University of Heidelberg

Department of Economics

Discussion Paper Series | No. 564

Safety Net for India's Poor or Waste of Public Funds? Poverty and Welfare in the Wake of the World's Largest Job Guarantee Program

Stefan Klonner
and Christian Oldiges

May 2014

Safety Net for India's Poor or Waste of Public Funds? Poverty and Welfare in the Wake of the World's Largest Job Guarantee Program

Stefan Klonner* and Christian Oldiges†
South Asia Institute, Universität Heidelberg

May 2014

Abstract

This paper examines the effects of India's Mahatma Gandhi National Rural Employment Guarantee Act, currently the world's largest public employment program, on household consumption and poverty rates in rural India. Combining regionally coded data from consumption surveys with information on the district-wise rollout of the program, we employ a regression discontinuity design to estimate program effects during the years 2007 and 2008. We find large, season-specific effects among a traditionally deprived sub-group of the rural population, whose incomes are particularly dependent on agricultural wage labor. We find that for this group of households, which accounts for thirty percent of India's rural population, employment opportunities under the scheme have cut poverty during the agricultural lean season by as much as one half while we find no effect during the agricultural peak season. In a cost-benefit analysis we find that consumption increases among this group of households are of the same order of magnitude as the wage outlays of the program. We document that consumption among this group of households had previously exhibited severe systematic seasonal fluctuations and conclude that the employment program has had a lasting effect on consumption smoothing across agricultural seasons.

JEL Classifications: I38; J38; O15

Keywords: job guarantee, public employment programs, welfare programs, poverty, consumption smoothing, India

---

*Contact Information: klonner@uni-heidelberg.de
†Contact information: c.oldiges@uni-heidelberg.de

1 Introduction

Poverty around the globe is concentrated in rural areas. For 2002, Chen and Ravallion (2007) have estimated that more than two thirds of the 1.14 billion living on less than a dollar per day resided in rural areas while, at the same time, the rural population share figured at less than one third. Rural development and poverty alleviation programs have been and continue to be popular, in particular in low and middle-income countries. Well-known programs have involved cash-transfers, pensions, free or subsidized food provision including school feeding programs, subsidized credit and directed lending, asset creation, and various kinds of agricultural subsidies and extension work (Basu, 1991). In addition to bringing down poverty figures, the declared purpose of most of these programs is to help poor rural households to cope with various forms of risk (Lal et al., 2010).

A fundamental problem of all such programs is targeting, that is reaching out to the most needy (Besley and Coate, 1992). When benefits come at no cost for the recipients and administrative capacities for ensuring proper targeting are limited, the benefits from welfare programs have often been found to be captured by wealthy and politically well-connected households (Basu, 1991; Gaiha, 2000). 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 (Subbarao, 2003). According to the World Development Report 2014, in sub-Saharan Africa alone, around 150 public works programs are currently active, and Subbarao (2003) enumerates several large-scale public works programs in Asia and Latin America from the 1980s and 1990s. The effort involved in the physical labor has the potential to ensure proper targeting (Besley and Coate, 1992; Basu, 1991) and households can decide on a day-to-day basis whether to supply their labor and receive benefits. In addition, public works programs have the potential to build growth-enhancing local public goods (World Bank, 2013).

Ethiopia's Productive Safety Net Program 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, providing employment to fifteen percent of India's workforce. In 2012, it accrued a cost of close to $10 billion, about one percent of the country's GDP. 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.

Several recent papers have evaluated the Act's labor market effects on a national scale econometrically. Studies using National Sample Survey data on employment (Azam, 2012; Imbert and Papp, 2013; Zimmermann, 2012), as well as Berg et al. (2012), who use agricultural wage data from the Indian Ministry of Agriculture, find that the Act has resulted in increases in agricultural wages. Moreover, female workers and marginalized groups belonging to scheduled castes and scheduled tribes, formerly untouchables within the Hindu caste system, appear to be among the main beneficiaries of the Act.

While rural wages and rural consumption are likely positively correlated, particularly among India's rural poor (Lanjouw and Murgai, 2009; Berg et al., 2012), increases in agricultural wages are merely a second order, general equilibrium effect of a public employment program. In our view, the net welfare effects of this large employment program have received too little attention in comparison. In this paper, we set out to assess whether the NREGA has increased rural households' consumption, to what extent the Act has helped rural households to smooth consumption, and whether the program has been well-targeted as far as the distribution of welfare effects over the rural population is concerned.

We combine data from two waves of India's nationally representative National Sample Survey on household consumption with information on the district-wise rollout of the NREGA. We make use of the phase-wise rollout of the Act. The NREGA was implemented first in 200 districts in the fiscal year 2006-07 (Phase I), in another 130 districts in 2007-08 (Phase II), and in India's remaining 263 districts in 2008-09 (Phase III).[1] We construct a district pseudo panel with consumption and program coverage data for the agricultural years 2006-07 and 2007-08 to estimate program effects on rural households' consumption expenditures and consumption-based poverty measures. To deal with potential endogeneity in program placement, we employ a modification of Zimmermann's (2012) regression discontinuity approach. We use an official district backwardness index published by India's National Planning Commission in 2003, which has served as the basis for allocating districts to different phases of the program's rollout. In this process, the declared intention of policy makers has been to give more backward districts earlier access to the program. Following Zimmermann (2012), we predict a district's actual program status in 2007-08 by whether it is among the 130 most backward districts according to the Planning Commission's index, and regress the outcomes of interest on program status thus predicted. To be precise, we estimate local average treatment effects of the Act, where "local" pertains to the fact that our estimated effects are for districts that are the least backward among the 130 districts predicted to obtain the program in Phase II, or equivalently the most backward among the 263 districts predicted to obtain the program in Phase III.

To assess whether the Act has been well targeted, we study households belonging to scheduled castes and scheduled tribes (SC/STs), which account for 29.8 percent of India's rural population according to the Census 2011 (Government of India, 2011), in detail. In our sample, among scheduled castes and scheduled tribes, poverty is close to three times the figure for the non-SC/ST population. Further, we assess to what extent

[1] These numbers are based on the 2001 Census definition of districts(Government of India, 2001). By now, the Act is active in all 640 Census 2011 districts (Government of India, 2011).

NREGA employment has helped households to smooth consumption across agricultural seasons. Given that, at least in backward districts, consumption used to plummet during the agricultural slack season in spring, a particular focus of our analysis is on the Act's effect on seasonal consumption fluctuations.

Our results are as follows. For the sample of all rural households residing in NREGA Phase II and Phase III districts, we find a statistically significant effect on neither the average level of consumption nor consumption-based poverty measures. For the sub-sample of SC/ST households, in contrast, we find large effects on both average consumption and poverty for the agricultural slack season in spring while there are no statistically significant effects for the fall season. According to our point estimates, which are imprecisely measured, the Act has increased SC/ST consumption during the spring season by as much as 30 percent and halved poverty.

In addition to the econometric estimations, we also carry out a detailed descriptive analysis of seasonal consumption patterns with National Sample Survey data from 2003 to 2012. We document that, prior to 2007-08, SC/ST households in NREGA Phase II districts experienced far greater systematic consumption fluctuations between fall and spring seasons than in the generally better-off NREGA Phase III districts. From 2007-08 onward, in contrast, we find substantially smaller differences in seasonal consumption and poverty patterns across these two groups of districts. Combining these descriptive with the econometric results, our findings are suggestive of a scenario where the Act has reduced seasonal consumption fluctuations for SC/ST households in India's more backward districts in a sustained fashion by increasing spring consumption to levels close to those during the fall season.

We also conduct a rough cost-benefit analysis of the NREGA by combining our estimates with program expenditure data. According to NREGA expenditure figures, more than 80 percent of the program's wage expenditures in Phase II districts during the agricultural year 2007-08 occurred during the agricultural slack season, that is the spring of 2008, when NREGA wages paid to SC/ST employees amounted to about Rs. 60 per rural SC/ST individual. Per rural SC/ST individual, our most conservative point estimates predict an increase in monthly average individual consumption due to the NREGA of around Rs. 70. We conclude that the program's wage expenditures have been cost-effective in increasing slack-season consumption of SC/ST households, even if our point estimates of the program's effect on consumption are overstated.

This paper contributes to a rapidly growing literature on welfare effects of rural anti-poverty and development programs. To name only a few examples, Djebbari and Smith (2008), among many others, study welfare effects of the Mexican PROGRESA conditional cash transfer program. Duflo (2003) studies the effect of old-age pensions on child nutrition in South Africa. Kochar (2005) and Tarozzi (2005) estimate nutritional effects of India's public food distribution system. Rural credit expansion and poverty in India and Bangladesh is the subject of Burgess et al. (2005) and Pitt and Khandker (1998). Moyo et al. (2007) analyze the effect of agricultural extension on poverty in Uganda. Regarding public works programs prior to the NREGA, most existing econometric studies focus on targeting rather than welfare and poverty (Jayne et al., 2002). An exception is Datt and Ravallion (1994), who find a moderate poverty-reducing effect of the Maharashtra Employment Guarantee Scheme, a predecessor to the NREGA active in only one of India's states. Regarding the NREGA, most existing empirical research by economists is on labor market rather than welfare effects (see the citations above). Exceptions are Afridi et al. (2012), who find a positive effect on child schooling in data from six districts in the state of Andhra Pradesh. Scandizzo et al. (2009) find that the NREGA smooths household income in two villages in the state of Maharashtra. Under the assumption that the manual labor required to receive NREGA benefits is a burden for program participants, Lagrange and Ravallion (2012) propose to correct welfare and poverty effects by the disutility from working on NREGA sites and illustrate this conceptual approach with cross-sectional National Sample Survey data from the state of Bihar.

In terms of the study object, welfare effects of the NREGA, the following three papers are closest to ours. Ravi and Engler (2009) use a small panel data set of 320 households residing in the state of Andhra Pradesh and find that consumption expenditures increase by about ten percent in response to the Act. Their program effect estimates are based on propensity score matching and, in our view, rely on rather strong identifying assumptions. Deininger and Liu (2013) use a panel of 4,000 households residing in the same state. With three waves of data from 2004, 2006 and 2008, they perform double and triple differences estimations and propensity score matching. Similar to our empirical results, they find that the program was well targeted and had large effects on food consumption and asset accumulation, particularly among SC/STs and casual laborers, whose magnitudes exceed the value of direct transfers. Our analysis differs from these studies in four regards. First, in terms of scope, we consider all major Indian states. Second, our empirical identification strategy does not rely on parallel trend assumptions, which we find not to hold in various placebo estimations. Third, we consider effects not only on consumption averages, but also on consumption-based poverty. Fourth, and most importantly, we unfold the seasonal pattern of program effects and show how the NREGA has not only reduced poverty levels but contributed to consumption smoothing. Bose (2013) uses two waves of Indian National Sample Survey data to estimate the effect of the first phase of the NREGA on consumption and poverty. Employing a differences-in-differences estimation technique with Phase I as treatment and Phase III as control group, which requires strong identifying assumptions, her estimated program effects are similar to ours.

The structure of this paper is as follows. In Section 2, we discuss the NREGA in some detail and present the data used in our analyses. We introduce our empirical approach and identification strategy in Section 3. Section 4 contains the empirical results, Section 5 various robustness checks and extensions. A cost-benefit analysis is the subject of Section 6. The final section concludes.

2 Background and Data

2.1 The National Rural Employment Guarantee Act

The NREGA, enacted in 2005 by the United Progressive Alliance government, was envisioned as a safety net for rural households. Under the Act 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 guarantees employment within 14 days to any rural resident who is willing to work, irrespective of income level, gender, caste, or religion. The Act includes a provision for an unemployment allowance in case of failure to provide work within this time frame. 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). As to the first, the NREGA marks a move away from doling out benefits to recognizing certain basic entitlements, including the notion of a right to work and to a minimum income. The NREGA 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 civil society, which in turn has helped to expose several instances of corruption (Vanaik and Siddhartha, 2008a,b).

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. The Maharashtra Employment Guarantee Scheme, enacted in 1977 and active until the inception of the NREGA, has received some interest by researchers in the past (Basu, 1981; Drèze, 1990; Ravallion et al., 1993).

The NREGA started in 200 districts, which we will refer to as Phase I districts, in the fiscal year spanning from April 2006 to March 2007. In April 2007, another 130 districts started implementing the Act (Phase II), and in April 2008 all remaining 263 districts were covered (Phase III). The spatial pattern of districts' allocation to the three phases is mapped in Figure 1. We identify Phase II and Phase III districts as published on the official website of the Ministry of Rural Development (Government of India, 2013c). From the same source we collected year, district, and month-wise program intensity data. In our subsequent analysis, where we approach the NREGA rollout as a natural experiment, we focus on the fiscal year 2007-08 and regard Phase II districts as treatment group and Phase III districts as control group. We disregard Phase I of the NREGA for two reasons. First, in the 200 Phase I districts, the NFFWP had been operating up to the initiation of the NREGA making it difficult to separate effects of the NREGA from those of the NFFWP. Second, unlike for Phase II and III districts, we are not aware of a convincing empirical strategy addressing the problem of selection of districts into this Phase.[2]

[2] See, however, Bose (2013) for a comparative analysis of Phase I and III districts in the fiscal year 2006-07.

Planning Commission Backwardness Index

In the subsequent analyses we employ a district-wise backwardness index published by India's National Planning Commission (Government of India, 2003). For 447 districts in India's major states, this index is calculated from three sub-indices, percentage of SC/ST population, agricultural output per worker, and the agricultural wage rate. The final composite index figures between 0.078 (most backward) to 2.159 (least backward). This index has served as the basis for allocating districts to each of the three phases of the NREGA (Zimmermann, 2012). In our empirical analysis we use this index for dealing with selection problems in district-wise program status assignment. Unfortunately, the index is available for only 92 and 163 of the NREGA's 130 Phase II and 263 Phase III districts, respectively. All districts listed by the Planning Commission belong to the seventeen major Indian states of Andhra Pradesh, Assam, Bihar, Chhattisgarh, Gujarat, Haryana, Jharkhand, Karnataka, Kerala, Madhya Pradesh, Maharasthra, Orissa, Punjab, Rajasthan, Tamil Nadu, Uttar Pradesh, and West Bengal. As our identification strategy can only accommodate districts for which the Planning Commission's backwardness index is available, our subsequent analysis is restricted to those 255 Phase II and III districts for which the backwardness index is available.

Descriptive Statistics

Table 1 presents key program statistics for our sample of 92 Phase II districts during the fiscal year 2007-08.[3] According to Table 1, seventy percent of the program's expenditures of about Rs. 30 billion were spent on wages. Given a population of 25.5 million households, this amounts to Rs. 840 per rural household residing in these districts. Employment in NREGA works and thus NREGA expenditures follow a marked seasonal pattern. They peak during the dry spring season when labor demand in rural areas plummets. To illustrate, Figure 2 depicts NREGA wage expenditures per rural inhabitant (not per NREGA worker) in our sample districts by month. Accordingly, wage expenditures per rural inhabitant stood at less than Rs. 10 per month during the first six months of Phase II for which program expenditure data is available (May to October 2007). This figure more than tripled to about Rs. 30 per month during the agricultural off-season, the first half-year of 2008. During the same period, monthly wage expenditures amounted to Rs. 55 per capita among SC/ST households. The same figure also demonstrates that this cyclical expenditure pattern continues into the fiscal year 2008-09.

2.2 Household Welfare

In our main empirical analysis, we use the 63rd and 64th round of the Indian National Sample Survey's (NSS) consumption expenditure module. These two rounds cover the

[3] For a discussion of the quality of official NREGA program data see Drèze and Oldiges (2011).

agricultural years July 2006 to June 2007 and July 2007 to June 2008. Our reason for this choice of rounds is as follows. For a differences-in-differences estimation of the program effect of NREGA's Phase II with one baseline and one endline wave of data, we are bound to use the 64th round as endline since this is the only agricultural year in which the Act was active in all Phase II districts but in none of the Phase III districts.[4] The natural choice for the baseline is the 63rd round canvassed in 2006-07. In comparison to prior rounds, such as the large 61st or the slightly smaller 62nd round, using a baseline as close to the endline as possible minimizes the effect of confounding factors, which we expect to be numerous given India's rapid rate of transformation during the 2000s. There is an additional reason in favor of the 63rd, and against the 61st round, which serves as baseline in Azam (2012) and Zimmermann (2012). The summer monsoon rainfall (June to September) of 2004 was more than fifteen percent below the long-term average for India as a whole resulting in a kharif (fall) crop failure (Government of India, 2012), while the monsoon rainfalls during the three following years were exceptionally similar with deviations from the long-term average of -1.3, -0.4 and +5.7 percent in 2005, 2006 and 2007, respectively (Government of India, 2014). Hence, as far as weather conditions are concerned, the three agricultural years covered by the 62nd, 63rd and 64th round are similar in terms of weather conditions, which is mirrored by growth rates of the agricultural gross domestic product of 5.1, 4.2 and 5.8 percent, while there was zero growth in 2004-05 (Government of India, 2012). We will revisit the issue of alternative baselines when we address the robustness of our empirical results.

In all our analyses, a household is the unit of observation; we do not aggregate welfare outcomes at the district-level. Throughout, we use the sampling weights provided with the NSS data, which are meant to ensure that consumption aggregates calculated from the household-level data are representative for the rural population at the individual (not the household) level. While India's National Sample Survey Organization (NSSO) points out that consumption estimates are representative at the district level for neither the 63rd nor the 64th round because of a sample size which is small by NSS standards (Chaudhuri and Gupta, 2009), we shall point out here that random sampling within each district is sufficient for consistent estimation of program effects within our empirical approach. The smaller numbers of observations in these two "thin" rounds (on average 14,000 households rather than 32,500 in the "thick" 61st round) will merely reduce the estimation precision.

Our key outcome variable of interest, Monthly per Capita Consumption Expenditure

[4]This statement is not exactly true as the program commenced in the Phase III districts with the beginning of the fiscal year 2008-09, that is in April 2008. However, this occurred at a low intensity with average monthly wage expenditures per capita of less than Rs. 10 in April and May of 2008, which compares to an average of Rs. 34 in our Phase II sample districts. While the former figure increased to Rs. 23 during the month of June, the resulting total wage expenditures per capita during the first half-year of 2008 in our Phase III sample districts amount to no more than Rs. 42, which compares to Rs. 204 in our Phase II sample districts. Moreover, since we expect some lag between wage disbursement and households' consumption, and since the interviews conducted by the NSS rely on a thirty day recall period, we regard the start of NREGA in the Phase III districts in April 2008 as a minor threat to our empirical approach, which treats Phase II districts as treatment and Phase III districts as control group. Nonetheless, we will revisit this issue in the robustness checks section.

(MPCE), takes into account a mixed recall period applied by the NSSO, thirty days for high-frequency items and 365 days for certain lumpy expenditure items. In line with common practice (Deaton, 2008), all prices are deflated to constant 2004-05 prices using the monthly Consumer Price Index for Agricultural Laborers (CPI-AL).[5] At 0.5 percent per month, rural inflation was similar to the overall rate of inflation in India during the time period we consider, July 2006 to June 2008. We calculate two poverty measures based on MPCE figures and state-wise poverty lines suggested by the Tendulkar Commission (Government of India, 2009), the headcount ratio (HCR), P_0, and the poverty gap ratio (PGR), P_1 (Foster et al., 1984). The 2004-05 Tendulkar poverty line for rural India, which equals Rs. 446.68 (about 30 US Dollars, purchasing power parity concept) is higher than the previously common Indian national poverty line, equal to Rs. 356.30 (or $23) (Government of India, 2007). Hence the Tendulkar poverty measure captures roughly "one dollar a day" poverty. We also experimented with poverty measures based on the traditional national poverty line but faced a problem of too little estimation precision because less than fifteen percent of the rural population in our sample districts is poor by that definition. For the Tendulkar poverty line, this figure stands at 32 percent in our sample.

Summary statistics for the sample of all rural households and the sub-sample of SC/ST households for the 63rd and 64th NSS round are set out in Tables 2 and 3, respectively. Each table gives sample means by year, phase, and season. For considerations of space, we have opted not to report standard deviations or standard errors. In accordance with the general trend since the 1990s, there is a decline in poverty in both samples. Both in Phase II and Phase III districts and across both groups of households, poverty according to the headcount ratio and the poverty gap ratio has declined between 2006-07 and 2007-08. At the same time, as expected, poverty in Phase II districts is higher than in Phase III districts in each of these two NSS rounds.

There are marked seasonal variations in the distribution of consumption, in particular its lower part, by NREGA phase. For the sample of all rural households, there is an increase in poverty as measured by the headcount ratio from fall to spring in Phase II districts in both rounds while the opposite is true in the less backward Phase III districts. Such a pattern is in line with smoother consumption across seasons in more forward districts. Given the general secular decline in rural poverty in India, a smooth consumption path across seasons implies a slight decrease in poverty from fall to spring in each agricultural year and hence NSS round. It is only the less backward Phase III districts that achieve such a pattern, however, while consumption in Phase II districts mirrors the annual agricultural cycle, where the bulk of agricultural activity, employment and yield occurs during the monsoon-fed kharif (fall) season.

[5] India's Labour Bureau provides these figures online (Government of India, 2013a).

3 Empirical Approach: Regression Discontinuity Design

In this section, we lay out our estimation strategy. While the papers by Azam (2012), Berg et al. (2012), Imbert and Papp (2013) as well as Bose (2013) all rely on the phase-wise rollout of the program and use differences-in-differences estimation techniques to identify labor market effects of the NREGA, Zimmermann (2012) casts doubts on the identifying assumptions behind such an approach. The intention of the phase-wise rollout of the program has been to bring the program to India's poorest districts first. The critical identifying assumption of a differences-in-differences analysis which uses Phase II districts as the treatment and Phase III districts as control group is that time trends are parallel between the baseline and endline, 2004-05 and 2007-08 in Azam (2012), for example, across Phase II and Phase III districts, that is between two groups of districts with markedly different baseline characteristics. While Azam (2012) finds no evidence against such a parallel time trend assumption in employment data spanning the time period 1999 to 2005, we find strong evidence against this assumption in NSS consumption data from the 2005-06 and 2006-07 NSS rounds (see below). In our view, this is not surprising. Given that monthly per capita consumption expenditures were more than twenty percent higher in Phase III relative to Phase II districts in 2006-07, our baseline year, Phase II and Phase III districts likely also exhibit markedly different structural features, such as access to financial and other markets and non-farm employment opportunities for rural households. That such structural features are predictors of subsequent growth and poverty reduction rates has been shown for Indian states by Datt and Ravallion (2002) and is, in our view, likely for districts, too.

To provide intuition for our empirical identification strategy, consider the union set of Phase II and III districts and suppose that, within this set, Phase II status was assigned to only the 130 most backward districts according to the Planning Commission's 2003 district backwardness index. Under the identifying assumption that expected consumption growth in a district is continuous in the Planning Commission's (PC) backwardness index, a local average treatment effect of the NREGA could be estimated using a sharp regression discontinuity design (RDD) by regressing consumption growth of a district between 2006-07 and 2007-08 on a flexible polynomial in the PC backwardness index and a dummy for belonging to the 130 most backward districts. Notice that, in this case, such a dummy equivalently captures Phase II status. That dummy's regression coefficient would yield a consistent estimate of the program's expected effect for a district whose PC backwardness index is at the Phase II - Phase III cutoff value.

The way the NREGA's Phase II was implemented deviates from such a clean scenario in two ways. First, the assignment of Phase II status to districts was implemented at the state rather than at the national level. This means that, in a first step, each state s was prompted to nominate a given number of districts, m_s say, for Phase II with the guideline that the state's poorest districts as measured by the PC backwardness index are to be given priority. Second, because of constraints in administrative capacity or other reasons such as political favoritism (Gupta, 2006), no state government nominated precisely the m_s poorest districts - as measured by the PC index - within its boundaries. Instead, some districts that should have been nominated following the PC index rule did not obtain Phase II status while some less backward districts in the same state did.

The first complication can be addressed by implementing a regression discontinuity design for each state. The required identifying assumption is that, within each state, a district's expected consumption growth rate conditional on the district's PC index is continuous in the latter. The second complication can be resolved by employing a fuzzy RDD at the state level. Toward this, a district's consumption growth rate is regressed on predicted Phase II status, where the prediction is based on the district's PC backwardness index and the state-wise PC index rule, rather than actual Phase II status. The additional two identifying assumptions needed for this procedure are, first, that a district's probability to be in Phase II is continuous in its PC index and, second, that there is a discontinuous jump in this probability at the state-specific threshold value of the PC backwardness index.

We implement this latter procedure in two steps. Consider a district as the unit of observation. In the first step, for each district of state s, we predict the probability of being notified in Phase II based on whether the district is among the m_s most backward districts of that state according to the PC index. In the second, consumption growth in each district is regressed on the predicted Phase II probability from the first step and a flexible polynomial in the backwardness index.

For practical purposes, Zimmermann (2012) suggests to use each district's within-state PC backwardness rank rather than the index itself and to force the polynomial of all states to be identical. More precisely, for each state, we rank the union set of all Phase II and III districts in descending order of the Planning Commission's index. Denoting the PC backwardness index for district d in state s by x_{sd}, we consider a district's rank among the Phase II and III districts of the same state, $rank_{sd}$. To be precise, we define

$$rank_{sd} = \sum_{i=1}^{n_s} {}^{1}\{x_{si} \geq x_{sd}\},$$

where n_s is the number of Phase II and III districts in state s and ${}^{1}\{\cdot\}$ denotes the indicator function. Recall that x_{sd} is smaller, the more backward the district. Then, the way $rank_{sd}$ is defined, the least (most) backward district of state s is assigned the first (n_s'th) rank. Taking m_s as given for each state, we define the *centered rank* of a district within its state by $crank_{sd}$, where

$$crank_{sd} = rank_{sd} - m_s.$$

Notice that, within each state, the centered rank of the least backward district that would obtain Phase II status if, within that state, selection of Phase II districts was based solely on the PC index, equals zero. Accordingly, the dummy variable ${}^{1}\{crank_{sd} \leq 0\}$ tells whether district d of state s should be a Phase II district if, in each state, districts were allocated to phases following the PC backwardness index strictly.

Using local linear regression as recommended by Lee and Lemieux (2010), our first stage estimating equation is

$$Phase2_{sd} = c_s + \alpha\ ^1\{crank_{sd} \leq 0\} + \eta_1\ crank_{sd} + \eta_2\ crank_{sd} *\ ^1\{crank_{sd} \leq 0\} + u_{sd}, \quad (1)$$

where $Phase2_{sd}$ equals one if district d in state s has Phase II status and u_{sd} is a stochastic error term. Notice that we allow for state-specific intercept terms and a different slope of the regression function to the left and the right of the cutoff value. Figure 3 plots the relative frequency of Phase II status averaged over all seventeen states in our sample over the variable *crank* together with a piece-wise linear regression function in the forcing variable, which includes a jump at zero. We have trimmed the sample to include only districts whose *crank* is no greater than ten in absolute value. There clearly is a downward jump in the data where the centered rank equals zero. This is mirrored by our first stage estimation results, which are set out in the first column of Table 4. Accordingly, conditional on a district's within-state centered rank, its probability to be in Phase II increases by 67.3 percent if it is among the state's m_s poorest districts.

While the first estimation stage is for a cross-section of districts, our second stage is for a repeated cross-section of households forming a district pseudo panel,

$$\begin{aligned} y_{sdit} &= \mu_{sd} + \gamma_{st} + \beta\ \widehat{Phase}2_{sd} * D0708_t + \delta_1\ crank_{sd} * D0708_t \\ &\quad + \delta_2\ crank_{sd} *\ ^1\{crank_{sd} \leq 0\} * D0708_t + \epsilon_{sdit}, \end{aligned} \quad (2)$$

where y denotes an outcome of interest, i and t are subscripts for households and time periods, respectively, and ϵ_{sdit} is a stochastic error term. There are two time periods, one for each of the NSS rounds canvassed in 2006-07 and 2007-08. The dummy variable $D0708_t$ equals one if an observation is from the latter NSS round. For estimating (2), we use the survey weights provided by the NSSO. This second stage can be viewed as a modified differences-in-differences estimating equation for a district pseudo panel. The first modification is the addition of a control variable, the centered within-state rank, which is assumed to be related to the outcome variable in a piece-wise linear fashion, the second one the use of a predicted value for a district's Phase II status rather than the district's actual program status.

Our estimation strategy as laid out in (1) and (2) gives each district an equal weight in the first stage while each district's implicit weight in the second stage estimation equals its population share. An alternative, more standard, approach to the estimation of the program effect would be to estimate (2) with $Phase2_{sd}$ substituted for $\widehat{Phase}2_{sd}$ by instrumental variables, where $Phase2_{sd}$ is treated as endogenous regressor and $^1\{crank_{sd} \leq 0\} * D0708_t$ is used as identifying instrument (see Lee and Lemieux, 2010). In such a specification, each district is given the same weight, its population share, in both estimation stages. The resulting point estimate of β is a Wald estimator, the extent of the discontinuity in the outcome variable of interest divided by the extent of the discontinuity in the probability of being notified in Phase II. An essential feature of our subsequent empirical analysis is that we will consider alternative subsets of households. In the instrumental variables approach, the first stage estimation results and hence the denominator of the Wald estimator of the program effect depend on the district weights implied by the respective subset of households that is being considered. As the vector of implied district weights varies greatly across the sub-samples which we will consider, the instrumental variables approach yields substantially different program effect estimates in two alternative sub-samples, even if the discontinuity in the outcome variable of interest is exactly the same in both sub-samples. System (1) and (2), on the other hand, avoids this artefact because the program effect estimates for different sub-samples are essentially Wald estimators with an identical denominator.[6] We will revisit this issue in Section 5.4.

We close this section with a discussion of the computation of standard errors for our two-stage approach. As shown by Murphy and Topel (1985), ordinary least squares standard errors are biased when a "generated regressor" is used, as in our second stage. Another complication regarding the calculation of standard errors is that, for each of the two NSS rounds, we want to allow for a non-zero correlation among the error terms of households residing in the same district. Since we were not able to find explicit formulas for standard errors when there is a generated regressor as well as the need for clustering, we calculate clustered standard errors as if there was no generated regressor in (2) and correct those standard errors as suggested by Murphy and Topel (1985; equation 17) for non-clustered standard errors by the factor $\sqrt{1+\widehat{\beta}^2 MSE_1/MSE_2}$, where $\widehat{\beta}$ is the estimate of β from an ordinary least squared estimation of (2) and MSE_k denotes the mean squared error from estimation of the k'th stage estimating equation. We realize that such an approach is somewhat ad hoc. On the other hand, it is beyond the scope of this paper to assess whether the resulting standard errors are consistent. Therefore, in Section 5.4, we also estimate a standard two stage least squares version of system (1) and (2), where consistent clustered standard errors are available, without obtaining any qualitatively different results - that is with respect to sign and statistical significance - from the ones reported in the next section.

4 Results

4.1 Main Results

Our outcomes of interest are individual consumption and consumption-based poverty measures. Table 5 contains the coefficient estimates of β in (2) for alternative dependent

[6] While the point estimate of β in our approach does not exactly equal the ratio between the second-stage and the first-stage discontinuity, none of the point estimates in Table 5 differs from that ratio by more than 1.5 percent of the respective standard error.

variables and different (sub-)samples of households. We also report the number of districts for which there are observations in both the 63rd and the 64th NSS round as our estimates of the program's effect are based on only such districts. For all regressions whose results are reproduced in this table, the predicted values of Phase II status for each district are obtained from the estimation of (1) whose results are set out in the first column of Table 4. In all regressions, the sample is trimmed such that only districts with a *crank* of no more than ten in absolute value are included.

Standard errors are calculated as described above, that is we conduct an ordinary least squares estimation of (2), calculate clustered standard errors, where a cluster is the set of households residing in the same district in a given NSS round, and adjust the standard error for $\widehat{\beta}$ thus obtained according to equation 17 in Murphy and Topel (1985). The correction factor that obtains for the estimations whose results are set out in Table 5 never exceeds 1.1. All results in Table 5 are estimated from the 2006-07 and 2007-08 NSS rounds. From the upper left panel, it is evident that the trimming results in a loss of 54 districts, 201 instead of 255. Since the 63rd NSS round fails to contain one of these 201 districts, there are 401 clusters. Comparing the upper and center left panels, we see that 23 percent of the households that are sampled in the two relevant NSS rounds and reside in either a Phase II or a Phase III district, belong to scheduled castes or scheduled tribes. That this fraction is substantially smaller than the share of SC/ST households in all rural households, 26.5 percent, is due to the sampling stratification employed by the NSSO, by which relatively wealthy households are systematically oversampled in thin survey rounds (see Table 6). While the sampling methodology in both NSS rounds ensures that almost all districts are represented in each round, SC/ST households are not sampled deliberately. Hence, even if there are SC/ST households in each district, random sampling within each district results in no SC/ST households being interviewed in some districts. Comparing the upper and center left panels of Table 5, we see that this has happened in three instances, that is district-year pairs. A loss of clusters also occurs when we consider observations from only one of the two agricultural seasons, that is July to December or January to June. Comparing the center left with the two panels to its right, we see that such random drawing of the interview date results in a loss of twenty and sixteen clusters in fall and spring, respectively.

In each panel, the column "MPCE" has logarithmic monthly per capita consumption expenditures at constant prices as dependent variable, while in columns HCR and PGR, the headcount ratio and poverty gap ratio are the dependent variables, respectively. As pointed out previously, the estimated effects are not average treatment effects for the set of all Phase II districts, but local treatment effects capturing the expected program effect for a household residing in a district that is on the edge of being allocated to Phase II or Phase III as predicted by the district's backwardness index.

Turning to the estimation results, there are only small and statistically insignificant results for our full sample. For SC/ST households, we estimate an increase in logarithmic consumption and economically significant decreases in poverty when pooling the observations from both agricultural seasons (center left panel). The disaggregated seasonal analyses for SC/ST households reveal that the effects for the full year are entirely driven by the spring season, where we find large increases in consumption expenditures and decreases in poverty. Albeit imprecisely estimated, the center right panel's entry in the MPCE column implies that SC/ST consumption expenditures have increased by 37.3 percent on average due to the presence of NREGA sights during the agricultural lean season, the spring of 2008. Turning to the poverty measures, our estimates imply a reduction in the incidence of poverty as measured by the headcount ratio of 45 percentage points and a decrease in the poverty gap measure of 11.7 percentage points. These effects are very large taking into account the 2006-07 reference values of 69.3 and 19.8 for these two measures. The limited number of observations in the seasonal SC/ST analyses, each of which comprises only a little more than a tenth of the observations in our full data set, and the loss of clusters due to random sampling results in a considerable loss in estimation precision and a lamentable increase in standard errors.

For reference, Table A1 contains estimates of β for a variation of (2) in which actual Phase II program status, $Phase2_{sd}$, is substituted for predicted program status $\widehat{Phase}2_{sd}$. Such a specification amounts to a standard differences-in-differences estimation of the NREGA's program effect. While it allows for different time trends in the outcome variable across districts by centered rank, it fails to purge any bias in the estimation of β arising from selection issues. Such a bias will occur in particular if, absent the NREGA, a district that is assigned Phase II (III) status with a centered rank greater (weakly smaller) than zero exhibits a systematically different growth rate in the outcome of interest than predicted by that same district's *crank*. To make a case, suppose that districts that should have been in Phase II according to the state-wise PC index rule, that is $Phase2_{sd} = 1$ if and only if $crank_{sd} \leq 0$, but end up in Phase III, have an especially poor administrative capacity. If administrative capacity of a district is positively correlated with its rate of consumption growth absent the NREGA, then such selection will bias an estimate of β upward because on average the growth rate of a district actually in Phase II is greater than predicted by its *crank* and the converse is true for Phase III districts. Regarding the seasonal pattern of program effects, the point estimates obtained from this approach exhibit marked qualitative differences relative to the ones obtained from our two stage procedure for the sample of SC/ST households. As expected, the coefficients are estimated much more precisely when actual rather than predicted Phase II status is used. We will turn to the credibility of these differences-in-differences estimates in the context of a placebo experiment in the next section.

5 Extensions and Robustness Checks

5.1 Alternative Sub-sample of Vulnerable Households

In this subsection we consider an alternative subgroup of especially poor and vulnerable households, rural laborers. This is facilitated by the fact that the NSS consumption questionnaires report the household's principal occupation. While we would have preferred to look at only agricultural laborers, we found the resulting sub-sample too small. The union set of agricultural and other laborers, in contrast, is of a similar size (twelve percent larger, to be precise) as the one comprising all SC/ST households. According to the descriptive statistics for our base year 2006-07 in Table A2, this group's and SC/ST's welfare characteristics, as captured by consumption and poverty, are very similar. Rural laborers' average monthly per capita consumption expenditure, headcount ratio and poverty gap ratio figure at Rs. 513, 51.4 percent and 12.3 percent in 2006-07, which compares to Rs. 497, 52.7 percent and 13.5 percent among SC/STs, respectively. Still, the two subgroups overlap only partially. In the data set that we use for our core analysis, 26.5 percent of the population belong to scheduled castes and scheduled tribes and 36.2 percent are laborers. A little more than half of the SC/ST population report themselves as laborers. As a consequence, fifteen percent of the population in our full sample are both SC/ST and laborers, which implies that the majority of laborers, 58 percent to be precise, does not belong to scheduled castes and tribes. Analogous to our core analysis, we estimate system (1) and (2) with the sub-sample of rural laborers only. According to the bottom panel of Table 5, there is no statistically significant effect of the NREGA for this part of the rural population.

We end this subsection by pointing out that the sub-sample of SC/ST households is our preferred group of especially vulnerable households. Classification as rural laborer is in response to a question regarding the household's principal occupation, where the three relevant categories for our purposes are laborer, self-employed, and other. In this connection, we fear two potential problems in conjunction with the NREGA. The first one is a selection issue. The presence of NREGA sights creates additional non-farm employment opportunities, which may affect a household's choice of principal occupation. For example, the extra availability of non-farm employment may prompt a household head that would have formerly reported himself as working primarily as agricultural laborer to report the household as doing primarily non-agricultural labor. Such an effect of the NREGA should not jeopardize the consistency of our analysis of rural laborers' welfare because we consider the union set of agricultural and non-agricultural laborers. Among marginal farmers, however, it is conceivable that NREGA employment opportunities prompt some households to move from the category self-employed in agriculture to laborers. As a consequence, the laborer sub-samples in our baseline and endline rounds would, in general, not be comparable. The second issue may be labelled reporting bias. The answer to the occupational question is based on a perception of the household head.

Even if the household's own occupational activities do not change with the NREGA, the change in behavior among peer households, in this case working more in non-farm wage employment, may affect a household's perception of its principal occupation. Any of these two effects is likely to result in biased program effect estimates, even within our identification framework.

5.2 Migration

A concern that arises in the context of our analysis is that the program potentially alters migration incentives and hence the composition of the rural population in the Phase II and III districts differently between the baseline and endline surveys. For the Mexican PROGRESA, for example, Stecklov et al. (2005) find that exposure to PROGRESA reduced out-migration to the United States by about one fifth while it did not affect domestic migration in a measurable way. Given a rate of out-migration to the US of less than a one percent per year, however, such a program effect on migration would not severely jeopardize an analysis like ours, where baseline and endline together span no more than two years.

In rural India, migration is substantial. According to the 2001 Census of India, the annual rate of rural-urban migration stood at around seven percent per year. Our results of substantial welfare improvements among SC/ST households would be jeopardized if the availability of NREGA sights increased the rate of out-migration among especially poor households or decreased the rate of out-migration of especially wealthy, non-poor households. In such a scenario, improvements in poverty due to the NREGA would merely be due to a relocation of poverty away from the rural areas of the Phase II districts. Using a differences-in-differences estimation approach and NSS migration modules from two years, Ravi et al. (2013) find that the NREGA drives down migration in Phase II districts by as much as a quarter. Similarly, in a study of two north-western states of India, Imbert and Papp (2014) find that the NREGA reduces short-term migration of rural laborers. Such a pattern would lead to a systematic increase in the population of Phase II districts relative to our control (Phase III) districts. While these authors do not explicitly disaggregate migration flows by initial wealth, both papers find that the entire effect of NREGA on migration is driven by laborers, which are far more likely to belong to the poorer half of the rural consumption distribution (see Table A2). This implies that the migration effect of NREGA will result in lower average consumption in Phase II districts - as one would expect intuitively. Hence, our estimates regarding household welfare should be conservative ones.

5.3 Placebo Experiment (or Falsification Test)

In this subsection, we assess the validity of one of the identifying assumptions underlying our two-stage analysis. In particular, we test whether a district's expected growth rate conditional on its *crank* does not exhibit a discontinuity at *crank* equal to zero absent the NREGA. Toward this, we estimate system (1) and (2) with data from the 62nd and 63rd NSS round.

Sample means for the 62nd NSS round are set out in Table A5. The results of this exercise are set out in Table 7. For all sub-samples, the point estimates are far from being statistically significant. The standard errors for the sub-sample of SC/ST households are around forty percent larger in the placebo than in our core analysis, which is due to the smaller number of observations in the 62nd NSS round relative to the 64th. This raises the issue whether the placebo analysis suffers from a lack of power. The absolute magnitude of the greatest point estimates obtaining in the placebo, SC/ST during fall, is still only about a half of those for SC/ST during spring in our main estimations, which we take as evidence in favor of the hypothesis that there is no discontinuity in a district's expected consumption growth rate at the *crank*-cutoff absent the NREGA.

Another potential issue with our placebo is that the NREGA started to operate in Phase II districts in April 2007. Hence, in our Phase II districts, the last three months of the 63rd round may be affected by the onset of the NREGA. As Figure 2 shows, however, this occurred at a very low intensity. In particular, no wage expenditures are recorded for the month of April 2007 and the somewhat greater expenditures during June 2007 are unlikely to affect June consumption as the consumption data are based on a mixed recall of thirty and 365 days. In line with this argument, all three point estimates for SC/ST households during spring are small and insignificant.

Table A3 sets out the results of a placebo experiment for a differences-in-differences version of (2), where $Phase2_{sd}$ is substituted for $\widehat{Phase}2_{sd}$. There are large and significant placebo effects for all rural households during the fall season and for SC/ST households during spring. Accordingly, consumption growth and poverty reduction between the spring seasons of 2006 and 2007 was about twice as large as between 2007 and 2008. Taken together, we conclude from the results of the two placebo experiments that the parallel time trend assumption underlying a differences-in-differences approach is clearly violated while the identifying assumptions of the fuzzy regression discontinuity design appear to be valid.

5.4 Sampling Weights and Two-stage Least Squares Implementation of Fuzzy Regression Discontinuity Design

As discussed in Section 3, our empirical specification as laid out in (1) and (2) assigns equal weights to all districts in the first stage while each district's weight in the second stage estimation is equal to its population share. This is the fundamental difference to a textbook implementation of a fuzzy regression discontinuity design, which amounts to two stage least squares estimation (Lee and Lemieux, 2010). The latter can be implemented by conducting an instrumental variables estimation of (2) with $Phase2_{sd}$ substituted for $\widehat{Phase}2_{sd}$, where $Phase2_{sd}$ is treated as endogenous regressor and $^1\{crank_{sd} \leq 0\} * D0708_t$ is used as identifying instrument. In such a specification, each district is assigned the same weight in each estimation stage, which equals its population share.

To illustrate the sensitivity of the Wald estimator of the program effect to the choice of sub-sample within this approach, columns 3 to 8 of Table 4 set out alternative first stage results of a standard instrumental variables version of system (1) and (2). The third column, which uses data from all rural households, gives results very similar to the first column. On the other hand, between the last two columns, which are for the sub-samples of SC/ST households during fall and spring, respectively, the difference in the estimated jump varies by almost a quarter.

As expected, the magnitude of estimated coefficients, which are set out in Table 8, is even more dramatic than in Table 5 for the sub-sample of SC/ST households during the spring season. On the other hand, the order of magnitude and the pattern of statistical significance across the different sub-samples is unchanged, at least as far as the five percent significance level is concerned. We take this as support for the validity of our procedure for calculating the standard errors in our preferred specification. As an additional robustness check, we also carry out a placebo estimation using instrumental variables estimation and data from the 62nd and 63rd NSS rounds. According to Table A4, as in Table 7, none of the estimated coefficients is statistically significant at conventional levels.

A third possibility of weighting districts in the two stages of the estimation is to give each district an identical weight in both stages. This corresponds to Zimmermann's (2012) approach, who carries out all estimations with district averages. Such an approach yields a program effect estimate which is representative for an average district at the cutoff of the centered rank, while the estimates set out in Table 5 are representative for the population in districts located around the cutoff. Asymptotically, the resulting coefficients of interest will be different if the local average treatment effect is heterogenous with regards to district population size. The results of this exercise are set out in Table 9 and confirm our previous findings qualitatively. The point estimates are much smaller with this alternative weighting scheme, however, and only logarithmic monthly per capita consumption of SC/ST households during the spring season increases in a statistically significant fashion.

5.5 Regression Discontinuity Design Applied to only Endline Data

Our estimation strategy in the main empirical analysis can be thought of as a fuzzy regression discontinuity design applied to changes in welfare outcomes between two years, where the relevant unit of observation is a district and district averages for each of the two years of data are calculated from household-level data in a first step. One key identifying assumption of such an approach is that the expected change in average household welfare in a district conditional on the district's backwardness index is continuous in that index absent the NREGA. Over the last ten years, panel RDD analyses have become common in empirical economics and have been applied fruitfully in many different contexts (see Lee and Lemieux, 2010, for references).

In this subsection we explore a simpler RDD specification using a cross section of districts with data from the endline survey only, that is from 2007-08. This corresponds to the fuzzy RDD textbook case. The underlying identifying assumption then is that the level of expected average household welfare in a district conditional on the backwardness index is continuous in that index absent the NREGA. The estimation continues to proceed in two steps. The first step (1) for predicting Phase II remains unaffected. The estimating equation for the second step now becomes

$$y_{sdi} = \mu_s + \beta \ \widehat{Phase}2_{sd} + \delta_1 \ crank_{sd} + \delta_2 \ crank_{sd} * {}^1 \{crank_{sd} \leq 0\} + \epsilon_{sdi},$$

where all observations for y are from the 64th NSS round. We expect this approach to have less power because pre-program differences between districts become unobserved heterogeneity in this cross-sectional approach.

The results are set out in Table 10. While the pattern of the signs of the estimated coefficients is the same as in Table 5, none of the estimated effects is statistically significant at the five percent level, which comes as no surprise given our just-mentioned reservations regarding the power of such an approach in our small sub-samples.

5.6 Alternative Baseline Year

In this subsection, we explore the 62nd NSS round as an alternative baseline round. We see two advantages and two disadvantages using the 62nd in place of the 63rd round as baseline. Turning to the disadvantages, we expect the residual variance to be greater because of a longer time spell between baseline and endline. Second, all estimates will be less precise as the sample size in the 62nd round is only about half of that of the 63rd round. On the other hand, unlike the 63rd, the 62nd round as a baseline is not affected by the onset of the NREGA in April of 2007. Finally, compared to the 63rd round, it has a sampling strategy more similar to that of the 64th round. As set out in Table 6, both the 62nd and 64th round follow the Indian NSSO's usual second stage stratification strategy, where an equal number of wealthy and non-wealthy households is interviewed in each block that has been drawn for inclusion in the NSS sample. It is only the definition of "wealthy" that varies across these two rounds. In particular, land ownership serves as criterion in the 62nd round while it is the possession of certain assets in the 64th. The 63rd round, on the other hand, has the singular feature of initially stratifying by participation in public works. If the sampling weights, which the NSS includes with each observation, were correct, variations in the second-stage stratification across survey rounds should not matter. Given the sensitivity of various findings derived from these surveys, e.g. regional poverty trends, to other survey features, such as the recall period (see, e.g., Deaton and Kozel, 2005), we are somewhat sceptical about variations in the sampling methodology, however. In particular, since SC/ST households demand NREGA employment much more often than non-SC/ST households, we suspect that the stratification by public works employment in the 63rd round could lead to a misrepresentation of such households, even when using the weights supplied by the NSSO.

To assess this possibility, we estimate system (1) and (2) with the dependent variable equal to a dummy which takes the value of one if the interviewed household belongs to a scheduled caste or a scheduled tribe. We would like to stress that, as in all other regressions, we use the weights provided by the NSSO. Hence, in principle, the estimated effects are representative for the entire rural population. For the baseline and endline years underlying our main analysis, the results are set out in columns 1, 3 and 5 of the upper panel of Table 11. According to column 1, the incidence of SC/ST individuals has dropped by 12.8 percentage points in response to NREGA's Phase II. This point estimate is significant at the five percent significance level and driven by the fall season, for which the point estimate equals more than fifteen percent. For the spring season, there is no statistically significant effect. We have carried out the same exercise with the dependent variable rural laborer, whose results are set out in columns 2, 4 and 6 of the upper panel. Again there are statistically significant effects of the NREGA, albeit of opposite sign. The lower panel of Table 11 sets out the results of the same exercise with the 62nd and 64th rounds of NSS data. For SC/STs, all estimated effects are small and statistically insignificant. Taken together, the pattern of results across the two panels is suggestive of differences regarding the populations that are represented in the 62nd and 64th round on the one hand, and the 63rd round on the other.

Sample means for the 62nd round are set out in Table A5 for SC/ST households. The results for system (1) and (2) with the 62nd round as baseline are set out in Table 12. They confirm our findings for SC/STs during the spring season both qualitatively and quantitatively. As expected, the precision of the point estimates deteriorates relative to our main results in Table 5.

5.7 Timing of NREGA Onset and Consumption Survey Interviews

As mentioned in Section 2.2, the program commenced in Phase III districts in April 2008. While this occurred at a very low intensity, in principle this onset of the program in our control group of districts potentially biases our program effect estimates. This applies in particular to the spring season, for which consumption interviews take place between January and June. While we expect our program effect estimates set out in Table 5 to be downward-biased in this scenario, we repeat our main analysis for SC/ST households during the spring season with consumption interviews held only during the first quarter of 2007 and 2008. The results are set out in column 6 of Table 13. With less than 1,500 observations, for logarithmic MPCE we continue to find a statistically significant effect very similar in magnitude to the one in Table 5. The effects for the two poverty measures, on the other hand, are muted and insignificant.

5.8 Trimming, Functional Form of the Regression Discontinuity Design and Control Variables

As pointed out by Lee and Lemieux (2010), unlike in many instances of panel data fixed effects estimation, panel RDD regression equations do not require the inclusion of any controls or fixed effects to ensure consistent estimation of causal effects. The essential explanatory variables are a polynomial in the continuous forcing variable, here the centered rank, and a dummy for the discontinuity, each interacted with an endline dummy. In this subsection we explore alternative specifications of our system of estimating equations regarding the choice of trimming, fixed effects and control variables. For considerations of space, we discuss only results for SC/STs during spring. Columns 1 and 2 of Table 13 set out results for different extents of trimming. Neither widening nor narrowing the *crank* window by five steps changes our main results in a remarkable way, though narrowing decreases the precision greatly. In this context, it is to be noted that further trimming, as in column 1, results in a loss of a third of the districts used in our main estimations.

We also explore a local polynomial regression with distinct quadratic polynomials to the left and right of the cutoff. To be precise, the first stage in this specification is

$$\begin{aligned} Phase2_{sd} \;&=\; c_s + \alpha \; ^1\{crank_{sd} \leq 0\} + \eta_1 \; crank_{sd} \; + \eta_2 \; crank_{sd}^2 \\ &\quad +\xi_1 \; crank_{sd} \; * \; ^1 \{crank_{sd} \leq 0\} + \xi_2 \; crank_{sd}^2 \; * \; ^1 \{crank_{sd} \leq 0\} + u_{sd} \end{aligned}$$

and the second stage

$$\begin{aligned} &y_{sdit} = \mu_{sd} + \gamma_{st} + \beta \; \widehat{Phase2}_{sd} * D0708_t + \delta_1 \; crank_{sd} * D0708_t + \delta_2 \; crank_{sd}^2 * D0708_t \\ &+\kappa_1 \; crank_{sd} \; * \; ^1 \{crank_{sd} \leq 0\} * D0708_t + \kappa_2 \; crank_{sd}^2 \; * \; ^1 \{crank_{sd} \leq 0\} * D0708_t + \epsilon_{sdit}. \end{aligned}$$

According to column 3 of Table 13, our previous findings continue to obtain under this modification and the point estimates are larger.

Column 4 is as our main specification but without state-endline year interactions. To be precise, the terms c_s and γ_{st} in (1) and (2) are replaced by c and γ_t, respectively. As the point estimates show, our main results are robust to these two omissions but the estimated effects are muted. Figure 4 depicts the reduced form corresponding to system (1) and (2) with c and γ_t substituted for c_s and γ_{st}, respectively,

$$\begin{aligned} y_{sdit} \;&=\; \mu_{sd} + \gamma_t + \beta \; ^1\{crank_{sd} \leq 0\} * D0708_t + \delta_1 \; crank_{sd} * D0708_t \\ &\quad +\delta_2 \; crank_{sd} * \; ^1 \{crank_{sd} \leq 0\} * D0708_t + \epsilon_{sdit}, \end{aligned}$$

for logarithmic MPCE. Accordingly, there is an estimated downward jump at the discontinuity of 17.5 percentage points, which, once divided by the estimated jump in the probability of being notified under Phase II, 0.75 (see column 2 of Table 4), roughly gives the point estimate in the fourth column of the upper panel of Table 13, 22.98.

Finally, in column 5 we have added dummies for different household sizes as explanatory variables, so the interpretation of the estimated program effect is conditional on household size. Our main findings continue to obtain, albeit slightly muted in magnitude.

6 Cost-Benefit Analysis

We first summarize the empirical findings obtained thus far. While we have not found statistically significant program effects for the sample of all rural households and the subsample of rural laborers residing in NREGA Phase II and Phase III districts, we have found very large and statistically significant local average treatment effects on consumption growth and poverty reduction for the subgroup of scheduled castes and scheduled tribes during spring, which is the agricultural lean season. While all point estimates in our disaggregated analyses suffer from a lack of precision, the pattern of the Act's welfare effects as elicited by our findings is clear. The main beneficiaries are households belonging to a particularly deprived subgroup of the rural population and the effects occur during the season in which the risk of consumption shortfalls is greatest. For the subgroup of SC/STs, consumption gains are especially large in the lower part of the consumption distribution. Given the lack of precision in the respective estimations, we view the pattern of welfare improvements generated by the NREGA as the major insight of our empirical analysis, rather than the point estimates, which we think should be taken with a grain of salt.

Figure 5 depicts the estimated effect on consumption expenditures among SC/ST households in a stylized fashion. The solid and the dashed line depict fall and spring consumption in 2006-07, respectively, by district backwardness. The location and slope of the two lines imply that, in backward districts, spring consumption falls considerably short of fall consumption, while fall and spring season consumption are similar in the less backward districts with a *crank* greater than zero. This is in line with the sample means set out in the center left and bottom left panels of Table 3. Accordingly, Phase II districts experienced a consumption drop of about eighteen percent from fall 2006 to spring 2007 while Phase III districts enjoyed an increase of about two percent. In terms of Figure 5, our results imply a program effect resulting in an upward shift of the left part of the dashed line. To be precise, the estimated local average treatment effect only tells that there is an upward shift at zero, the cutoff value of the centered rank. For the figure we have implicitly assumed a homogeneous treatment effect of the NREGA with regards to a district's centered rank, which implies a parallel shift upwards of the dashed line to the left of zero. The resulting new situation clearly implies smoother consumption across the two seasons for SC/ST households in backward districts, and this is in fact what the sample means in the center right and bottom right panels of Table 3 imply. Accordingly, mean per capita consumption increased by about two percent for both the Phase II and III districts from the second half-year of 2007 to the first half-year of 2008.

To assess whether the NREGA has had a lasting impact on seasonal consumption patterns of SC/ST households, Table A5 sets out SC/ST consumption and poverty by

NREGA phase and season for all NSS rounds featuring a consumption expenditure module since 2003. The upper panel covering the agricultural year 2003-04 is calculated from two rounds, the 59th and 60th, as the former covers the calendar year 2003 and the latter only the first half-year of 2004. Figure 6 depicts the time series of logarithmic MPCE among households belonging to scheduled castes and scheduled tribes by NREGA phase together with all-India agricultural production for each half-year from fall 2003 to spring 2012. Two stylized facts emerge from the table and figure. First, consumption averaged over an agricultural year tracks agricultural output closely in both groups of districts, that is consumption smoothing across years is far from complete irrespective of NREGA phase status. Second, prior to the NREGA, the risk of a consumption shortfall during the second half of an agricultural year is far greater in Phase II than in the less backward Phase III districts. To be precise, according to Table A5, average logarithmic consumption is lower in spring than in fall in Phase II districts in all years up to 2006-07, while Phase III districts enjoyed a moderate increase in logarithmic consumption at the same time. This seasonal consumption pattern in Phase II districts is in accordance with the seasonality in agricultural output, which is on average roughly ten percent larger in fall than in spring. Given an increase in logarithmic consumption between 2003-04 and 2011-12 of 14.5 and 20.1 in Phase II and Phase III districts, respectively, a perfectly smooth consumption path involves an increase in logarithmic consumption of 0.90 and 1.25 from fall to spring in each agricultural year, respectively.[7] It is evident from Table A5 that Phase III districts get much closer to such a pattern than Phase II districts prior to the NREGA. In particular, for the four agricultural years between 2003-04 and 2006-07, intra-year changes in logarithmic consumption averaged at -8.1 in Phase II compared to +4.5 in Phase III districts with standard deviations equal to 7.5 and 3.4, respectively. In the three consumption surveys available since 2007-08, the mean changes between the two seasons of the same agricultural year are -0.1 and +1.1, respectively, with standard deviations equal to 3.5 and 3.8, respectively. A related fact that emerges from Figure 6 is that the consumption paths of Phase II and Phase III districts co-move much more closely from 2007-08 onward. To elaborate, the correlation coefficient between the two consumption time series is -0.15 up to the spring of 2007 and +0.96 afterwards. Together, we take these facts as suggestive evidence for a sustained effect of the NREGA on consumption smoothing among SC/ST households in backward districts.

The pattern of program effects that we find is consistent with the pattern of NREGA program expenditures. Regarding the beneficiaries of the program, Table 1 tells that almost half of all NREGA work days in our sample was performed by SC/STs. The figures imply that non-SC/ST individuals performed only 1.23 person days on average, about a third of the 3.51 person days performed by a representative individual from scheduled castes and scheduled tribes. Regarding seasonality, Figure 2 plots monthly NREGA wage expenditures in our sample's 92 Phase II districts relative to the rural population and

[7] There are sixteen half-years between January 2004 and January 2012. The two numbers 0.90 and 1.25 are obtained from dividing 14.5 and 20.1 by sixteen, respectively.

wages paid to SC/STs relative to the rural SC/ST population. Accordingly, expenditures between January and June 2008 were on average three times the expenditures during the agricultural peak season in the fall of 2007. Combining the information on NREGA expenditures with our point estimates, we conclude that the estimated increases in SC/ST consumption are large relative to NREGA wage expenditures. The monthly wage expenditures of close to Rs. 60 per SC/ST capita during the spring season of 2008 come together with estimated consumption benefits of between Rs. 61 and Rs. 140, depending on whether we take the smallest of our point estimates for MPCE from Table 10 or the one from our preferred specification (Table 5). Hence, the Act appears to have been cost-effective in improving welfare among SC/ST households by reducing exposure to systematic seasonal consumption shortfalls. This conclusion continues to hold even if our point estimates overstated the true effect by a factor of two or four. A qualification that has to be made regarding the methodology of this cost benefit analysis is that we have compared our local average treatment effect as an estimate of the benefit of the program to the cost measured as an average difference between Phase II and III districts.

Deininger and Liu (2013) find similarly large short-term effects, of Rs. 140 per month, on SC/ST consumption in Phase II and III districts in the state of Andhra Pradesh. These authors' findings also imply that the welfare effects exceed the direct transfers to workers from the program. As their data cover only the agricultural peak season, however, their analysis does not address the seasonal pattern of program effects.

In principle, there are three channels by which income of poor rural households may benefit from a public employment program, first, wage income increases from more days of employment due to work on the NREGA sites, second, wage income increases from earnings in non-NREGA employment due to an increase in the equilibrium wage rate in the rural labor market and, third, income increases from wage labor and self-employed activity due to a higher marginal product of labor, which arises from the infrastructure put in place by NREGA work. As Berg et al. (2012) point out, the third of these channels appears to be negligible in the context of our analysis. Given that we consider only the first year of the program's second phase and that much of the activity unfolded not before January 2008, it is unlikely that household consumption benefited much from such infrastructure by the first half-year of 2008. Regarding the second channel, Berg et al. (2012) find that impacts on non-NREGA wage rates are lagged by about nine months, which is at odds with Zimmermann (2012), who finds a large instant, but imprecisely estimated, effect of the NREGA's Phase II on female casual wages during the fall season of 2007, that is when the program operated at a low intensity in the Phase II districts compared to the first half-year of 2008 (see Figure 2). Our empirical analysis leaves open the potential contributions of the just-mentioned three channels to the consumption increases that we estimate. Moreover, in principle, NREGA employment and the high female participation rate may also affect consumption through changes in household savings or intra-household decision making processes. Still, a rough calculation with Zimmermann's estimates suggests that the effect on female private-sector wages will result in a per capita monthly income increase of no more than Rs. 15 (this is based on a five person household with two female laborers) while the program expenditure data in Figure 2 implies that NREGA employment increases monthly per capita income by around Rs. 35 during the first half-year of 2008 (which is based on the assumption that no private sector employment is crowded out and that the program's wage expenditures fully reach the employed laborers). Hence, our impression is that the NREGA's short-term effect on consumption in Phase II districts is mostly attributable to the program's direct effect on households' labor incomes rather than any of the indirect, general equilibrium, effects.

7 Concluding Remarks

Governments of low and middle-income countries around the globe have been and are using large-scale public employment programs to provide livelihood security and combat poverty in their rural areas. Given that more than a third of the world's rural poor who live on less than a dollar per day resided in India in 2002 (Ravallion et al., 2007), assessing the costs and benefits of India's thus far largest public employment program is of immediate interest. We have embarked on our analysis of welfare and poverty effects of India's National Rural Employment Guarantee Act arguing that a pure labor market perspective is certainly important in its own right but not sufficient to judge an employment program's effect on rural households' livelihoods. Previous, often qualitative, field studies have claimed that many workers employed under India's NREGA use their public works' wages for goods and services which they previously considered prohibitive, like bicycles or children's education (Khera, 2011). In this paper, we have explored quantitatively whether NREGA employment opportunities have translated into higher levels and smoother patterns of consumption at an all-India level.

While we have not found statistically significant program effects in a sample representative for the entire rural population in the districts that we study, we have found economically and statistically significant poverty-reducing effects for the sub-sample of households belonging to scheduled castes and scheduled tribes during spring, which is the agricultural slack season. Our econometric findings for the time period 2006 to 2008 combined with patterns emerging from descriptive statistics for the years 2003 to 2012 suggest that the NREGA has helped this group of households in a sustained fashion to smooth consumption between the agricultural peak and slack seasons. Our main conclusion is hence that the NREGA has been successful not only in increasing consumption levels of particularly vulnerable households but also in reducing these households' exposure to the risk of seasonal drops in consumption. The pattern of these effects is consistent with the pattern of program expenditures. We have documented that, in our sample, about one in two workers on NREGA sites belongs to a scheduled caste or a scheduled tribe, and that the bulk of NREGA work is carried out during the spring season. Combining this information with our estimated welfare effects, we conclude that much of the public works' wages appear to have contributed to additional consumption by marginalized rural households during the agricultural off-season.

The text of the Act itself specifies among the main goals of the scheme "ensuring livelihood security for the poor" and "ensuring social protection for the most vulnerable people living in rural India" (Government of India, 2013b). In the language of economists, the former calls for risk reduction while the latter highlights the aspect of proper targeting. Our analysis suggests that the Act has successfully delivered on both of these two objectives.

In our view, the main shortcoming of our empirical analysis is the low precision of the estimated program effects, which is rooted in three reasons. First, we identify program effects from district-wise changes in consumption and there are only 255 districts available for our analysis. Second, our analysis relies on so-called thin rounds of India's National Sample Survey, where the sample size is comparatively small. Third, we conduct disaggregated analyses by agricultural seasons and population subgroups, which cuts the size of our sample further by a factor of up to ten. To deal with these complications, first, we have employed a modification of the usual instrumental variables implementation of a fuzzy regression discontinuity design, which substantially reduces the variability of program effect estimates in our small samples when program status is assigned at a higher than the individual level, in our case the district. Second, we have carefully examined the validity and robustness of our main findings by subjecting them to numerous robustness checks and extensions. Third, we have pointed out that we view the seasonal and subgroup-specific pattern of welfare improvements generated by the NREGA as our major insight, rather than the magnitude of the point estimates.

Finally, there are limitations to the scope of our analysis. Driven by the objective to identify causal program effects, we have examined only one, the first, year of that phase of the Act which was the smallest among the three phases of the NREGA rollout, covering merely a fifth of India's rural population. Since then the NREGA's scale has further grown, from about 2.1 billion person-days in the fiscal year 2008-09 to 2.3 billion person-days in 2012-13. Moreover, several important design features, including mandatory bank payments and administrative processes such as linking the NREGA with India's Unique Identification Project have been added. Hence, a comprehensive analysis of the Act's welfare impacts since its inception is warranted, but the methodological challenges of such an endeavor appear to prevail.

References

Afridi, F., A. Mukhopadhyay, and S. Sahoo (2012). Female Labour Force Participation and Child Education in India: The Effect of the National Rural Employment Guarantee Scheme. IZA Discussion Paper No. 6593.

Azam, M. (2012). The Impact of Indian Job Guarantee Scheme on Labor Market Outcomes: Evidence from a Natural Experiment. IZA Discussion Paper No. 6548.

Basu, K. (1981). Food for Work Programmes: Beyond Roads That Get Washed Away. *Economic and Political Weekly* 16(1): 37–40.

Basu, K. (1991). The Elimination of Endemic Poverty in South Asia: Some Policy Options. In J. Drèze and A. Sen (Eds.), *The Political Economy of Hunger: Volume 1: Entitlement and Well-being*. Oxford University Press, Oxford.

Berg, E., S. Bhattacharyya, R. Durgam, and M. Ramachandra (2012). Can Rural Public Works Affect Agricultural Wages? Evidence from India. CSAE Working Paper WPS/2012-05.

Besley, T. and S. Coate (1992). Workfare vs. Welfare: Incentive Arguments for Work Requirements in Poverty Alleviation Programs. *American Economic Review* 82(1): 249–61.

Bose, N. (2013). *Raising Consumption through India's National Rural Employment Guarantee Scheme*. Ph. D. thesis, Vanderbilt University.

Burgess, R., R. Pande, and G. Wong (2005). Banking for The Poor: Evidence from India. *Journal of the European Economic Association* 3(2-3): 268–278.

Chaudhuri, S. and N. Gupta (2009). Levels of Living and Poverty Patterns: A District-Wise Analysis for India. *Economic and Political Weekly* 44(9): 94–110.

Chen, S. and M. Ravallion (2007). Absolute Poverty Measures for The Developing World, 1981–2004. *Proceedings of the National Academy of Sciences* 104(43): 16757–16762.

Datt, G. and M. Ravallion (1994). Transfer Benefits from Public-Works Employment: Evidence for Rural India. *The Economic Journal* 104(427): 1346–1369.

Datt, G. and M. Ravallion (2002). Why Has Economic Growth Been More Pro-Poor in Some States of India Than Others? *Journal of Development Economics* 68(2): 381–400.

Deaton, A. (2008). Price Trends in India and Their Implications for Measuring Poverty. *Economic and Political Weekly* 43(6): 43–49.

Deaton, A. and V. Kozel (2005). *The Great Indian Poverty Debate*. Macmillan.

Deininger, K. and Y. Liu (2013). Welfare and Poverty Impacts of India's National Rural Employment Guarantee Scheme: Evidence from Andhra Pradesh. World Bank Policy Research Working Paper No. 6543.

Djebbari, H. and J. Smith (2008). Heterogeneous Impacts in PROGRESA. *Journal of Econometrics* 145(1): 64–80.

Drèze, J. (1990). Famine Prevention in India. In J. Drèze and A. Sen (Eds.), *The Political Economy of Hunger: Volume 1: Entitlement and Well-being*. Oxford University Press, Oxford.

Drèze, J. and C. Oldiges (2011). Employment Guarantee: The Official Picture. In R. Khera (Ed.), *The Battle for Employment Guarantee*. Oxford University Press, New Delhi.

Duflo, E. (2003). Grandmothers and Granddaughters: Old-Age Pensions and Intrahousehold Allocation in South Africa. *The World Bank Economic Review* 17(1): 1–25.

Foster, J., J. Greer, and E. Thorbecke (1984). A Class of Decomposable Poverty Measures. *Econometrica* 52(3): 761–766.

Gaiha, R. (2000). Do Anti-Poverty Programmes Reach The Rural Poor in India? *Oxford Development Studies* 28(1): 71–95.

Government of India (2001). *Census of India 2001: Primary Census Abstract.* Office of the Registrar General, New Delhi.

Government of India (2003). *Report of the Task Force: Identification of Districts for Wage and Self Employment Programmes.* Planning Commission.

Government of India (2007). *Poverty Estimates for 2004-05.* Press Information Bureau, New Delhi.

Government of India (2009). *Report of the Expert Group to Review the Methodology for Estimation of Poverty.* Planning Commission.

Government of India (2011). *Census of India 2011: Primary Census Abstract.* Office of the Registrar General, New Delhi.

Government of India (2012). *State of Indian Agriculture 2011-12.* Ministry of Agriculture.

Government of India (2013a). Consumer Price Index for Agricultural Labourers. Online Documents: http://labourbureau.nic.in/indtab.html. Labour Bureau.

Government of India (2013b). *Mahatma Gandhi National Rural Employment Guarantee Act, 2005, Operational Guidelines, 4th Edition.* Department of Rural Development. Ministry of Rural Development.

Government of India (2013c). NREGA Districts and Data from Online Documents: http://nrega.nic.in/netnrega/home.aspx. Ministry of Rural Development .

Government of India (2014). Rainfall Data from Online Documents: http://www.imd.gov.in/section/nhac/dynamic/Monsoon_frame.htm. India Meteorological Department. Ministry of Earth Sciences.

Gupta, S. (2006). Were District Choices for NFFWP Appropriate? *Journal of Indian School of Political Economy* 18(4): 641–648.

Imbert, C. and J. Papp (2013). Labor Market Effects of Social Programs: Evidence from India's Employment Guarantee. CSAE Working Paper WPS/2013-03.

Imbert, C. and J. Papp (2014). Short-term Migration and India's Employment Guarantee. Unpublished Working Paper.

Jayne, T. S., J. Strauss, T. Yamano, and D. Molla (2002). Targeting of Food Aid in Rural Ethiopia: Chronic Need or Inertia? *Journal of Development Economics* 68(2): 247–288.

Khera, R. (2011). *The Battle for Employment Guarantee.* Oxford University Press, New Delhi.

Kochar, A. (2005). Can Targeted Food Programs Improve Nutrition? An Empirical Analysis of India's Public Distribution System. *Economic Development and Cultural Change* 54(1): 203–235.

Lagrange, A. and M. Ravallion (2012). Evaluating Workfare When the Work Is Unpleasant: Evidence for India's National Rural Employment Guarantee Scheme. World Bank Policy Research Working Paper No. 6272.

Lal, R., S. Miller, M. Lieuw-Kie-Song, and D. Kostzer (2010). Public Works and Employment Programmes: Towards a Long-Term Development Approach. Working Paper, International Policy Centre for Inclusive Growth.

Lanjouw, P. and R. Murgai (2009). Poverty Decline, Agricultural Wages, and Nonfarm Employment in Rural India: 1983–2004. *Agricultural Economics* 40(2): 243–263.

Lee, D. S. and T. Lemieux (2010). Regression Discontinuity Designs in Economics. *Journal of Economic Literature* 48(2): 281–355.

Moyo, S., G. W. Norton, J. Alwang, I. Rhinehart, and C. M. Deom (2007). Peanut Research and Poverty Reduction: Impacts of Variety Improvement to Control Peanut Viruses in Uganda. *American Journal of Agricultural Economics* 89(2): 448–460.

Murphy, K. M. and R. H. Topel (1985). Estimation and Inference in Two-Step Econometric Models. *Journal of Business and Economic Statistics* 3(4): 370–379.

National Sample Survey Organisation (2008a). *NSS Report No. 523: Household Consumer Expenditure in India, 2005-06.* Ministry of Statistics and Programme Implementation.

National Sample Survey Organisation (2008b). *NSS Report No. 527: Household Consumer Expenditure in India, 2006-07.* Ministry of Statistics and Programme Implementation.

National Sample Survey Organisation (2010). *NSS Report No. 523: Household Consumer Expenditure in India, 2007-08.* Ministry of Statistics and Programme Implementation.

Pitt, M. M. and S. R. Khandker (1998). The Impact of Group-Based Credit Programs on Poor Households in Bangladesh: Does the Gender of Participants Matter? *Journal of Political Economy* 106(5): 958–996.

Ravallion, M., S. Chen, and P. Sangraula (2007). New Evidence on the Urbanization of Global Poverty. *Population and Development Review* 33(4): 667–701.

Ravallion, M., G. Datt, and S. Chaudhuri (1993). Does Maharashtra's Employment Guarantee Scheme Guarantee Employment? Effects of the 1988 Wage Increase. *Economic Development and Cultural Change* 41(2): 251–275.

Ravi, S. and M. Engler (2009). Workfare in Low Income Countries: An Effective Way to Fight Poverty? The Case of NREGS in India. Indian School of Business Working Paper. Hyderabad: Indian School of Business.

Ravi, S., M. Kapoor, and R. Ahluwalia (2013). The Impact of NREGS on Urbanization in India. Available at SSRN 2134778.

Scandizzo, P., R. Gaiha, and K. Imai (2009). Option Values, Switches, and Wages: An Analysis of the Employment Guarantee Scheme in India. *Review of Development Economics* 13(2): 248–263.

Stecklov, G., P. Winters, M. Stampini, and B. Davis (2005). Do Conditional Cash Transfers Influence Migration? A Study Using Experimental Data from the Mexican PROGRESA Program. *Demography* 42(4): 769–790.

Subbarao, K. (2003). *Systemic Shocks and Social Protection: Role and Effectiveness of Public Works Programs.* Social Protection, World Bank.

Tarozzi, A. (2005). The Indian Public Distribution System as Provider of Food Security: Evidence from Child Nutrition in Andhra Pradesh. *European Economic Review* 49(5): 1305–1330.

Vanaik, A. and Siddhartha (2008a). Bank Payments: End of Corruption in NREGA? *Economic and Political Weekly* 43(17): 33, 35–39.

Vanaik, A. and Siddhartha (2008b). CAG Report on NREGA: Fact and Fiction. *Economic and Political Weekly* 43(25): 39–45.

World Bank (2013). *World Development Report 2014: Risk and Opportunity, Managing Risk for Development.* Washington, DC.

Zimmermann, L. (2012). Labor Market Impacts of a Large-Scale Public Works Program: Evidence from the Indian Employment Guarantee Scheme. IZA Discussion Paper No. 6858.

Figure 1: Phase-wise Rollout of the NREGA Across Districts

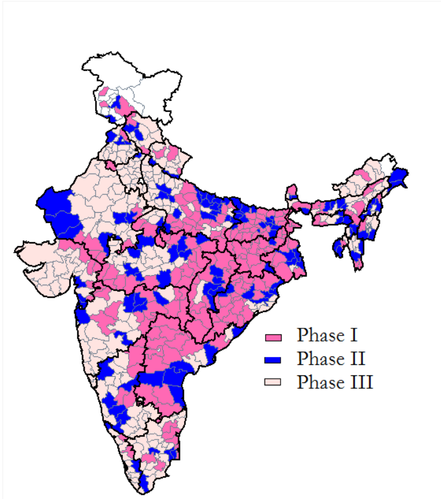
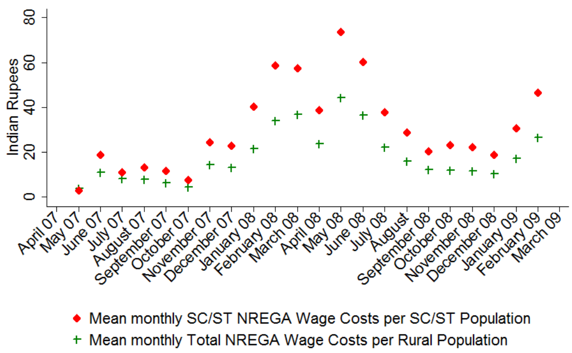


Figure 2: NREGA Wage Costs



Notes:
Based on district-wise data, downloaded from the Ministry of Rural Development website.
District-wise SC/ST Population is based on Census 2001 (Government of India, 2001).
SC/ST share is calculated from annual district-wise program participation shares.

Figure 3: Probability of NREGA Phase II Status by Centered Rank

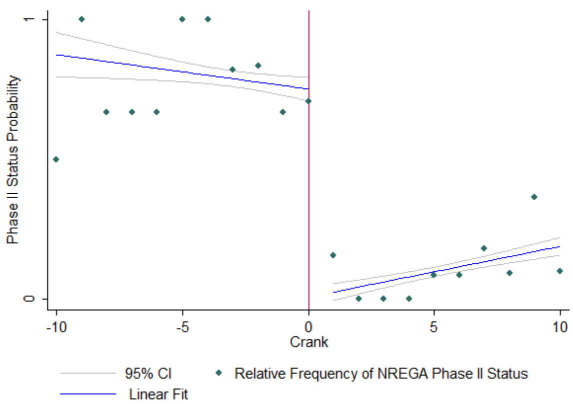


Figure 4: NREGA Effect on Mean Logarithmic Monthly per Capita Consumption Expenditures by Households Belonging to Scheduled Castes and Scheduled Tribes

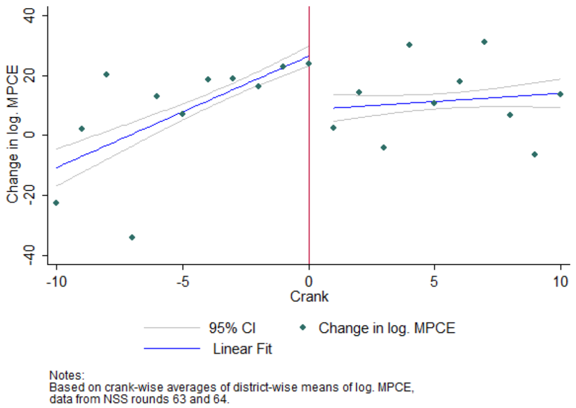


Figure 5: Estimated Effect of the NREGA on Seasonal Consumption Patterns

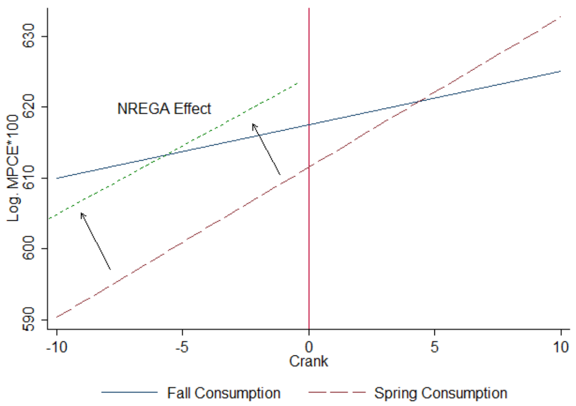


Figure 6: Mean Logarithmic Monthly per Capita Consumption Expenditures by Rural Households Belonging to Scheduled Castes and Scheduled Tribes in NREGA Phase II and III Districts and All-India Food Grain Production by Agricultural Season

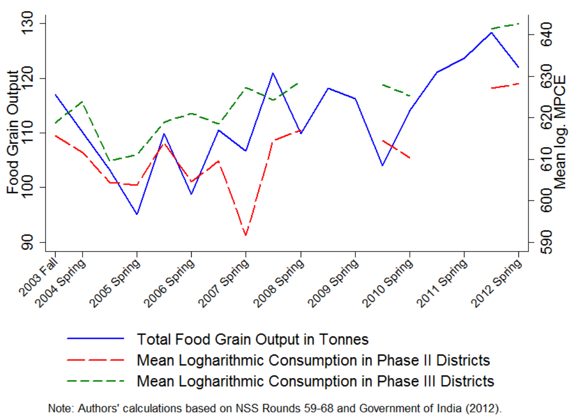


Table 1: NREGA Facts for Phase II Sample Districts

NREGA Expenditures in Phase II Sample Districts	
Total Expenditures (in million INR)	29,926.17
Expenditures on Wages (in million INR)	21,437.76
Share of Exp. on Wages in Total Exp. (in %)	71.64
Population in Phase II Sample Districts	
Rural Population (in million)	149.41
Rural Households (in million)	25.50
Rural SC/ST Population (in million)	38.74
Rural SC/ST Households (in million)	7.11
NREGA Employment in Phase II Sample Districts	
Households Employed under the NREGA (in million)	8.99
Percentage of Rural Households Employed under the NREGA	35.27
NREGA Person-Days in Phase II Sample Districts	
Total Person-Days (in million)	287.14
Person-Days per Rural Population	1.92
Person-Days per Rural Household	11.26
SC/ST Person-Days (in million)	136.05
SC/ST Person-Days per SC/ST Population	3.51
SC/ST Person-Days per SC/ST Household	19.14
Observations	92

Notes: NREGA figures pertain to the fiscal year 2007-08 (April 2007 to March 2008), are in current Indian Rupees, and are calculated from district-wise statistics published by the Ministry of Rural Development (Government of India, 2013c).
Population totals are calculated from district-wise Census 2001 figures (Government of India, 2001). Household size estimates from NSS data as set out in column 4 of the center panel in Tables 2 and 3 are used to calculate the number of rural households and SC/ST households.

Table 2: Summary Statistics for All Rural Households

	2006-07			2007-08		
	All Year	Fall	Spring	All Year	Fall	Spring
	Phase II and Phase III Sample Districts					
MPCE	643.71	651.36	636.46	670.09	672.96	667.20
Log. MPCE	631.80	632.94	630.71	637.47	636.76	638.18
HCR	35.60	35.89	35.33	29.31	30.29	28.32
PGR	7.66	7.45	7.86	5.38	5.87	4.89
Household Size	6.10	6.07	6.13	5.93	6.03	5.82
Crank	3.04	3.73	2.38	3.33	3.32	3.34
SC/ST[a] (in %)	29.34	27.89	30.71	26.37	25.60	27.15
Laborers (in %)	38.62	39.62	37.68	36.10	35.31	36.89
Observations	14,860	7,446	7,414	12,901	6,456	6,445
Districts	255	255	255	255	255	255
	Phase II Sample Districts					
MPCE	558.44	585.65	535.42	587.13	590.04	584.27
Log. MPCE	619.50	622.88	616.64	627.09	627.38	626.80
HCR	42.85	41.69	43.84	34.98	34.22	35.72
PGR	9.90	9.20	10.49	6.54	6.59	6.49
Household Size	6.20	6.06	6.32	5.86	5.89	5.83
Crank	−2.27	−1.43	−2.98	−1.91	−2.00	−1.82
SC/ST[a] (in %)	34.99	31.50	37.94	29.80	27.90	31.65
Laborers (in %)	40.69	42.81	38.90	37.17	37.26	37.09
Observations	5,703	2,856	2,847	5,450	2,728	2,722
Districts	92	92	92	92	92	92
	Phase III Sample Districts					
MPCE	711.14	698.20	724.54	728.64	730.19	727.06
Log. MPCE	641.52	640.11	642.98	644.80	643.24	646.40
HCR	29.87	31.75	27.92	25.31	27.58	22.97
PGR	5.89	6.20	5.56	4.56	5.36	3.74
Household Size	6.02	6.07	5.96	5.98	6.13	5.82
Crank	7.23	7.41	7.04	7.03	6.99	7.07
SC/ST[a] (in %)	24.87	25.32	24.41	23.95	24.01	23.89
Laborers (in %)	36.99	37.36	36.61	35.34	33.97	36.74
Observations	9,157	4,590	4,567	7,451	3,728	3,723
Districts	163	163	163	163	163	163

[a] Scheduled Castes and Scheduled Tribes.

Notes: Calculated from NSS rounds 63 and 64.
"Log. MPCE" is the natural logarithm of monthly per capita consumption expenditures, multiplied by 100.
Individual weights provided by the NSSO are used so that all figures are representative for the rural population of individuals.
"Fall" and "Spring" include observations from July to December and from January to June, respectively.
The sample is restricted to Phase II and Phase III districts for which the Planning Commission Backwardness Index is available.
All measures in 2004-05 constant prices using monthly CPI-ALs, and state-wise Tendulkar poverty lines for 2004-05.

Table 3: Summary Statistics for Rural Households Belonging to Scheduled Castes and Scheduled Tribes

	2006-07			2007-08		
	All Year	Fall	Spring	All Year	Fall	Spring
	Phase II and Phase III Sample Districts					
MPCE	496.93	511.59	484.32	547.39	542.60	551.94
Log. MPCE	610.19	614.25	606.69	621.52	619.85	623.10
HCR	52.72	49.03	55.90	42.56	43.80	41.39
PGR	13.50	12.06	14.74	8.61	9.73	7.55
Household Size	6.20	5.84	6.51	5.60	5.70	5.51
Crank	1.97	2.85	1.22	2.53	2.70	2.37
Laborers (in %)	57.13	61.17	53.65	58.64	56.36	60.81
Observations	3,579	1,785	1,794	2,724	1,352	1,372
Districts	253	239	245	252	240	238
	Phase II Sample Districts					
MPCE	446.23	494.02	412.67	517.34	511.50	522.40
Log. MPCE	599.00	609.55	591.58	615.88	614.50	617.07
HCR	61.44	50.25	69.31	46.35	46.50	46.21
PGR	17.25	13.60	19.80	9.57	10.32	8.92
Household Size	6.59	5.86	7.10	5.45	5.53	5.38
Crank	−3.34	−3.35	−3.33	−2.44	−2.48	−2.40
Laborers (in %)	53.15	55.03	51.83	56.31	52.78	59.36
Observations	1,661	829	832	1,341	643	698
Districts	92	86	90	92	91	89
	Phase III Sample Districts					
MPCE	553.33	527.17	581.40	573.77	567.54	580.19
Log. MPCE	622.64	618.42	627.17	626.47	624.14	628.87
HCR	43.02	47.94	37.74	39.24	41.63	36.78
PGR	9.33	10.69	7.87	7.77	9.25	6.25
Household Size	5.77	5.82	5.71	5.73	5.83	5.63
Crank	7.87	8.34	7.37	6.89	6.86	6.92
Laborers (in %)	61.56	66.62	56.12	60.69	59.23	62.21
Observations	1,918	956	962	1,383	709	674
Districts	161	153	155	160	149	149

Notes: See Table 2.

Table 4: Predictors of NREGA Phase II Status

Unit of observation:	**District**		**Household**					
Sample:			**All Rural Households**			**Rural SC/ST[a] Households**		
	All Year (1)	No State FE (2)	All Year (3)	Fall (4)	Spring (5)	All Year (6)	Fall (7)	Spring (8)
Positive-Crank-Dummy	0.673***	0.745***	0.670***	0.659***	0.681***	0.705***	0.788***	0.621***
	(0.089)	(0.089)	(0.104)	(0.104)	(0.105)	(0.127)	(0.112)	(0.143)
Crank	0.020*	0.018	0.023	0.020	0.025*	0.027	0.032*	0.021
	(0.011)	(0.011)	(0.014)	(0.013)	(0.014)	(0.018)	(0.016)	(0.020)
Crank*Positive-Crank-Dummy	−0.027	−0.030*	−0.020	−0.018	−0.021	−0.030	−0.028	−0.032
	(0.019)	(0.018)	(0.023)	(0.023)	(0.024)	(0.028)	(0.026)	(0.030)
Observations	201	201	10,394	5,196	5,198	2,268	1,116	1,152
Clusters			201	201	201	199	189	188
State Fixed Effects	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes

[a] Scheduled Castes and Scheduled Tribes.

Notes: Columns 3-8: Robust Standard Errors in parentheses, clustered at the district level. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Based on the 64th NSS round, 2007-08, individual weights provided by the NSSO.

The sample is trimmed to Phase II and Phase III districts whose crank is not smaller than -10 and not greater than 10.

Phase II status by PC score rule (Dummy) equals one if the district's centered rank is equal to or smaller than zero and zero otherwise.

Postive-Crank-Dummy equals one if the district's centered rank is greater than zero and zero otherwise.

Table 5: Regression Discontinuity Analysis of Consumption and Poverty

	MPCE	HCR	PGR	MPCE	HCR	PGR	MPCE	HCR	PGR
	Sample: All Rural Households								
	All Year			**Fall**			**Spring**		
Year0708 * $\widehat{Phase}2$	0.586	−6.964	−0.935	−2.305	−6.328	0.711	2.584	−8.130	−1.887
	(5.547)	(6.414)	(1.668)	(6.521)	(7.640)	(1.974)	(7.658)	(7.993)	(2.028)
Observations	22,929	22,929	22,929	11,435	11,435	11,435	11,494	11,494	11,494
Clusters	401	401	401	401	401	401	401	401	401
Dist. in both years	200	200	200	200	200	200	200	200	200
	Sample: Rural Households Belonging to Scheduled Castes and Scheduled Tribes								
	All Year			**Fall**			**Spring**		
Year0708 * $\widehat{Phase}2$	23.380***	−29.102***	−6.798**	−4.963	−3.774	1.581	37.283***	−45.196***	−11.678***
	(6.888)	(10.814)	(2.918)	(9.803)	(13.342)	(4.504)	(10.156)	(16.153)	(3.838)
Observations	5,285	5,285	5,285	2,607	2,607	2,607	2,678	2,678	2,678
Clusters	398	398	398	378	378	378	382	382	382
Dist. in both years	197	197	197	178	178	178	181	181	181
	Sample: Rural Laborers								
	All Year			**Fall**			**Spring**		
Year0708 * $\widehat{Phase}2$	6.518	−1.532	0.591	6.805	−8.087	2.317	9.740	−0.319	−2.187
	(6.399)	(8.720)	(2.691)	(6.900)	(9.536)	(2.820)	(8.694)	(12.843)	(3.414)
Observations	6,881	6,881	6,881	3,379	3,379	3,379	3,502	3,502	3,502
Clusters	400	400	400	393	393	393	389	389	389
Dist. in both years	199	199	199	192	192	192	189	189	189

Notes: Robust standard errors (Murphy Topel) in parentheses, clustered at the district-year level. * p<0.1, ** p<0.05, *** p<0.01
Additional regressors whose coefficients are not displayed in the table: District fixed effects, state-year interactions, a crank-year 2007-08 interaction, and crank-year 2007-08 above the crank cutoff (Dummy) interaction. Weights: Individual weights as provided by the NSSO.
Data: NSS rounds 63 and 64 for the years 2006-07 and 2007-08, sample trimmed to Phase II and Phase III districts with a crank no smaller than -10 and no greater than 10.
"Fall" and "Spring" include observations from July to December and from January to June, respectively.

Table 6: Second Stage Stratification by NSS Round

				Relative Frequency of Second Stage Sub-Stratum (SSS)					Oversampling	
Year (1)	NSS Round (2)	SSS[a] (3)	Type (4)	As per NSS (%) (5)	All-India Rural NSS Sample (6)	All-India Rural Population (%)[b] (7)	Rural NSS Sample Used in Estimations (%) (8)	Population in Districts Used in Estimations (%)[b] (9)	All-India Rural NSS Sample (10)	Rural NSS Sample Used in Estimations (11)
2005-06	62	1	Land-poor	50	54	84	53	84	0.6	0.6
2005-06	62	2	Land-rich	50	46	16	47	16	2.9	2.9
2006-07	63	1	Public works employment	33.3	19	12	13	8	1.6	1.6
2006-07	63	2	Of the rest, land-poor	33.3	51	75	56	79	0.7	0.7
2006-07	63	3	Of the rest, land-rich	33.3	30	13	31	14	2.3	2.2
2007-08	64	1	Affluent	50	48	8	49	8	6.0	6.1
2007-08	64	2	Non-affluent	50	52	92	51	92	0.6	0.6

[a] Second Stage Stratum, [b] Calculated from sampling weights provided by the NSSO.
"As per NSS": Sampling proportions for each surveyed village according to NSS instructions.
Columns 8 and 9 refer to the sample used in the estimations (Phase II and III districts for which -10 $\leq crank \leq$ 10).
Column 10 is the ratio of column 6 and 7, and column 11 is the ratio of column 8 and 9.
Sources: National Sample Survey Organisation (2008a,b, 2010) and own calculations.

Table 7: Regression Discontinuity Analysis of Consumption and Poverty, Placebo

	MPCE	HCR	PGR	MPCE	HCR	PGR	MPCE	HCR	PGR
	Sample: All Rural Households								
	All Year			**Fall**			**Spring**		
Year0607 * $\widehat{Phase2}$	−1.223	1.607	0.417	−2.905	8.186	1.331	−0.529	−3.259	0.925
	(5.692)	(6.471)	(1.666)	(7.033)	(7.609)	(2.062)	(8.931)	(9.713)	(2.666)
Observations	19,381	19,381	19,381	9,630	9,630	9,630	9,751	9,751	9,751
Clusters	402	402	402	400	400	400	396	396	396
Dist. in both years	201	201	201	199	199	199	195	195	195
	Sample: Rural Households Belonging to Scheduled Castes and Scheduled Tribes								
	All Year			**Fall**			**Spring**		
Year0607 * $\widehat{Phase2}$	−12.257	14.537	3.697	−21.159	24.209	7.663	−5.170	0.298	0.215
	(11.011)	(12.088)	(4.060)	(13.201)	(17.533)	(4.975)	(14.231)	(18.386)	(4.960)
Observations	4,405	4,405	4,405	2,163	2,163	2,163	2,242	2,242	2,242
Clusters	396	396	396	367	367	367	367	367	367
Dist. in both years	195	195	195	171	171	171	169	169	169
	Sample: Rural Laborers								
	All Year			**Fall**			**Spring**		
Year0607 * $\widehat{Phase2}$	4.226	−13.500	−1.955	−1.228	−5.681	3.805	0.856	−5.912	0.085
	(6.908)	(8.453)	(2.739)	(8.198)	(11.998)	(3.397)	(11.513)	(13.452)	(4.696)
Observations	6,275	6,275	6,275	3,113	3,113	3,113	3,162	3,162	3,162
Clusters	396	396	396	375	375	375	377	377	377
Dist. in both years	195	195	195	176	176	176	177	177	177

Notes: See Table 5, except for data: NSS rounds 62 and 63 for the years 2005-06 and 2006-07, respectively.

Table 8: Two Stage Least Squares Regression Discontinuity Analysis of Consumption and Poverty

	MPCE	HCR	PGR	MPCE	HCR	PGR	MPCE	HCR	PGR
	Sample: All Rural Households								
	All Year			**Fall**			**Spring**		
Year0708 * $\widehat{Phase2}$	0.665	−7.285	−0.986	−2.396	−7.018	0.730	2.572	−8.117	−1.872
	(5.766)	(6.886)	(1.748)	(7.084)	(8.561)	(2.146)	(7.686)	(8.156)	(2.059)
Observations	22,929	22,929	22,929	11,435	11,435	11,435	11,494	11,494	11,494
Clusters	401	401	401	401	401	401	401	401	401
Dist. in both years	200	200	200	200	200	200	200	200	200
	Sample: Rural Households Belonging to Scheduled Castes and Scheduled Tribes								
	All Year			**Fall**			**Spring**		
Year0708 * $\widehat{Phase2}$	24.631***	−30.744**	−7.159**	−5.105	−3.972	1.617	40.969***	−49.664**	−12.833**
	(8.307)	(13.035)	(3.206)	(10.146)	(13.648)	(4.670)	(13.750)	(20.825)	(5.054)
Observations	5,285	5,285	5,285	2,607	2,607	2,607	2,678	2,678	2,678
Clusters	398	398	398	378	378	378	382	382	382
Dist. in both years	197	197	197	178	178	178	181	181	181

Notes: See Table 5.

Table 9: Regression Discontinuity Analysis of Consumption and Poverty, Equal District Weights

	MPCE	HCR	PGR	MPCE	HCR	PGR	MPCE	HCR	PGR
	Sample: All Rural Households								
	All Year			**Fall**			**Spring**		
Year0708 * $\widehat{Phase}2$	0.449	−3.287	−0.072	3.879	−5.016	−0.417	−2.990	−1.567	0.258
	(4.310)	(3.314)	(0.839)	(5.843)	(4.239)	(1.120)	(5.546)	(4.270)	(1.035)
Observations	22,929	22,929	22,929	11,435	11,435	11,435	11,494	11,494	11,494
Clusters	401	401	401	401	401	401	401	401	401
Dist. in both 2006-07 and 2007-08	200	200	200	200	200	200	200	200	200
	Sample: Rural Households Belonging to Scheduled Castes and Scheduled Tribes								
	All Year			**Fall**			**Spring**		
Year0708 * $\widehat{Phase}2$	8.925	−7.694	−1.797	−2.802	1.017	1.505	16.838**	−9.791	−2.661
	(5.791)	(7.330)	(1.743)	(9.095)	(9.712)	(2.666)	(7.890)	(10.533)	(2.404)
Observations	5,285	5,285	5,285	2,607	2,607	2,607	2,678	2,678	2,678
Clusters	398	398	398	378	378	378	382	382	382
Dist. in both 2006-07 and 2007-08	197	197	197	178	178	178	181	181	181

Notes: See Table 5, but instead of individual weights each district in each of the years is given the same weight.

Table 10: Cross-sectional Regression Discontinuity Analysis of Consumption and Poverty

	MPCE	HCR	PGR	MPCE	HCR	PGR	MPCE	HCR	PGR
	Sample: All Rural Households								
	All Year			**Fall**			**Spring**		
Year0708 * $\widehat{Phase}2$	−8.032	3.587	0.151	−9.914	7.794	0.361	−6.070	−0.457	−0.090
	(7.149)	(6.404)	(1.572)	(8.101)	(7.698)	(1.910)	(7.823)	(7.850)	(1.803)
Observations	10,394	10,394	10,394	5,196	5,196	5,196	5,198	5,198	5,198
Clusters	201	201	201	201	201	201	201	201	201
Districts in 2007-08	201	201	201	201	201	201	201	201	201
	Sample: Rural Households Belonging to Scheduled Castes and Scheduled Tribes								
	All Year			**Fall**			**Spring**		
Year0708 * $\widehat{Phase}2$	1.630	−3.477	−0.089	−12.106	17.156	3.894	14.732*	−22.455	−4.022
	(7.458)	(10.646)	(2.696)	(9.958)	(13.090)	(3.302)	(8.267)	(13.661)	(3.149)
Observations	2,268	2,268	2,268	1,116	1,116	1,116	1,152	1,152	1,152
Clusters	199	199	199	189	189	189	188	188	188
Districts in 2007-08	199	199	199	189	189	189	188	188	188

Notes: Robust standard errors in parentheses, clustered at the district-year level. * p<0.1, ** p<0.05, *** p<0.01

All measures in 2004-05 constant prices using monthly CPI-ALs, state-wise Tendulkar poverty lines for 2004-05.

Additional regressors whose coefficients are not displayed in the table:

State dummies, crank, and crank-above the crank cutoff (Dummy) interaction.

Weights: Individual weights as provided by the NSSO.

Data: NSS round 64 for the year 2007-08, sample trimmed to Phase II and Phase III districts with a crank no smaller than -10 and no greater than10.

"Fall" and "Spring" include observations from July to December and from January to June, respectively.

Table 11: Regression Discontinuity Analysis of the Incidence of Scheduled Castes and Scheduled Tribes and Laborers

Dependent Var.:	SC/ST[a] (1)	Laborer (2)	SC/ST[a] (3)	Laborer (4)	SC/ST[a] (5)	Laborer (6)
	Sample: All Rural Households in 2006-07 and 2007-08					
	All Year		**Fall**		**Spring**	
Year0708 * $\widehat{Phase}2$	−0.128***	0.084*	−0.151**	0.131*	−0.072	0.054
	(0.046)	(0.043)	(0.065)	(0.067)	(0.063)	(0.066)
Observations	22,928	22,920	11,435	11,432	11,493	11,488
Clusters	401	401	401	401	401	401
Dist. in both years	200	200	200	200	200	200
	Sample: All Rural Households in 2005-06 and 2007-08					
	All Year		**Fall**		**Spring**	
Year0608 * $\widehat{Phase}2$	−0.036	0.073	−0.051	0.101	−0.010	0.089
	(0.053)	(0.050)	(0.079)	(0.063)	(0.071)	(0.083)
Observations	17,206	17,202	8,569	8,567	8,637	8,635
Clusters	402	402	400	400	396	396
Dist. in both years	201	201	199	199	195	195

[a] Scheduled Castes and Scheduled Tribes.

Notes: See Table 5.

Table 12: Regression Discontinuity Analysis of Consumption and Poverty, Alternative Base Year

	MPCE	HCR	PGR	MPCE	HCR	PGR	MPCE	HCR	PGR
	Sample: All Rural Households								
	All Year			**Fall**			**Spring**		
Year0608 * $\widehat{Phase2}$	−1.956	−3.795	−0.042	−8.612	6.995	2.135	6.390	−13.201	−1.765
	(5.672)	(6.119)	(1.682)	(6.942)	(7.707)	(2.000)	(7.463)	(8.562)	(2.322)
Observations	17,208	17,208	17,208	8,571	8,571	8,571	8,637	8,637	8,637
Clusters	402	402	402	400	400	400	396	396	396
Dist. in both years	201	201	201	199	199	199	195	195	195
	Sample: Rural Households Belonging to Scheduled Castes and Scheduled Tribes								
	All Year			**Fall**			**Spring**		
Year0608 * $\widehat{Phase2}$	11.207	−20.943*	−2.605	−15.987	19.514	7.382	38.575***	−52.924***	−14.811***
	(10.024)	(12.092)	(3.890)	(15.118)	(16.543)	(5.297)	(13.440)	(17.008)	(5.262)
Observations	3,647	3,647	3,647	1,782	1,782	1,782	1,865	1,865	1,865
Clusters	395	395	395	366	366	366	360	360	360
Dist. in both years	194	194	194	168	168	168	164	164	164

Notes: See Table 5, except for data: NSS rounds 62 and 64 for the years 2005-06 and 2007-08, respectively.

Table 13: Regression Discontinuity Analysis of Consumption and Poverty for Rural Households Belonging to Scheduled Castes and Scheduled Tribes during Spring, Alternative Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
			Specifications for MPCE			
Year0708 * $\widehat{Phase2}$	40.373***	30.641***	48.481***	22.982***	30.700***	29.022**
	(14.805)	(9.989)	(9.184)	(8.352)	(9.015)	(11.488)
Observations	1,992	3,045	2,678	2,678	2,678	1,406
Clusters	253	456	382	382	382	341
			Specifications for HCR			
Year0708 * $\widehat{Phase2}$	−33.340	−44.654***	−63.720***	−26.239*	−36.297**	−12.477
	(23.850)	(15.543)	(14.887)	(13.999)	(15.256)	(13.447)
Observations	1,992	3,045	2,678	2,678	2,678	1,406
Clusters	253	456	382	382	382	341
			Specifications for PGR			
Year0708 * $\widehat{Phase2}$	−8.148	−11.747***	−16.900***	−6.833**	−9.631***	−3.585
	(6.338)	(3.714)	(3.679)	(3.393)	(3.673)	(3.356)
Observations	1,992	3,045	2,678	2,678	2,678	1,406
Clusters	253	456	382	382	382	341
Trimming	±5	±15	±10	±10	±10	±10
Stage 1 State FE, Stage 2 State-Year Interactions	*Yes*	*Yes*	*Yes*	*No*	*Yes*	*Yes*
Polynomial Order	1	1	2	1	1	1
Household Size	*No*	*No*	*No*	*No*	*Yes*	*No*
January - March only	*No*	*No*	*No*	*No*	*No*	*Yes*

Notes: See Table 5. Household Size is a categorical variable. The seven categories are 1-2, 3, 4, 5, 6, 7, and 8 or more members.

Appendix

Table A1: Differences-in-Differences Analysis of Consumption and Poverty

	MPCE	HCR	PGR	MPCE	HCR	PGR	MPCE	HCR	PGR
	Sample: All Rural Households								
	All Year			**Fall**			**Spring**		
Year0708 * Phase2	1.034	−3.877	−1.546**	2.071	−3.400	−1.683*	−0.715	−3.881	−0.967
	(2.388)	(2.747)	(0.758)	(3.268)	(3.386)	(0.950)	(2.778)	(3.284)	(0.829)
Observations	22,929	22,929	22,929	11,435	11,435	11,435	11,494	11,494	11,494
Clusters	401	401	401	401	401	401	401	401	401
Dist. in both years	200	200	200	200	200	200	200	200	200
	Sample: Rural Households Belonging to Scheduled Castes and Scheduled Tribes								
	All Year			**Fall**			**Spring**		
Year0708 * Phase2	10.110***	−9.690**	−4.943***	6.363	−5.527	−4.944***	8.405	−9.385	−2.543
	(3.210)	(4.769)	(1.310)	(4.012)	(5.296)	(1.817)	(5.173)	(7.134)	(1.679)
Observations	5,285	5,285	5,285	2,607	2,607	2,607	2,678	2,678	2,678
Clusters	398	398	398	378	378	378	382	382	382
Dist. in both years	197	197	197	178	178	178	181	181	181

Notes: See Table 5.

Table A2: Summary Statistics for Rural Households whose Principal Occupation is Labor

	2006-07			2007-08		
	All Year	Fall	Spring	All Year	Fall	Spring
	Phase II and Phase III Sample Districts					
MPCE	513.49	517.71	509.28	549.31	540.36	557.95
Log. MPCE	612.34	613.87	610.82	621.66	620.45	622.81
HCR	51.41	52.09	50.72	42.83	43.75	41.94
PGR	12.33	11.99	12.67	8.47	8.89	8.06
Household Size	5.59	5.75	5.43	5.37	5.43	5.33
Crank	2.91	3.86	1.96	3.03	3.12	2.94
SC/ST[a] (in %)	43.40	43.06	43.74	42.84	40.86	44.75
Observations	5,299	2,648	2,651	2,898	1,405	1,493
Districts	255	252	252	254	249	246
	Phase II Sample Districts					
MPCE	439.18	451.64	427.59	495.05	505.88	484.38
Log. MPCE	599.67	603.10	596.48	612.67	614.90	610.48
HCR	59.39	57.64	61.01	49.31	45.60	52.96
PGR	15.59	14.28	16.81	10.12	9.43	10.80
Household Size	5.64	5.91	5.39	5.35	5.35	5.34
Crank	−2.43	−1.65	−3.17	−2.12	−1.85	−2.40
SC/ST[a] (in %)	45.70	40.50	50.55	45.13	39.53	50.66
Observations	2,050	1,039	1,011	1,254	616	638
Districts	92	89	91	92	91	89
	Phase III Sample Districts					
MPCE	578.14	571.68	584.96	589.60	566.46	611.55
Log. MPCE	623.36	622.66	624.10	628.32	624.66	631.80
HCR	44.46	47.55	41.19	38.02	42.36	33.90
PGR	9.50	10.12	8.84	7.25	8.49	6.07
Household Size	5.54	5.61	5.47	5.40	5.48	5.31
Crank	7.56	8.36	6.71	6.85	6.89	6.82
SC/ST[a] (in %)	41.40	45.16	37.43	41.14	41.86	40.45
Observations	3,249	1,609	1,640	1,644	789	855
Districts	163	163	161	162	158	157

[a] Scheduled Castes and Scheduled Tribes

Notes: See Table 2.

Table A3: Differences-in-Differences Analysis of Consumption and Poverty, Placebo

	MPCE	HCR	PGR	MPCE	HCR	PGR	MPCE	HCR	PGR
	Sample: All Rural Households								
	All Year			**Fall**			**Spring**		
Year0607 * Phase2	0.428	1.582	−0.190	−5.091	8.876**	2.309**	6.168	−3.823	−2.143
	(2.360)	(2.942)	(0.829)	(3.522)	(3.795)	(1.053)	(4.698)	(4.913)	(1.456)
Observations	19,381	19,381	19,381	9,630	9,630	9,630	9,751	9,751	9,751
Clusters	402	402	402	400	400	400	396	396	396
Dist. in both years	201	201	201	199	199	199	195	195	195
	Sample: Rural Households Belonging to Scheduled Castes and Scheduled Tribes								
	All Year			**Fall**			**Spring**		
Year0607 * Phase2	3.907	−5.525	−0.591	0.948	−3.975	1.625	13.579**	−15.010**	−5.911**
	(4.532)	(6.033)	(1.747)	(6.131)	(9.535)	(2.333)	(5.843)	(7.183)	(2.395)
Observations	4,405	4,405	4,405	2,163	2,163	2,163	2,242	2,242	2,242
Clusters	396	396	396	367	367	367	367	367	367
Dist. in both years	195	195	195	171	171	171	169	169	169

Notes: See Table 7.

Table A4: Two Stage Least Squares Regression Discontinuity Analysis of Consumption and Poverty, Placebo

	MPCE	HCR	PGR	MPCE	HCR	PGR	MPCE	HCR	PGR
	Sample: All Rural Households								
	All Year			**Fall**			**Spring**		
Year0607 * $\widehat{Phase2}$	−1.279	1.681	0.436	−2.999	8.453	1.374	−0.563	−3.472	0.985
	(5.940)	(6.760)	(1.743)	(7.176)	(7.654)	(2.093)	(9.515)	(10.359)	(2.857)
Observations	19,381	19,381	19,381	9,630	9,630	9,630	9,751	9,751	9,751
Clusters	402	402	402	400	400	400	396	396	396
Dist. in both years	201	201	201	199	199	199	195	195	195
	Sample: Rural Households Belonging to Scheduled Castes and Scheduled Tribes								
	All Year			**Fall**			**Spring**		
Year0607 * $\widehat{Phase2}$	−15.176	17.999	4.577	−20.931	23.949	7.580	−8.248	0.475	0.342
	(13.834)	(15.553)	(4.996)	(13.007)	(17.398)	(4.774)	(23.125)	(29.353)	(7.928)
Observations	4,405	4,405	4,405	2,163	2,163	2,163	2,242	2,242	2,242
Clusters	396	396	396	367	367	367	367	367	367
Dist. in both years	195	195	195	171	171	171	169	169	169

Notes: See Table 5, except for data: NSS rounds 62 and 63 for the years 2005-06 and 2006-07, respectively.

Table A5: Sample Means for Various Years of NSS Consumption Surveys, Rural Households Belonging to Scheduled Castes and Scheduled Tribes

	Phase II			Phase III		
	All Year	Fall	Spring	All Year	Fall	Spring
	2003-04 (59th/60th Round)			**2003-04 (59th/60th Round)**		
MPCE	511.49	529.96	500.53	562.68	536.43	577.47
Log. MPCE	613.16	615.65	611.67	621.96	618.66	623.83
HCR	48.20	44.19	50.58	44.95	48.40	43.00
PGR	11.29	10.26	11.90	9.31	11.12	8.29
Observations	1,641	756	885	1,677	753	924
	2004-05 (61st Round)			**2004-05 (61st Round)**		
MPCE	462.22	461.45	463.00	489.74	488.41	491.07
Log. MPCE	604.07	604.46	603.68	610.33	609.48	611.18
HCR	57.69	57.72	57.67	56.80	57.64	55.95
PGR	14.32	14.12	14.52	13.49	14.09	12.88
Observations	3,556	1,807	1,749	4,390	2,168	2,222
	2005-06 (62nd Round)			**2005-06 (62nd Round)**		
MPCE	487.40	507.22	469.71	536.51	532.65	540.39
Log. MPCE	608.94	613.91	604.50	619.95	618.85	621.06
HCR	53.36	46.21	59.75	44.46	46.98	41.93
PGR	13.55	10.82	15.98	10.19	11.28	9.10
Observations	697	333	364	934	447	487
	2006-07 (63rd Round)			**2006-07 (63rd Round)**		
MPCE	446.23	494.02	412.67	553.33	526.26	582.13
Log. MPCE	599.00	609.55	591.58	622.64	618.25	627.31
HCR	61.44	50.25	69.31	43.02	48.14	37.57
PGR	17.25	13.60	19.80	9.33	10.73	7.84
Observations	1,661	829	832	1,918	947	971
	2007-08 (64th Round)			**2007-08 (64th Round)**		
MPCE	517.34	511.50	522.40	573.77	567.20	580.53
Log. MPCE	615.88	614.50	617.07	626.47	624.09	628.91
HCR	46.35	46.50	46.21	39.24	41.67	36.75
PGR	9.57	10.32	8.92	7.77	9.25	6.25
Observations	1,341	643	698	1,383	707	676
	2009-10 (66th Round)			**2009-10 (66th Round)**		
MPCE	506.14	519.56	492.04	576.50	588.12	564.10
Log. MPCE	612.47	614.55	610.30	626.66	627.98	625.25
HCR	50.20	50.12	50.29	38.05	38.09	38.00
PGR	11.87	11.26	12.52	7.49	7.56	7.42
Observations	2,677	1,295	1,382	3,490	1,795	1,695
	2011-12 (68th Round)			**2011-12 (68th Round)**		
MPCE	590.03	590.71	589.34	682.28	680.68	683.64
Log. MPCE	627.65	627.07	628.22	642.05	641.40	642.61
HCR	33.21	34.46	31.97	22.69	23.38	22.10
PGR	6.91	7.88	5.96	4.01	4.49	3.61
Observations	2,462	1,206	1,256	3,128	1,521	1,607

See Table 2, except for data.