	(35.42)
Observations	20359	855	980
Agricultural Wage Rate	47.93	48.62	51.31
Females	(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).

for Haryana and Kerala, two relatively affluent states, which also pay higher NREGA wages, are comparable to Assam, much fewer work days in public works are reported by survey respondents for the former two. This is in accordance with the figures in Drèze and Oldiges (2009), who report NREGA work days per household calculated from the same data source. For 2007–08 as a whole, these range between 2 in Punjab and 68 in Rajasthan. We choose to follow these authors and classify the six states with an NREGA activity level above the national average of 16 as high performers, and the remaining eight as low performers. This creates a natural partition of the set of 14 states in our sample as Drèze and Oldiges (2009) report 19 person days for the worst performing state above the national average, Andhra Pradesh, and merely 11 for the best performing one below the national average, Uttar Pradesh. Borrowing Imbert and Papp's (2015) term for NREGA high performers, we will refer to them as 'star states'.[9] 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. 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 8650 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 Fig. 6). A comparison of Figs. 6 and 7 reveals, however, that even in the star states wage incomes reported by survey respondents 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 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 eleventh among the fourteen states that we consider, while Assam is ranked fourth. Second, Kerala is an outlier regarding our RDD's marginal districts, which are both in the most affluent decile of the backwardness ranking

[9] With the sole exception of Assam, our choice of star states based on NREGA person days per household coincides with Imbert and Papp's (2015), who use the fraction of time spent on public works as indicator. We base our sample partition on Drèze and Oldiges' (2009) measure, person days per household, as we consider it the more welfare-relevant one.

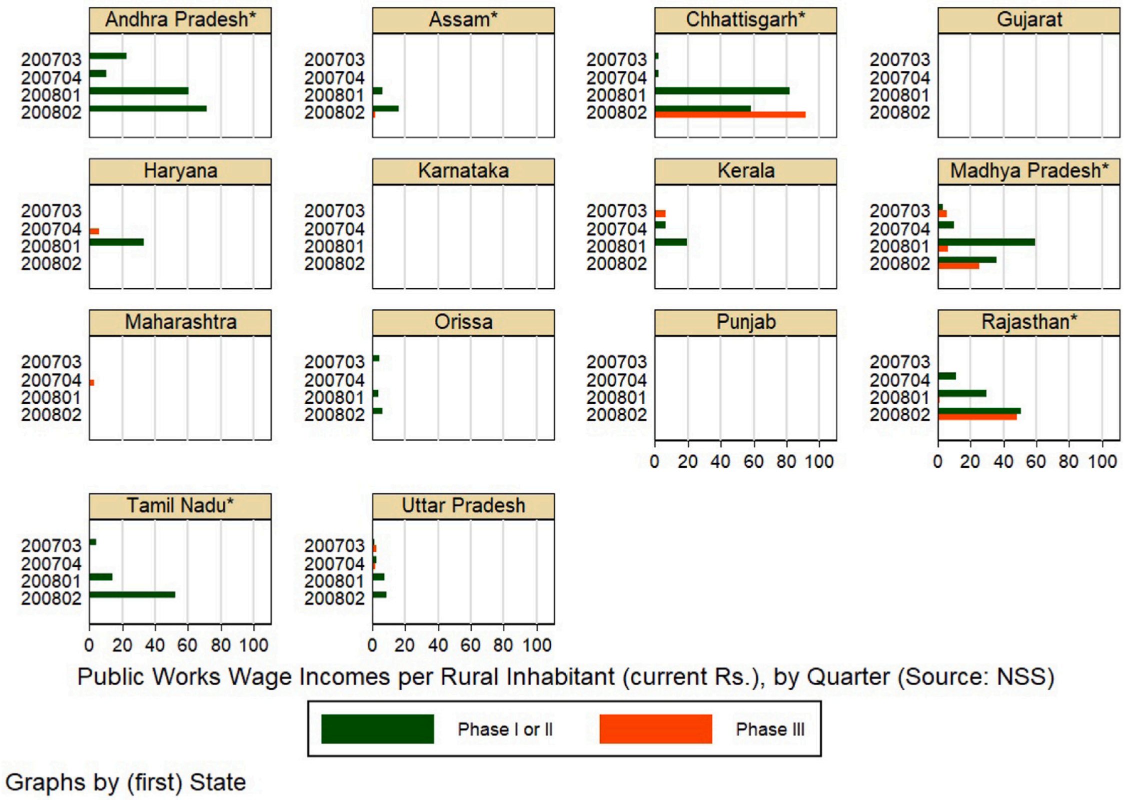

Fig. 7. Monthly NREGA Wage Incomes in 2007-08.
Source: NSS 64th Round, Schedule 10.

plotted in Fig. 2. This is also true for baseline poverty rates in Kerala's marginal districts, which are less than half the sample average in the other star states. Since our main interest is in the NREGA's effect on poverty reduction, including Kerala's districts is not meaningful.[10]

According to Fig. 7, households in phase III districts of Madhya Pradesh, Chhattisgarh and Rajasthan report substantive NREGA wage incomes 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.[11]

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 seven to eight 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 obtains 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.[12] In contrast, the shares of agricultural laborers and disadvantaged social groups (scheduled castes and tribes) as well 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

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

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 Fig. 8 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 piece-wise constant regression function as given by the right hand side of (1) and using the specifications laid out in the previous paragraph. According to these plots and the estimation output in Table 4, columns 1 and 2, 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. The former figure implies that our RDD is almost sharp for the star states and hence ITT effects obtained from estimating (1) will

[10] With the exception of Assam, our classification of star states is congruent with Imbert and Papp (2015), who use one percent of adult work days in public works as cutoff.

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

[12] 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.

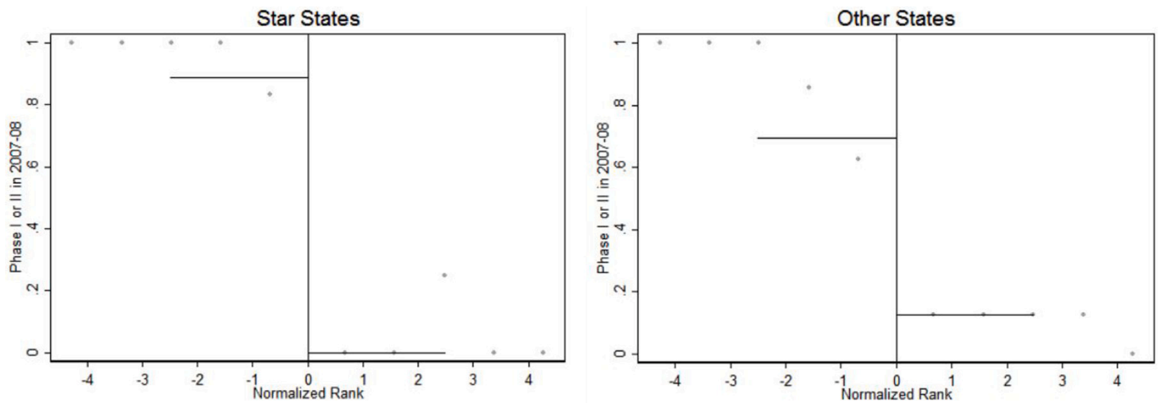

Fig. 8. Probability of being covered by the NREGA in 2007–08, non-priority districts in 14 major states.

be fairly tight lower bounds for average causal treatment effects of the NREGA, for districts close to the threshold.

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, that 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 which are in the vicinity of the RD threshold. Regarding this issue, our research design faces the challenge that there is no ex-ante specified threshold for 'treatment' within each state in Fig. 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.[13] Since we cannot fully ascertain 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 the RD threshold absent the NREGA. In particular, we conduct placebo estimations with lagged data and balancing tests with contemporary realizations of arguably unaffected covariates.[14]

We start out with level comparisons in different sets of districts employing data from the consumption and employment schedules of the 55th (1999–00) and 61st (2004–05) NSS round. To assess how comparable treatment and control districts in our research design are relative to using all districts in India, we use pre-NREGA survey data and modify the regressor of interest in (1) to an indicator for phase I or II status.[15] We conduct these regressions for three sets of districts, all districts covered by the NSS in the seventeen major states (453 districts), all districts that are not on one of the three priority lists (261 districts; see Figure A3 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.[16] 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 a good third five years later.

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

The lower half of Table 2 contains results of analogous estimations for basic indicators of the agricultural labor market. For agricultural wage rates, according to columns 1, 2, 4 and 5, there are vast, statistically significant differences 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. A labor market indicator for which more data are available is the fraction of households that report agricultural labor as their principal occupation. According to the estimation results in Table 2, 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.[17]

[13] 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 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.

[14] McCrary tests for the continuity of the running variable's density at the threshold 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.

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

[16] 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.

[17] We also assess whether there have been parallel trends in the three sets of districts prior to the NREGA. The results are set out in Table A1

Table 2
Early and late NREGA districts before the NREGA: Consumption, poverty and agricultural wages.

Year:	1999/2000			2004/2005		
Estimation Sample:	All Districts (1)	Non-Priority Districts (2)	RD Sample Districts (3)	All Districts (4)	Non-Priority Districts (5)	RD Sample Districts (6)
Dependent Variable: MPCE (logarithmic)						
Phase I or II District (dummy)	−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						
Phase I or II District (dummy)	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: Principal Occupation Agricultural Labor						
Phase I or II District (dummy)	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 observations	138534	81858	19210	126986	75932	17073
Dependent Variable: Agricultural Wage Rate						
Phase I or II District (dummy)	−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 observations	35839	20187	4919	19481	11595	2903

Notes: Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
An observation is a district mean in a given year. All specifications include state fixed effects.
Data source: NSS 55th round (columns 1–3) and 61st round (columns 4–6), schedules 1 and 10, rural households in 17 major states.
For the agricultural wage rate the dependent variable is the residual from a regression of agricultural wages on state–year–season–gender–task dummies (fully interacted).

For the balancing tests, we estimate (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, which are set out in Table 3, 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 — in line with qualitative information supplied by one of the decision makers in that process. We therefore think that our approach – while imperfect – has some advantages relative to previous studies of the NREGA's effects based on the program's rollout.

4.3. Main results

Table 4 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 Fig. 6, 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 Fig. 6), there are only small differences during the fall of 2007, of Rs. 9 and 2 in star and other states, respectively.

Columns 7 to 10 contain estimates for public works wage incomes from the NSS employment survey. For the reasons mentioned in Section 3.2, they are rather imprecisely estimated. While Figs. 6 and 7 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 eight percent of the national (monthly) poverty line (see Table 1). Moreover, it almost equals the estimate obtained from the administrative data in column 5, implying only minimal leakage when the program is intensely implemented on the ground in an average 'ordinary' (as opposed to a priority) district in the star states.[18] 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 or ineffectively: in star states during the fall season and other states during both seasons.

While the treatment effect's magnitude for the star states in spring stands out, the income gain from public works in our sample districts is less than the average in all the star states' phase I and II districts taken together, where they amount to Rs. 49 per person and month.[19] The smaller wage increase of Rs. 31 in our estimation sample has three sources. First and foremost, according to the survey data, the NREGA has been implemented more vigorously in the star states' priority

of the Online Appendix. According to these results, the null hypothesis of parallel trends in early and late NREGA districts is clearly rejected for both welfare measures at the 5 percent significance level. For wages, despite the fact that the point estimates closely resemble the ones obtaining for the welfare measures, the hypothesis of parallel wage trends cannot be rejected for either subsample, because of lower estimation precision, which is due to the considerably smaller sample size: the numbers of unit-level observations for the two welfare measures are about five times as large as for agricultural wages. This failure to reject parallel wage trends pre NREGA mirrors the main finding of the placebo analyses of Berg et al. (2018) for agricultural wage rates and Imbert and Papp (2015) for wage incomes. In contrast, the parallel trend assumption passes muster for our RD sample for all four dependent variables.

[18] 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 eight percent — because they disregard the district populations' growth between 2001 and 2008.

[19] Given an average daily wage rate on public work sites of Rs. 73 in this sample, this corresponds to 3.6 person days per household and month, a figure which is consistent with the ones reported in Drèze and Oldiges (2009). For the star states in our sample, they report a median activity of 34 person days per rural household for the year 2007–08 as a whole.

Table 3
RDD balancing tests.

	Household size (1)	SC/ST (fraction) (2)	Landholdings (size class) (3)	Rural population (millions) (4)	Rainfall (dev. from average) (5)
Normalized rank negative (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 observations	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.

Table 4
NREGA status and public works wage income per rural inhabitant.

Dependent Variable:	Program District		Public Works Wages (monthly, current Rs. per rural inhabitant)							
Data Source:			Wage Expenditures *Gov. of India, Admin. Records*				Wage Earnings *National Sample Survey, Sch. 10*			
			Fall 2007		Spring 2008		Fall 2007		Spring 2008	
Estimation Sample:	Star States (1)	Other States (2)	Star States (3)	Other States (4)	Star States (5)	Other States (6)	Star States (7)	Other States (8)	Star States (9)	Other States (10)
Normalized rank negative (dummy)	0.885***	0.573***	9.48***	1.99	34.07***	13.28*	−0.14	0.00	31.37*	3.03
	(0.102)	(0.167)	(2.39)	(1.30)	(7.33)	(7.06)	(0.12)	(0.00)	(17.51)	(2.82)
Observations	23	31	138	168	105	168	23	31	23	31
R-squared	0.86	0.42	0.35	0.89	0.49	0.16	0.35	1.00	0.48	0.16
Unit-level observations							6332	9115	4832	9134

Notes: Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Standard errors are clustered at the district level in columns 3-6. State fixed effects are included in all specifications.
Estimation method: weighted least squares, triangular kernel, bandwidth (one-sided): 2.5.
An observation is a district in columns 1, 2 and 7–10, and a district in a month in columns 3–6.
A unit-level observation is an individual in columns 7–10.
(Unit-level) observations from Chhattisgarh, Madhya Pradesh and Rajasthan during April–June, 2008 are excluded in columns 3–10.

Table 5
Agricultural wage rates and occupational pattern.

Dependent Variable:	Agricultural Wages (logarithmic)				Princ. Occ.: Agricultural Laborer			
	Fall 2007		Spring 2008		Fall 2007		Spring 2008	
Estimation Sample:	Star States (1)	Other States (2)	Star States (3)	Other States (4)	Star States (5)	Other States (6)	Star States (7)	Other States (8)
Normalized rank negative (dummy)	−0.006	−0.048	−0.001	−0.116	−0.012	0.069	−0.137***	−0.025
	(0.050)	(0.082)	(0.093)	(0.087)	(0.049)	(0.047)	(0.042)	(0.047)
Observations	23	29	21	28	23	30	23	30
R-squared	0.521	0.298	0.386	0.430	0.740	0.581	0.819	0.647
Unit-level observations	819	812	669	825	3,298	4,109	2,704	4,088

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 columns 1–4: residuals from a unit-level data regression of logarithmic agricultural daily wages on sex and activity dummies (fully interacted). Data source columns 1–4: NSS 64th round, schedule 10.
Data source columns 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 columns 1–4 and a household in columns 5–8.

districts, where wage gains amount to Rs. 56, compared to Rs. 39 in non-priority districts. Second, for districts around the threshold, the gains are a little smaller, Rs. 35. Finally, because we use an intent-to-treat approach and the predicted treatment status differs from the actual one for one district in Tamil Nadu, there is a further reduction of Rs. 4. In sum, our research design leverages considerably less than the average wage gains generated by the NREGA in the star states, which implies that the treatment effects which we are estimating in the sequel can be seen as lower bounds to the NREGA's average effects in the star states during the spring of 2008 — other things equal.

Table 5 contains results for the two labor market outcomes that we consider. According to columns 1–4, there is no instant effect of the NREGA on agricultural wages. While the signs are negative throughout, the point estimates are mostly small in magnitude and little precise. In addition to the reasons given in Section 3.2, the lack of precision for this outcome is exacerbated by the fact that agricultural labor market activities are reported by only a subset of rural respondents.

In contrast, for the dependent variable in columns 5 through 8 (see also Fig. 9), agricultural labor as the household's principal activity, there are about four times as many unit-level observations and the estimation precision is correspondingly higher. With a sample mean

Fig. 9. Agricultural Labor as Household's Principal Occupation. *RDD plots by season and intensity of program implementation.*

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 6 contains results for the two welfare measures, consumption and poverty. RDD plots that correspond to the eight columns of this table are displayed in Figs. 10 and 11. 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. Evaluated at the sample mean, this point estimate implies average consumption gains of around Rs. 80, which compares to mean consumption expenditures of Rs. 627 (USD 52.69, purchasing-power parity adjusted) in the threshold districts. 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 1). 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 the same 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 Fig. 6, which average at around Rs. 80.

The seasonal and regional pattern as well as the order of magnitude of the welfare effects that we estimate are broadly in accordance with the results of Imbert and Papp (2015), who find economically and statistically significant increases in daily casual earnings, of about nine percent, only for districts in star states during the spring season. A qualitatively similar, albeit less pronounced seasonal pattern is also reported by Bose (2017), who uses national consumption survey data and difference-in-differences estimation methods, with gains in fall and spring of 7 and 11 percent, respectively. She does not differentiate states by their effectiveness of NREGA implementation, however. The order of magnitude of her average effect, across seasons and states, of around eight percent, is about twice as large as the average effect implied by columns 1 through 4 of Table 6. Our zero results for other (non-star) states do not support the findings of Murgai et al. (2015), who use structural methods to elicit program effects at a time when the program has already been universal, in 2009 and 2010. They report a poverty-reducing effect of NREGA employment during spring of about three percent for Bihar, which has been one of the worst implementers of the program.

As in Muralidharan et al. (2017), the magnitude of our welfare estimates suggests indirect effects that are of the same order as the direct income effects reported in Table 4: evaluated at the sample mean, the estimate in column 3 of Table 6 implies an increase in per capita consumption of around Rs. 80, which compares to direct income gains from public works of merely Rs. 32 (column 9 of Table 4). While we are not able to identify the nature of these indirect effects within our research design, spillovers on agricultural wage rates (as in Berg et al., 2018), other employment (Muralidharan et al., 2017), shifts away from little gainful non-agricultural self-employment (Merfeld, 2020), more risk taking by marginal farmers (Gehrke, 2019), reductions in savings or expansions in borrowings (Kaboski and Townsend, 2012), or additional productivity of the assets created by the public works (Gehrke and Hartwig, 2018) are possible channels that have been suggested by previous writers.

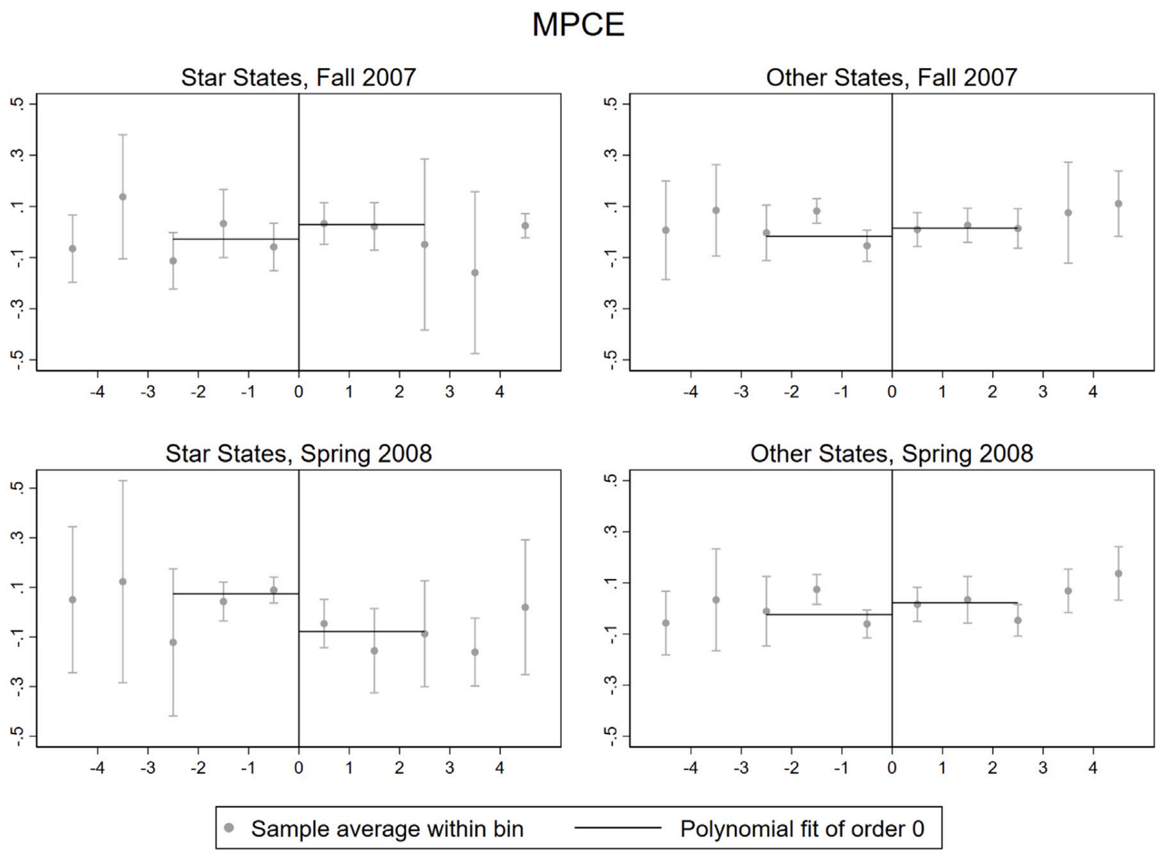

Fig. 10. Per Capita Consumption Expenditures (logarithmic). *RDD plots by season and intensity of program implementation.*

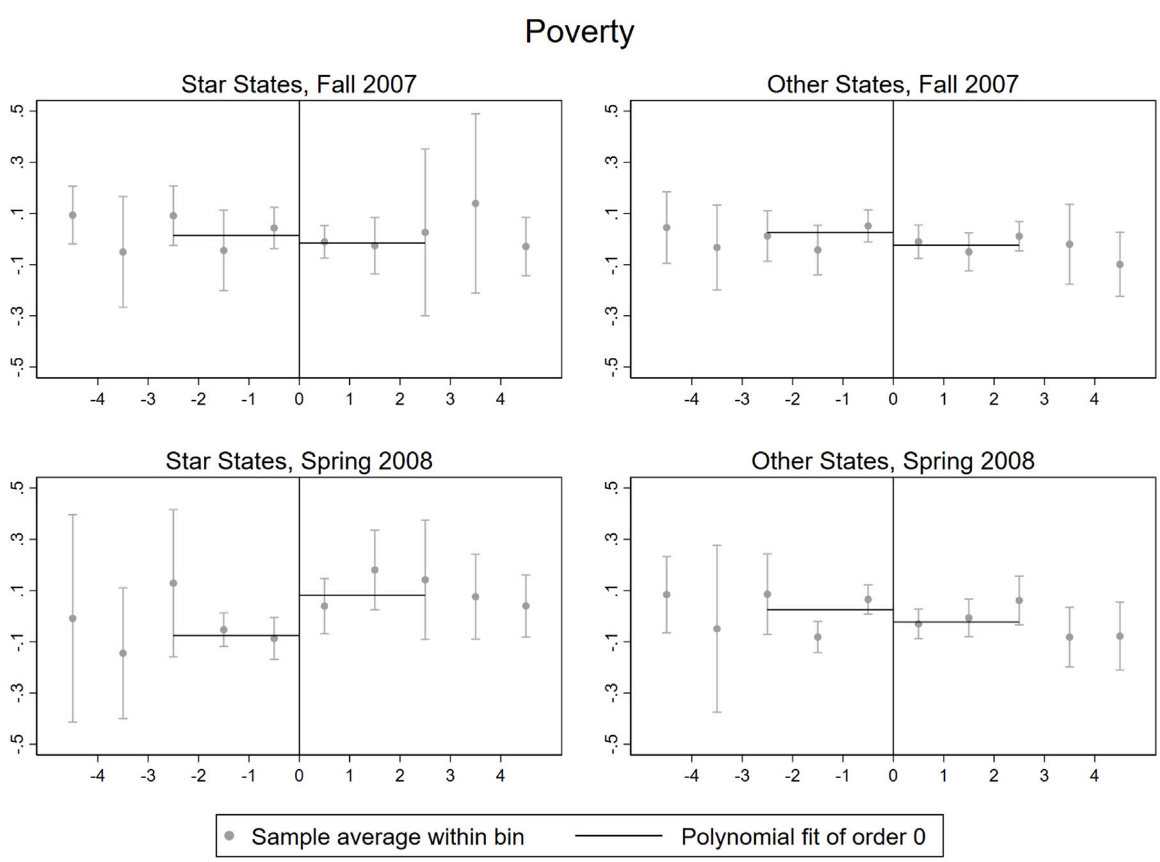

Fig. 11. Poverty Headcount Ratio. *RDD plots by season and intensity of program implementation.*

Table 6
Consumption and poverty.

Dependent Variable:	MPCE (logarithmic)				Poverty Headcount Ratio			
	Fall 2007		Spring 2008		Fall 2007		Spring 2008	
Estimation Sample:	Star States (1)	Other States (2)	Star States (3)	Other States (4)	Star States (5)	Other States (6)	Star States (7)	Other States (8)
Normalized rank negative (dummy)	−0.066	−0.033	0.160**	−0.050	0.035	0.050	−0.163**	0.052
	(0.068)	(0.049)	(0.054)	(0.048)	(0.062)	(0.050)	(0.061)	(0.047)
Observations	23	30	23	30	23	30	23	30
R-squared	0.675	0.907	0.774	0.893	0.627	0.749	0.614	0.815
Unit-level observations	3,298	4,109	2,704	4,088	3,298	4,109	2,704	4,088

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.
A unit-level observation is a household.
Unit-level observations from Chhattisgarh, Madhya Pradesh and Rajasthan during April–June, 2008 are excluded.

4.4. Robustness

To assess the internal validity and robustness of our main results, we conduct several validity and robustness checks. Appendix Tables A2 and A3 contain results of placebo experiments for labor and welfare outcomes for the 'thick' NSS round preceding the 64th (2007–08), the 61st fielded in 2004–05. 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 A3. Relative to columns 3 and 7 of Tables 5 and 6, 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.

A further challenge are other major welfare programs which were rolled out in 2006 and 2007, concurrent to the NREGA: first, 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); second, the Prime Minister's Rehabilitation Package for Farmers in Suicide Prone Districts with a central budget allocation of around Rs. 1000 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 Backward Regions Grant Fund 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 A5. 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 6 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 on 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 A6. 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 regarding consumption and occupational choice.

In a fifth robustness check, we use the bias-corrected regression discontinuity estimator derived by Calonico et al. (2014). A potential advantage of this estimation procedure relative to the one used in our main estimations is its superior performance in a recent study that compares several RD estimators to experimental estimates in the context of close elections (Hyytinen et al., 2018). Its disadvantage in our context is the need for larger bandwidths as the bias correction is obtained from fitting higher-order polynomials on both sides of the threshold. We use, as in our main specifications, local constant regression with a point estimation bandwidth of 2.5 and specify a bias correction bandwidth of 5. Results for the star states are set out in Table A7. They are virtually identical with the ones reported in Table 6.

In a sixth robustness check, we use randomization inference (Cattaneo et al., 2015) instead of the local polynomial RD estimator. An advantage of this estimation procedure in our context is that it allows a data-driven bandwidth choice even when there is only a small number of realizations of the running variable. Its disadvantage is that the bandwidth choice crucially depends on the covariates that are fed into the bandwidth search algorithm. Table A8 reports results from local randomization with a fixed bandwidth of 2.5 and Table A9 with an automatic bandwidth, where we use the same covariates that serve as controls in our main estimations for the bandwidth selection. With the fixed bandwidth, the consumption and poverty point estimates are almost identical with the ones in Table 6. The automatic bandwidths reported in Table A9 are, on the other hand, sometimes larger than 2.5 and the consumption and poverty estimates for the star states in spring smaller than in Table 6 by about one third. They remain significant at the 10 percent level nonetheless.

Our seventh robustness check uses time differences in district means between 2004–05 and 2007–08 as dependent variables. According to Table A10, 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).

In an eighth robustness check, we use the NSS's urban subsample. The welfare results are set out in Table A11. [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.

As a further robustness check, we estimate Eq. (1) with data from the 66th NSS round fielded in 2009–10, the second year in which the NREGA had been covering all of India's districts. As for the 61st (2007–08) NSS round, we use schedules 1 (consumption) and 10 (employment), which gives sample sizes similar to our main analyses. According to the results set out in Table A12 for the star states, there is a significant discontinuity for neither of the four outcomes that we consider — in accordance with the hypothesis 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.

Finally, we estimate the RDD directly from the unit-level observations rather than from district averages. As before, we give each district an identical weight, and we cluster standard errors at the district level. For inference, we employ the wild bootstrap Wu (1986) because the number of clusters is relatively small. The results for consumption and poverty are set out in Table A13. As expected, the point estimates virtually coincide with the ones in Table 6. And while the standard errors in Table A13 are about 20 percent smaller than in Table 6, the wild bootstrap inference turns out to be just a little more conservative than the classical one in Table 6. For reference, the p-values for columns 3 and 7 of Table 6 are 0.010 and 0.018, and 0.019 and 0.029 in Table A13.

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 which have received recent prominent attention (Sukhtankar, 2016), namely migration (Imbert and Papp, 2019), school attendance and child labor (Shah and Steinberg, 2021; Afridi et al., 2016). A limitation of our approach for studying these outcomes is that all three of them are recorded only in the employment survey (Schedule 10), which more than halves the number of unit-level observations relative to our welfare estimations, where we employ three schedules simultaneously. Another shortcoming of the NSS data regarding migration is that the employment schedule of the 64th round (2007–08) records only the number of individual temporary migrations during the 365 days preceding the interview. Hence, more than half of the recall period for households intensely exposed to the NREGA in spring 2008 covers a period with virtually no NREGA activity. In addition, no migration questions have been included in the 61st (2004–05) NSS round, so that no recent lagged values are available as a control.

Be that as it may, sample means for migration and regression results are set out in Tables A14 and 7, respectively. According to the former, rural households report 0.22 temporary migrations per year on average. The point estimates in Table 7 show a pattern which 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 estimated 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 dependent variable the number of days during which an educational institution was attended in the month preceding the interview.[21] According to Table A14, about 25 (17) days are spent in school by children (adolescents) while work activities are performed on one (10) day(s) on average. Boys and girls attend school similarly often, 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.

According to the results set out in Tables 8 and 9, the NREGA has a large and significant positive effect on adolescents' school attendance, which is also depicted in Fig. 12, 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 10 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. We have also explored whether these changes in adolescents' schooling are due to enrollment increases, the extensive margin, or more frequent attendance of enrolled teenagers, the intensive margin. While the NSS employment survey does not contain an explicit question on school enrollment, we find suggestive evidence for the latter, as the attendance of an educational institution as self-reported primary activity of adolescents also increases substantially and significantly in the star states in spring, by close to 20 percentage points. Given that adolescents are more productive in work activities than children, this pattern is consistent with the predictions of Jacoby and Skoufias' (1997) model. Put differently, through its insurance function 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 the smaller scale studies of Afridi et al. (2016) and Mani et al. (2020), who study six districts in one state around the NREGA's onset. Our findings somewhat contrast those of Shah and Steinberg (2021), who estimate schooling and child labor effects of the NREGA's onset with an identification strategy similar to Imbert and Papp's (2015) and Bose's (2017) empirical approaches. They do not differentiate by season or program implementation intensity and consider only school-aged children's self-reported primary activity — while we primarily consider the number of days during which school was attended or work performed. They find a four percentage point shift from schooling to work among adolescents and no effect for children, while we find no significant effects for the pooled

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

[21] The individual level data of the employment survey records activities during the seven days preceding the interview for all household members aged 5 years and older.

Table 7
Migration.

	Fall 2007		Spring 2008	
	Star States (1)	Other States (2)	Star States (3)	Other States (4)
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, triangular kernel, bandwidth (one-sided): 2.5.
A unit-level observation is a household.

Table 8
School attendance.

	Children (5–12 years)				Adolescents (13–18 years)			
	Fall 2007		Spring 2008		Fall 2007		Spring 2008	
	Star States (1)	Other States (2)	Star States (3)	Other States (4)	Star States (5)	Other States (6)	Star States (7)	Other States (8)
Normalized rank negative (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 observations	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.

Table 9
Child and adolescent labor.

	Children (5–12 years)				Adolescents (13–18 years)			
	Fall 2007		Spring 2008		Fall 2007		Spring 2008	
	Star States (1)	Other States (2)	Star States (3)	Other States (4)	Star States (5)	Other States (6)	Star States (7)	Other States (8)
Normalized rank negative (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 observations	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.

sample of adolescents (taking the average of columns 5 to 8 in Tables 8 and 9) and a shift from work to school in the star states during spring. Hence our findings, unlike Shah and Steinberg's (2021), imply no trade-off between greater off-season earning opportunities for adults and child welfare, at least when the program is vigorously implemented. A reason for the different findings in their and our paper could be dynamic complementarities in parental schooling investments (Foster and Gehrke, 2017). On the one hand, a half-hearted and little reliable public works program, which is probably a realistic average depiction of the early NREGA in India as a whole, might have increased opportunity costs of adolescents and led to less school attendance once a work site opened. On the other hand, expectations about future access to an effective and reliable public works program which serves as an income insurance mechanism, can result in instantaneous increases in schooling because parents feel confident that they will be able to afford complementary future investments in their adolescent children's education in subsequent years. The latter scenario is not unlikely for the star states in the spring of 2008.[22]

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 indirect and what are the 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

[22] We thank an anonymous referee of this paper for pointing out this possibility.

Table 10
Schooling and labor of adolescents, Star States, Spring 2008.

Estimation sample:	Male Adolescents (13–18 years)				Female Adolescents (13–18 years)			
Dependent Variable:	School	Work			School	Work		
	Attendance (1)	All (2)	Productive (3)	Domestic (4)	Attendance (5)	All (6)	Productive (7)	Domestic (8)
Normalized rank negative (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 observations	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.
Dependent variables: days attended educational institution (columns 1, 5), days with labor activity (columns 2, 6), days with productive labor activity (columns 3, 7), days with domestic chores (columns 4, 8) during the month preceding the interview.
Estimation method: weighted least squares, triangular kernel, bandwidth (one-sided): 2.5.

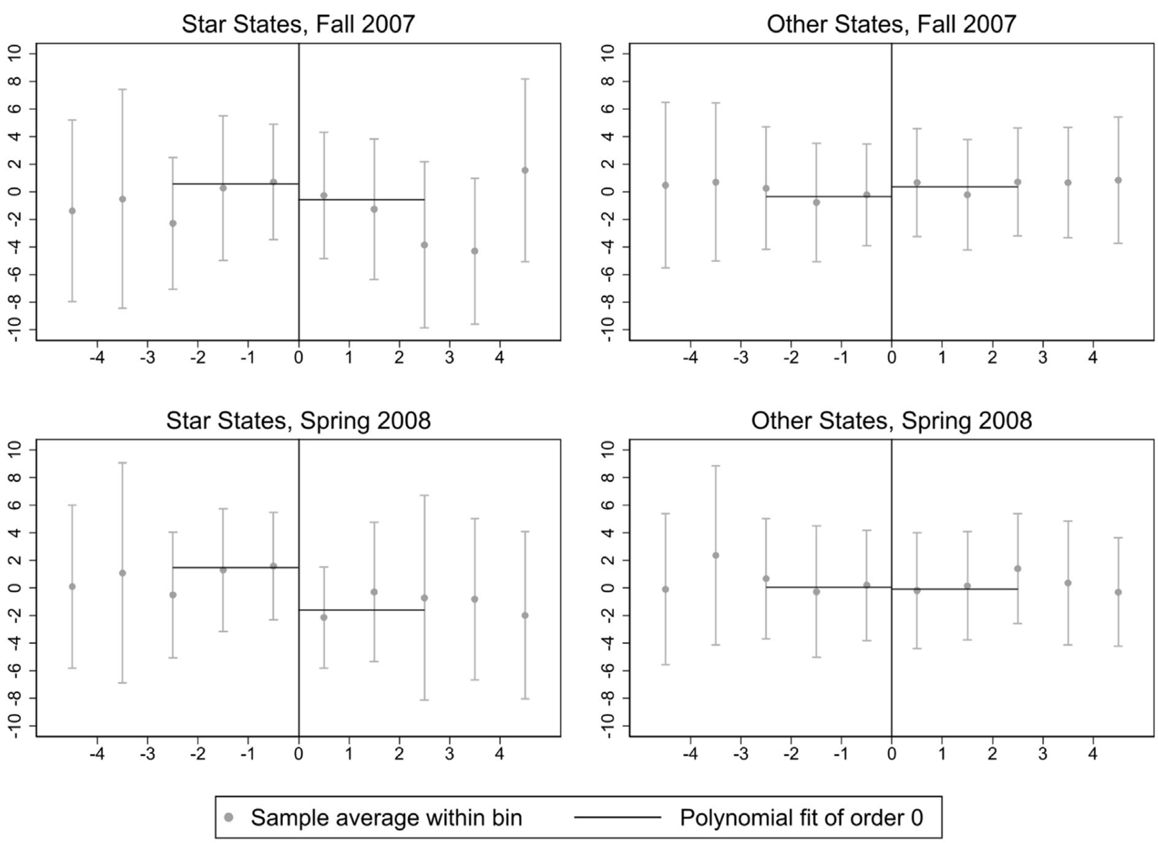

Fig. 12. Days in Educational Institution per Month, Adolescents. *RDD Plots by season and intensity of program implementation.*

rates or migration. 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 to robustly identify 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 and effectively. All effects are concentrated on the agricultural slack season when rural unemployment is high and the program most active. In contrast, we have found no program effects in states which implemented the program half-heartedly or where the program funds leaked.

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), lack of experimental variation, construct validity, the extent of effective NREGA presence in our context, as well as spillovers across district boundaries.

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 five 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 regards the scope of our analysis. Driven by the objective to identify causal program effects, our research design is focused on only a subgroup of 'ordinary' districts, ones that are not plagued by violent political extremism, excessive agrarian distress or dismal human development indicators.

We have documented that, when vigorously 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 indirect effects of the program, even in the short term, as in Muralidharan et al. (2016), who study the introduction of smartcards as an implementation improvement of the NREGA. Our heterogeneous findings for star versus other states, moreover, support the view that additional beneficial indirect effects only kick in when the program is implemented with sufficient intensity and effectiveness. 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.

CRediT authorship contribution statement

Stefan Klonner: Conceptualization, Methodology, Software, Validation, Formal analysis, Data curation, Writing – original draft, Writing – review & editing, Visualization, Supervision. **Christian Oldiges:** Conceptualization, Validation, Formal analysis, Data curation, Writing – original draft.

Data availability

Data will be made available on request.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jdeveco.2022.102848.

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