Workfare in Low Income Countries: An Effective Way to Fight Poverty? The Case of India's NREGS*

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July 16, 2009

Abstract

This paper analyses the impact of India's National Rural Employment Guarantee Scheme (NREGS) on rural households. In particular, we look at the impact of the program on food security, savings and health outcomes. We have an initial baseline survey data of 1066 households (June 2007) and a subsequent panel data of 320 households (December 2008) from Andhra Pradesh. We have estimates based on propensity score matching, double difference and triple difference to account for non-random attrition. We find that NREGS significantly increases expenditure on food by 40 percent (Rs. 96.6) and non-food consumables by 69 percent (Rs.16.3). The program improves probability of holding savings by 9 percent and reduces incidence of depression by 8 percent. The impact on food security is stronger for the poorest group.

*Please send comments to: shamika_ravi@isb.edu. We thank Shubhashis Gangopadhyay, Montek Singh Ahluwalia, Malcolm Harper, Jonathan Morduch, Mudit Kapoor, Reetika Khera and Nachiket Mor for comments. Ravi acknowldges support from Ford Foundation and MFMI, DC.

1 Introduction

While developed countries increasingly lean on workfare programs as a means to reduce work disincentives provoked by their far-reaching social security systems, the concept of cash-for-work has gained importance in less advanced countries as well. Looking back to a long history of food-for-work programs in times of economic distress, developing countries increasingly run public works programs not only to better target benefits to the poor but also to use the emerging labor force to build up the rural economic infrastructure. In the fore of that development is India where the ambitious National Rural Employment Guarantee Scheme (NREGS) came into force in 2006. Since then, each rural household is guaranteed access to a 100 days of unskilled wage employment per year. Common assignments include manual work in small-scale road construction, water supply, flood protection, irrigation infrastructure, land development and reforestation projects (MRD, 2006). Seeking a high labor intensity, the NREGS stipulates that at least 60 percent of total expenditures have to be spent on wages of unskilled workers and only 40 percent for skilled employees and material.

With a budget of almost 4 billion USD[1] or 2.3 percent of total central government spending, the program is by far the best endowed anti-poverty program in India (MRD, 2008a). The headcount of the program is thus impressive: Between April 2007 and March 2008, 33.7 million households – every fourth household in rural India – worked under the program and completed 1.4 billion working days (MRD, 2008b). Given this scale, the NREGS ranks among the major workfare initiatives worldwide. For example, it bears comparison with the major American workfare program, the

[1] 160 billion rupees at April 1, 2008, the beginning of the Indian financial year.

Earned Income Tax Credit (EITC) scheme which, although equipped with the tenfold budget of 40 billion USD, reaches not more than two thirds of NREGS's coverage in terms of participating households (Goodman-Bacon and McGranahan, 2008). However, unlike the famous pendant, the NREGS has received little attention in research so far.

While the merits of work requirements to direct transfers to most needy are widely recognized since the nonpoor are less likely to accept low wages and heavy labor to obtain benefits (e.g. Dev (1995), Mujeri (2002), Webb (1995), Haddad and Adato (2001)), the usefulness of the NREGS is in no case uncontested. Doubts mainly arise from the NREGS wages which quite often seem attractive relative to market wages and therefore bring the risk of upward wage pressures. In the case that they translate into price increases, there is obviously little change in the real situation of the involved. Recent inflation hikes in rural areas from less than 1 percent in 2000-01 to more than 9 percent in 2007-08 (Labour Bureau, 2008) nurture this concern. Beyond that, media reports on labor shortages raised fears that the NREGS crowds out private employment.[2] Work-for-benefit programs, as a matter of principle, come at the price of a lower participation of the poor in the private labor market and of more people to be supported to reach an aspired welfare level (Besley and Coate, 1992). If, in addition, attractive workfare salaries drive up the general wage level, the private labor is curbed also from the demand side and the shift from regular to public employment intensifies. This might overstrain not only the budgetary but also the job-creating capacity of the state, finally undermining the work guarantee of the scheme.

[2]See, e.g., "Rural employment scheme leads to labour shortage, higher wages", published August 4, 2008, on www.livemint.com, "Labour crisis hits farm sector in Cuddalore", published August 1, 2008, on www.thehindu.com, "Productivity Is The Key", published April 11, 2008, on www.businessworld.in.

This paper measures the welfare impact of the NREGS on the participating households. In so doing, it aims to provide necessary hard facts for India's policy makers as well as to add to the still modest empirical literature on the impact of workfare schemes in low-income countries (see Devereux and Solomon (2006) and Subbarao (1997, 2003) for overviews). Particularly, it goes beyond the bias-prone simple participant-nonparticipant comparisons of outcome measures many previous studies had to rely on due to limited data. As Ravallion and Datt (1995) pointed out, if the program did not exist, workfare participants, would not just be idle, but realize some income including outputs from subsistence production. Therefore the outcomes of interest of participants have to be corrected by forgone outcomes (Ravallion and Datt, 1995; Haddad and Adato, 2001), or the performance of the participants has to be compared with that of a similar group of nonparticipants (Jalan and Ravallion, 1999; Galasso and Ravallion, 2004).

Our analysis starts by using propensity score matching to draw groups of comparable nonparticipants and participants. Making use of panel data, we then continue with difference-in-difference methods and contrast different groups of households. We coompare households which stay within the NREGS for consecutive years with those that leave the program and we also compare with households that join later. To sharpen the comparison and over come the self selection problem, we use groups of households which demanded NREGS jobs but some who got them and others who didn't because there were no more projects. These refinements allow us to accurately measure the effect of the program. Another question of interest is: whom does the NREGS attract? That is, does it attract households with good chances of finding regular employment or does it act as temporary unemployment insurance? If households resort to the NREGS only when no other work

is available, example during lean agriculture season, the scheme's interference with the private labor market can be expected to be low even if the wages within NREGS are relatively high.

Our main results indicate that the NREGS improves food security and reduces anxiety levels amongst participating households. We have looked at various sub-categories of monthly per capita consumption expenditure. We find that NREGS significantly increases monthly p.c. expenditure on non-food consumables (Rs.11.4) and clothing (Rs. 11.2) but our results are strongest for food with a significant increase in monthly p.c expenditure of Rs.35.4. The increase in food expenditure amounts to 7 percent of pre-intervention expenditure level for all participants combined and 15 percent for the most poor group. Despite these benefits, however, it does not seem to be the case that the NREGS retains its participants on a permanent basis. On the contrary, the high share of relatively successful households in the program drop-outs indicates that participants choose private employment given the opportunity. When we compare involuntary non-participants i.e. households that demanded work but couldn't get them because of external reasons, to households which demanded and got employment, we find that the latter has a massive increase in food expenditure amounting to approximately 36 percent. Finally, we also study the physical and mental health outcomes and find that NREGS significantly lowers emotional distress and anxiety by 25 percent.

We have collected attitudinal data of households towards the NREGS which is helpful to understand how the program is commonly perceived. We find that 96 percent of all households think that this is a useful program. When asked about the basic merit of this program, 67 percent reported that it provided employment security when no other work was available while 12 percent felt that

it increased their household income as additional members got employment. The main reported impediment in the program seems to be delay in payments.

The paper is structured as follows. We sketch out the nature of India's poverty and the rationale of the NREGS in the next section and introduce our identification strategy in Section 3. Section 4 explains the data and Section 5 presents the results. Section 6 summarizes and concludes.

2 India's Poverty and the Rationale for the NREGS

If India's official poverty line of a monthly per capita consumption of 356 rupees or 9 USD in rural and 593 rupees in urban areas is taken as a benchmark, 300 million Indians are poor. In absolute numbers this is hardly less than thirty years ago, yet the share in total population has dropped steadily from 55% in the early seventies to 28% in 2005. What has not changed is, however, the deep-rootedness of poverty in rural areas where three quarter of the poor live (Planning Commission, 2008a). Their poverty is reflected in every aspect of life. For example, while among the rural households only 56% have electricity and 25% toilet facilities, the respective numbers for urban households are 93% and 85%. Primary school attendance is 81% in rural and 88% in urban areas, with the difference increasing for higher school levels. The birthrate is 3.0 children per women in villages and 2.1 in cities. Infant mortality is 50% higher in the country and undernourishment hits 41% of rural women vs. 25% of urban women (IIPS, 2007).

The main reason for the great poverty in rural areas lies in the still largely agrarian economy. Of the rural poor, 41% are informal agricultural laborers and 22% are self-employed farmers. This also affects urban poverty as most poor households in the cities are distress migrants from rural

areas with stagnating farming incomes (Planning Commission, 2008a). The one-sided orientation on farming implies dependence on a sector where productivity falls more and more behind the rest of the economy. Today, agriculture absorbs 52% of India's labor force but contributes less then one fifth to the country's GDP. Although recent growth rates of the sector compared favorably to its performance in the past, the average GDP growth per worker of 2.2% between 1994 and 2005 clearly lagged behind the economy-wide 4.4% in the same period (Planning Commission, 2008b).

One main reason for low productivity of agriculture lies in the excess of (underemployed) farming labor. People have to resort to agricultural work because there is no alternative, especially in the form of labor-intensive, low- and semi-skilled manufacturing jobs. Despite unprecedented growth rates of almost 9% in recent years, the manufacturing sector has not generated enough employment opportunities to absorb the underemployed workforce surplus so that its share in total employment still reaches not even 20% (Ministry of Finance, 2008). Meanwhile, the hiring of the agricultural sector has slowed down considerably, from 1.8% annually between 1983 and 1994 to 0.4% between 1994 and 2005 (Planning Commission, 2008b).

As the workforce continues to grow, the earning situation becomes more acute not only for the landless, who always lacked the possibility of subsistence farming, but gets worse also for those owning land. Their available land plots decrease from generation to generation, the share of landowning farmers holding less than 1 hectare of land having risen from 56% in 1982 to 70% today (Planning Commission, 2008a). As a consequence, the landowners, in order to supplement their incomes, increasingly join the landless in seeking wage labor. Since the low-skilled are widely barred from the formal sector (which accounts for only 14% of total employment), this intensifies

the competition for informal jobs, thereby weakening the bargaining power of the job seekers and abetting precarious employment without steady and reliable income flows or basic labor protection.

Against this background, the NREGS is a promising policy for poverty alleviation. After a rapid expansion since its launch in February 2006, the scheme is now introduced in all of India's 600 rural districts and is likely to create work opportunities for over 40 million households in the financial year 2008-09 (Table 1). By providing readily available employment – job-seekers are entitled to employment within two weeks – the program aims to put households in a better position to keep up a basic income flow when no other source of earning is available. Given the great destitution of many households, such immediate support might often boil down to mere survival aid. In addition, the NREGS also counteracts in-work poverty and powerlessness among the privately employed as, by intensifying the competition for casual laborers, it increases the pressure on employers to improve their terms of employment.

Since the work guarantee is limited to one hundred days per household and year, the purpose of the NREGS, however, is not to provide full employment but to offer temporary earning opportunities in periods of low labor demand such as during the agricultural lean seasons or in times of drought, floods, and other natural calamities. Figure1 displays the monthly number of households working under NREGS for the leading States in terms of public expenditures for the scheme. While the participation does not seem to follow a seasonal pattern in Uttar Pradesh, it does so in Madhya and Andhra Pradesh as well as in the other states. Although the peaks in April are certainly inflated due to adjustments in the NREGS reports at the beginning of new financial years, the participation visibly starts to rise in March and remains high until June which coincides with lean

farming season.

Low wealth level combined with credit constraint often limit the mechanisms that can be used to cope with income fluctuations. Under such circumstances, the introduction of a basic income insurance is an important benefit in itself. But there are also secondary effects which promote higher future income of households. This is the case firstly, when reliable emergency aid helps to avoid distress reactions such as sale of livestock, migration etc. that limit the households' economic possibilities in the long run. Secondly, a lower exposure to risk helps to release poor risk averse households from their short planning horizon and encourages the pursuit of higher-risk-higher-return strategies such as investing in productive assets, better production technologies, and the children's education (Holden et al., 2006; Barrett et al., 2001; Bezuneh et al., 1988).

Finally, besides making up for the insufficient private labor demand, the NREGS directs labor resources to the development of the rural infrastructure which further raises its potential to foster self-driven growth and sustainable employment. The work projects realized under the NREGS, as shown in Table1, imply that they encourage a higher agricultural productivity and also improve access to markets and alternative work opportunities. Moreover, by involving the *Gram Sabhas* (village assemblies) in the selection of work projects, the NREGS sets the stage for assets with greatest social benefits to be realized.

So is the NREGS an answer to India's poverty? Given the several transition mechanisms one is inclined to agree. However, experiences of earlier workfare schemes caution against too much optimism as benefits seldom seem to have reached a level which goes beyond temporary relief, allowing participants to escape poverty in the long run. In Maharashtra, where the predecessor of

the NREGS was introduced already in the seventies, the decline in poverty has been no faster than in the rest of India (Dev, 1995; Hirway, 2004).

Doubtlessly, the theoretical beauty of the NREGS encounters the complexity of the practical implementation. Besides offering wide scope for embezzlement of funds (Drze et al., 2008), NREGS raises a challenge to manage trade-off between fast and reliable procurement of jobs and creation of relevant and sustainable assets. Since the number of workers available for the projects is demand-driven, projects have to be selected and planned in a way that not only fulfills the most urgent infrastructure needs but also meets the region- and season-specific demand for labor. This puts high demands on the planning and organizational skills of the program executives. In fact, as Chakraborty (2007) pointed out in a first assessment of the NREGS, lower-income states with limited organizational capacities tend to lag in the implementation of the scheme.

The other concern is the conflict between granting benefits high enough to effectively alleviate the participants' destitution and avoiding distortions in the private labor market. The determination of the NREGS salaries, therefore, is most delicate and contentious which has resulted in a considerable variation across states. As follows from Table 2, ten out of twenty states, where the respective data are available, pay NREGS wages which are higher than the market wages for unskilled male and female labor. NREGS payments still exceed the women's market salaries in another three states, while they are entirely below the market level in the remaining states.

With above-market wages, public works become lucrative for households with private-sector jobs and the incentives to seek regular employment decrease. Moreover, the program can also induce previously inactive persons to join the program. While the job-switchers abet labor shortages

in the private sector, the general rise of wage-seekers and participants boost the program costs which, eventually, might jeopardize the general access to the program, given the limitation of funds earmarked for the public job creation. Indeed, rationing of jobs came true in the employment guarantee scheme of Maharashtra after that the statutory minimum wage rate doubled in 1988 Ravallion et al. (1993). Gaiha (2000), on the same episode, noted that the share of the poor among the participants fell.

The second possible consequence of attractive workfare payments is that households which remain in private employment materialize their strengthened bargaining power and claim higher wages from their employers. Depending on the producers' leeway to cut profit margins or shift wage costs to consumers, this entails either a reduced demand for labor or an increase in wages accompanied by an inflow (or reflux) of labor in the more attractive private-sector employment. The latter, however, comes at the risk of a general price rise which again erodes the higher earnings. There is some evidence that market wages responded to earlier workfare programs in Maharashtra as well as in Bihar (Gaiha, 1996; Nayyer, 2002).

The purpose of the following sections is to determine empirically how the different features of the NREGS – general access in rural areas, work guarantee, wage level, limited participation period – combine with regard to the welfare situation of the individual household and whether the intended purpose of the scheme to alleviate (extreme) poverty is reached. We focus on the participants' circumstances, that is the question to what extent the joining of the program improves their well-being if possible participation costs, particularly forgone benefits of alternative occupations or increased costs of living as a result of wage-induced price inflation, are netted out.

Besides, we gather indications of negative spillover effects of the NREGS on the labor market and nonparticipants by interpreting the turnover of the its workforce as well as the survey results on the motives to participate.

3 Empirical Strategy

Apart from the favorable situation where assignment to a program is randomized (Duflo et al., 2008), people who participate (treated group) are generally different from those who do not (untreated group). In case of poverty reduction programs, participants are typically less well-off, less educated, and more burdened with young or old dependants. As a consequence, estimating the impact of a program by a simple comparison between participants and nonparticipants most likely leads to biased results (see e.g. Ravallion (2008)). The danger is that the treatment effect of interest, i.e. the difference between an outcome with and without intervention, is distorted by a selection effect.

We proceed in two steps to determine the impact of the NREGS. We begin the analysis by using a cross section baseline data of 1066 households collected in June and July 2007 in Medak district of Andhra Pradesh. The NREGS had existed in these villages for the last 12 months. The sample of households were selected based on criteria to qualify as ultra-poor who are defined as households with less than one acre of land, no male earning member, and no access to micro finance. On this cross-section data, we apply the matching techniques developed by Rosenbaum and Rubin (1983) and Heckman et al. (1997, 1998) to calculate outcomes of interest without program participation. Specifically, we take weighted average of outcomes of nonparticipants who are similar to the participants in terms of the propensity score $P(X) = Prob(D = 1|X)$, i.e. the probability

of participating in the program conditional on a vector of observable characteristics X which is determined in a standard logit model. D indicates the participation and is 1 if an individual participates and 0 otherwise.

The average program impact is estimated by the single difference

$$SD = \frac{1}{T}\sum_{i=1}^{T}(Y_i^1 - \sum_{j=1}^{C} W_{ij} Y_j^0) \tag{1}$$

where Y_i^1 is the outcome of interest of participant i, Y_j^0 that of a nonparticipant j, T and C denote, respectively, the participants (treated) and nonparticipants (comparison group) in the region of common support where both groups have a positive probability to participate. W_{ij} are kernel-weights which give higher weights to closer matches of nonparticipants.

While the propensity-score matching method allows us to correct for the selection bias due to observable characteristics, we can not address a possible selection bias that arises out of unobserved heterogeneity between participants and nonparticipants. Therefore, in the second step, we use data from a follow-up survey of 314 households collected 18 months after the baseline in December 2008. This longitudinal data enables us to correct for the (time-invariant) unobservable bias by the means of difference-in-difference methods.

The most obvious approach is to use a conventional double-difference estimator to compare the changes in outcomes for participants and nonparticipants over time:

$$DD = [E(Y_2^T|D_2 = 1) - E(Y_1^T|D_1 = 1)] - [E(Y_2^C|D_2 = 0) - E(Y_1^C|D_1 = 0)] \tag{2}$$

where Y_t denotes the outcome of interest in period $t = \{1, 2\}$, T and C, respectively, stand for treated and untreated (or control), and D_t denotes the participation in t.

However, our case differs from the common double-difference setting since the NREGS was already in operation when the first survey (baseline) was collected. This means that the comparison between participants and nonparticipants estimates a different effect than the matching method in the first step. Instead of the treatment effect of the program introduction, the comparison of post-intervention data results in the estimate of the longer-term impact of the program for participating households. In other words, with the double-difference we can not estimate a possible first-stage shift in outcome brought about the introduction of the program but only track differences in the growth rates in the outcomes afterwards.

To estimate the first-stage level shift and verify the results of the matching approach we make use of the movements into and out of the program as well as whether non participation was voluntary. These two factors define subgroups of participants and nonparticipants from which we chose pairs of treated and untreated which are otherwise ex ante most similar. We do this with regard to the precondition for unbiased difference-in-difference estimates which is that the compared groups are similar enough that their outcomes would have run parallel if the intervention had not taken place, i.e.

$$[E(Y_2^C|D_2 = 1) - E(Y_1^C|D_1 = 1)] = [E(Y_2^C|D_2 = 0) - E(Y_1^C|D_1 = 0)].$$

Since the NREGS program is still a new program, one can reasonably expect a part of nonparticipants to be involuntary. They stay outside the program because, regardless of them demanding and entitlement, they are not provided jobs. Similarly, it is not unlikely that those who joined the scheme later would have participated earlier if they had been given the opportunity. Therefore, a first approach to obtain panel-based estimates of the program impact is comparison between

new joiners and long-term participants ("stayers"). In this way, the first-stage impact of the joining households is corrected by the performance of the stayers who have already experienced that impact but whose characteristics should not be too different otherwise. A second approach is to contrast the new joiners against the involuntary nonparticipants. That is groups of households that demanded work but some got it (joiners) while others didn't (involuntary nonparticipants) due to exogenous reasons. This comparison is possibly more precise because it takes care of the self selection problem because there is no reason to assume that the households which successfully (new joiners) or unsuccessfully (involuntary nonparticipants) demanded NREGS during the second survey differ with respect to their wish to participate in the past. Aside from that, the comparison between voluntary program leavers and voluntary nonparticipants is a third conceivable way to determine the program impact.

Beyond the direct computation of the double-differences of these pairs, we also calculate the double-differences conditional on observable household characteristics to further reduce the risk of uncontrolled (time-variant) selection bias. Finally, we follow Ravallion et al. (2005) and estimate "triple-difference estimators" which additionally allows us to control for potential changes in the outcomes not caused by the NREGS. This is of importance because all the households in the panel data were also participants in a separate anti-poverty program run by a local microfinance institution called the Ultra Poor Program. The central feature of the ultra poor program was an asset transfer to the selected households. It is most likely that the participation in this anti-poverty program improved the outcomes of interest as well.

The triple-difference estimator involves the correction for outcome changes of observationally

similar groups of never-participating households before conducting the comparison for the chosen pairs of similar groups. In the case of new joiners vs. stayers, the triple-difference estimator is defined as follows:

$$DDD = E[(Y_2^T - Y_1^T) - (Y_2^C - Y_1^C)|D_2 = 1, D_1 = 0] - E[(Y_2^T - Y_1^T) - (Y_2^C - Y_1^C)|D_2 = 1, D_1 = 1] \quad (3)$$

where the control groups for the new joiners and the stayers are matched according to their propensity-score.

4 Data and Descriptive Statistics

Tables 3a and 3b show selected descriptive statistics of the economic situation and the socio-economic profile of the participants and nonparticipants of the NREGS. The comparison of the participation rates shows the steep growth or quick penetration of the program. While 34% of the households participated in the NREGS during the first survey in summer 2007, this number rose to 47% within 18 months at the end of 2008.

In 2007, the households participating in the NREGS had average monthly per capita consumption expenditures of Rs.445 which was slightly more than the nonparticipating households. In the follow up survey the consumption expenditure of participants had risen by 4%. Nevertheless, it clearly fell short of the expenditure level of nonparticipants which had shown a remarkable increase of 29% between the two surveys. Note again that all households in the second survey were participants of the Ultra-Poor Program which certainly played a decisive role in improving consumption levels. Apart from that, as more detailed analyses below shows, the superior outcome of the non-

participants at the second survey is largely due to non-random attrition, i.e. better off households are more likely to drop out of the NREGS.

The available income data explains the expenditure pattern. If we look at the sources of income, we find that cultivation on own land generally contributed very little to the household earnings. This points to the largely subsistence character of farming activities of the ultra poor. Equally marginal was income from formal employment from which the low-qualified poor households are usually excluded. On the other hand, informal employment turns out to be the main source of income for the entire population - participants and nonparticipants. In the first survey, the participants derived 58.4% and 23.2% of their income, respectively, from informal agricultural and non-agricultural employment. The respective numbers for non-participants were lower at 54.4% and 16.4%. By the end of 2008, the share of income from non-agricultural labor had fallen for both groups. But share of income from agricultural labor fell more for the participants of NREGS than non-participants. Not surprisingly, finally, the income structure between participants and nonparticipants differed with regard to self-employment outside the agricultural sector, e.g., running a shop, making crafts, or offering services such as tailoring and providing meals. While NREGS participants derived only little income from such activities, nonparticipants drew about 10% of their income from there.

Looking at the saving behavior of the ultra poor households we find that the NREGS participants were more likely to save and the average monthly per capita savings was approximately Rs.10 in 2007. As the Ultra Poor Program prescribes saving, the propensity to save was close to 100% for all households in the second survey. By the end of 2008, the monthly per capita savings had risen to Rs.44 for participants and Rs.54 for nonparticipants.

As the comparison of consumption and savings expenditures with the annual income reveals, the financial outflow per household generally exceeds the inflow. This becomes apparent in the considerable indebtedness of the households which ranged from about 50% to over 70% of the average annual income among NREGS participants and nonparticipants, respectively. Interestingly, participants had a lower dependence on loans from relatives, neighbors, and friends (13% vs. 24% among non-participants) as well as from money lenders (30% vs. 38%). NREGS participants borrowed almost twice as often from self-help groups (49% vs. 27%) implying a greater degree of financial inclusion. In terms of household assets, the descriptive statistics show that participating households were more likely to own land, to live in a house which did not leak during rains, and to use electricity as the energy source for lighting.

5 Results

5.1 Impact on Consumption and Savings

Cross-section analysis

Let us start by first looking at the logit regression in Table 4 which was used to estimate the propensity scores for participating in the NREGS in 2007. Although participants and nonparticipants appear to have been quite similar in their observable characteristics, there are some significant determinants of participation. First, the place of residence played a central role whether a households participated in the NREGS or not, which indicates important differences in the degree and quality of the scheme's implementation. Second, as follows from F-tests for model comparisons,

the probability of program participation rose with the household size. Third, being self-employed or being employed in the non-agricultural sector decreased the probability of resorting to public works. On the other hand, owning land increased the NREGS participation, indicating that the independent agricultural production leaves people (seasonally) underemployed which results in ancillary income activities to reach a higher or sufficient income. Finally, being elected for the Ultra Poor Program also appears to have had a positive effect on program participation, which might be due to accompanying training sessions that rose the awareness about the NREGS.

Based on the propensity scores, Table 5 presents the single-difference calculations for the average impact of NREGS on the monthly per capita consumption expenditure and the savings behavior. The results indicate that the participation in the NREGS increased total consumption by Rs.25 or 6% of the pre-intervention income. The breakdown into subcategories shows that the increase was most distinctive with the non-food consumables in which the expenditures rose by 40%. They were followed by the increase in clothing purchases (+11%) and food expenditures (+7%). On the other hand, participants appear to have been able to reduce their transportation outlays by 18%, which suggests that the NREGS effectively provided close-by work. The changes in the other expenditure categories were not significant on at least a 10% level. Concerning savings, the impression from the descriptive statistics is confirmed that participants are more likely to save (68%) as opposed to nonparticipants (59%). But they do not differ signifcantly in the amount saved.

Figure 2 illustrates the consumption-increasing effect of the NREGS by comparing the post-intervention cumulative distribution function of total consumption (lower curve) with the pre-intervention equivalent calculated from the estimates obtained (upper curve). The x-axis shows

the official poverty line of Rs.356 monthly per capita consumption expenditure (MPCE) as well as a lower threshold of 0.75*MPCE ("extreme poor") and a higher threshold of 2*MPCE ("marginal"). It follows that, among the participants, the NREGS caused extreme poverty to fall from 23% to 16% and lowered poverty as measured by the official poverty line from 44% to 37%. For higher consumption levels, the impact – as intended – was samller. At the line of marginal poverty, the poverty rate fell only by one percentage point from 90% to 89%.

Panel-data analysis

As discussed, matching estimates come with the shortcoming that they only correct for observable differences but not for unobservable heterogeneity across households. Therefore, we repeat the expenditure impact analysis for the 314 households for whom we have a panel data as they were interviewed twice across 18 months. This comes with the advantage that we can break down the participants and nonparticipants into finer subgroups of householsd. Table 6 shows the change over time of the monthly per capita consumption (i.e. $Y_{t=2} - Y_{t=1}$) for households which never participated in the NREGS, households which left the scheme after one period ("leavers"), households that joined it only in the second period ("new joiners") and households that participated in the NREGS in both years ("stayers").

The finer breakdown reveals the considerable heterogeneity within the participant and nonparticipant groups of single cross-sections. As the first four rows of the table reveal, stayers and leavers – both classified as participants in the 2007 survey – performed very differently over time. While the stayers saw the least growth in total consumption, the leavers fared best with the highest increase in consumption. The leavers surpassed the nonparticipants' performance by 17%. The

non-consideration of longitudinal information explains the somewhat startling reversal of the higher consumption expenditures from participants in the first survey to nonparticipants in the second survey that came out in the descriptive statistics in Table 3a. The 2008 nonparticipants outperformed because they include leavers who have above-average performance. On the other hand, 2008 participants include permanent NREGS subscribers who form the weakest part of this population as they were unable to quit the program.

We have collected data on reasons for not participating in the program. There are involuntary non participants (because of non-provision of a NREGS job) and voluntary leavers. As expected, the households with no interest in the NREGS increased their total expenditures more than the average nonparticipant, whereas those households which involuntarily stayed outside the program did not fare as well. In contrast, the voluntary leavers, i.e., those who dropped out of the NREGS as soon as better work opportunities arose, appear to have performed by far the best.

Now let us look at the double difference which estimates a more accurate impact of the program as it corrects for possible unobservable selection biases. The differences of expenditure change over time, for selected pairs of participant and nonparticipant categories is presented in Table 7. The comparison of similar groups of households, example new joiners and stayers suggests that the entry into the program had a significant positive first-stage effect. With respect to program leavers, the results show the better performance of program leavers compared to the permanent participants of NREGS.

Now for our main results, let us look at Table 8 which consolidates the impact estimates and presents the triple differences for the new joiners vs. the stayers as well as for the new joiners

vs. the involuntary nonparticipants. By first comparing each of these groups to a observationally similar (i.e. propensity-score matched) group of households which was never part of the NREGS, we sort out possible changes in the expenditure level that happened independent of the NREGS. For example, new joiners might include those who voluntarily refrained from the scheme in the first period, it is possible that the group of new joiners on average had better economic prospects than the households which participated in both periods. If this is the case, the double difference new joiners vs. stayers overestimates the impact of the program.

Calculating the triple difference leads to a downward correction of the double difference of Table 7 and results in a Rs.35.4 gain in food consumption per capita and corresponds to 15% of the new joiners' average food expenditures during the first survey. The same procedure applies to the comparison of new joiners and involuntary nonparticipants. The triple difference boosts the estimates of the program impact to a highly significant Rs.96.6 increase in food expenditure.

Table 9 presents the triple-difference estimates based on the new joiners-stayers comparison for different consumption categories. It follows that the NREGS not only had a significant impact on the consumption expenditure of food (14.8%), but also substantially increased the spending for non-food consumables (+48.8%) and clothing (+43.9%), as well as virtually eliminated the outlays for transportation (-99.2%). These results fit well to the propensity score matching outcomes of the 2007 data.

Interestingly, the triple-difference results also suggest a reduction of the spending for the children's education as a result of the participation in the NREGS, indicating that the school-age household members have to take over tasks in and around the house which were previously done

by the now engaged adults. On the other hand, the small but exceptional increase of the educational expenditures between the first and second survey shown by the long-term participants (see Table 6) also imply that the educational efforts might recover in the longer-run. Unfortunately, the insufficient sample size, which results when only keeping the households with children, prohibits to test for these hypotheses.

Besides the consumption expenditures, we also recalculated the savings outcomes on the basis of the panel data. However, as the probability of saving was close to 100% for all twice-interviewed households due to their Ultra Poor Program participation, we had to restrict the analysis to the savings level. There, consistent with the cross-section analysis, we found no significant impact of the NREGS.

To summarize, both the cross-section and the panel-data analyses disclose a positive impact of the NREGS on relevant household expenditures. Most important is the significant improvement in food security as reflected in increased food expenditures. Being the main item in the households' budget, this category gains by far the most in absolute terms which indicates that the NREGS payments first of all improve the the food security before flowing into alternative uses. Furthermore, the positive impact of NREGS on propensity to save, which follows from cross-section analysis, further underscores the economical use of the earnings from the program.

Besides this, there is suggestive evidence that the relatively better off also benefit from the NREGS. Although the comparison is statistically insignificant –due to small sample size – those who voluntarily left the program after first year appear to have done better than those who voluntarily stayed outside the NREGS during the whole observation time of two years. This indicates that

the basic objective of the program to provide unemployment insurance is being met. Households are able to overcome temporary income shortfalls by flexibly and quickly moving in and out of the NREGS.

5.2 Impact on Health

Attempting to capture the impact of the NREGS beyond economic situation and to allow for a broader concept of welfare, we conclude our analyses by examining the scheme's effect on different health outcomes. Table 3b shows descriptive statistics for selected indicators of physical and mental health for NREGS participants and nonparticipants.

In the first survey, 27% of the participating households reported an improvement of their health status in the year preceding the survey, compared to 21% of the non-participating households. Conversely, participants were less likely to state a deterioration of their health condition in the last one year. We have a composite measure of health as an indicator of various measures. This indicator includes the ease of carrying out daily physical activities such as dressing, climbing a small hill, walking for 5 kilometers, bowing, squatting or kneeling. The comparison of this indicator suggests that participants were in a better health condition than the non-participants. We also gathered the height and weight of children under 14 years of age and find no significant difference between the participants and nonparticipants. In terms of mental health, participants appear to be in a better condition as they are less likely to report feeling worried, tensed or anxious.

Table 11 verifies whether the differences in the health outcomes of the 2007 survey persist if propensity-score matched groups of participants and nonparticipants are compared with each other. It follows that the differences in the physical health indicators are not significant. In contrast, the

better mental condition of the participants as measured by their lower probability of being worried, tensed or anxious is highly significant. While 32% of the nonparticipants display such problems, the share decreases to 23% among the NREGS participants which equals a reduction of one fourth. This strongly suggests that the NREGS succeeds in providing a sense of security from which the participating households benefit irrespective of whether they temporarily or repeatedly enroll in the scheme.

6 Conclusion

This paper examined the welfare impact of the Indian Rural Employment Guarantee Scheme (NREGS) as measured by the changes in expenditure level and physical and mental health indicators. Using two consecutive household surveys conducted in the years 2007 and 2008 in Medak district of Andhra Pradesh, we applied the propensity-score matching as well as difference-in-difference methods to compare the outcomes of interest of participants and nonparticipants net of selection bias due to observable and unobservable heterogeneity.

The results suggest that the NREGS has a significant impact in alleviating rural poverty. First of all, the NREGS appears to have a substantial effect on the food security of the participating households. The food expenditures, which generally account for about 60% of total consumption, show to increase by 15% if the weaker section of the population is focused on. The effect is about half as large if the households which participate only temporary and are able to leave the NREGS after some time are not separated from the – economically weaker – households which repeatedly resort to the program. This indicates that the effect of the NREGS increases with the vulnerability

of a household and stands for the targeting capability of the program.

Besides food, the NREGS raises the probability of saving as well as the expenditures in other categories. Spending in non-food consumables and clothing increases by impressive 40 to 50% among the less well off participants. Given the low starting level in these expenditure categories, this is not surprising. Finally, thanks to the decentralized implementation of the NREGS which promotes work opportunities close-by, the transport outlays of the participants decrease; among the poorest households they even disappear.

Finally, besides the improvement of the financial situation, the NREGS shows a significant decrease of emotional distress in the form of anxiety, tension, and worries. While this certainly is a benefit in itself, experience from other workfare schemes show that it is not unlikely that this promotes the farsightedness and forward planning of the participants and strengthens their ability to pursue higher risk-higher return strategies such as investing in in productivity-enhancing equipment or the children's education. Hence, in the long run, the NREGS might come along with multiplier effects that foster the well-being not only of actual but also of potential beneficiaries who are able change their behavior thanks to the NREGS. These findings also confirm the general sentiment of 96% of the households that it is a useful program (see Table 10).

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Table 1: Expansion of the NREGS, 2006-07 to 2007-08

	2006-07	2007-08	2008-09*
Total expenditures (million USD)	2146	3946	4000
Districts under the program	200	330	604
Employed households (million)	20.5	33.7	43.7
Working days (million)	884	1431	1609
Average salary per day (USD)	1.6	1.9	1.7
Completed work projects since the start of the NREGS (million)	0.38	0.82	2.62
- Water catchment and conservation	72.1%	50.8%	28.5%
- Rural connectivity	16.6%	26.4%	18.4%
- Drought control, flood control, irrigation	4.9%	11.2%	22.4%
- Land development	1.8%	6.9%	22.6%
- Others	4.5%	4.7%	8.0%

Notes: The figures for the year 2008-09 are estimates based on the information available by the end of December 2008. 1 USD = 40 Rs. Data source: Employment and financial status reports, MRD (2008b).

Table 2: Comparison of rural wages, financial year 2007-08

	NREGS wages		**Market wages unskilled non-agr. labor**			
State	**minimum**	*1-year growth*	**men**	*1-year growth*	**women**	*1-year growth*
Andhra Pradesh	80.00	*0.0%*	60.4	*12.1%*	44.6	*13.9%*
Assam	73.76	*14.1%*	73.7	*12.3%*	52.4	*17.8%*
Bihar	80.00	*17.6%*	58.3	*4.1%*	51.4	*0.8%*
Gujarat	50.00	*0.0%*	56.2	*9.0%*	53.7	*8.7%*
Haryana	126.05	*30.1%*	100.2	*4.7%*	93.4	*1.3%*
Himachal Pradesh	75.00	*5.3%*	113.6	*1.6%*	na	*na*
Jammu and Kashmir	70.00	*0.0%*	109.0	*2.5%*	na	*na*
Karnataka	74.00	*10.7%*	49.9	*3.1%*	37.3	*10.3%*
Kerala	125.00	*0.0%*	168.9	*11.1%*	129.6	*9.5%*
Madhya Pradesh	67.00	*7.7%*	40.9	*2.9%*	35.4	*4.9%*
Maharashtra	69.00	*0.0%*	57.4	*9.1%*	36.2	*7.5%*
Manipur	81.40	*12.4%*	52.8	*0.0%*	48.1	*0.0%*
Meghalaya	70.00	*0.0%*	78.3	*12.9%*	47.5	*-10.4%*
Orissa	68.75	*25.0%*	56.7	*8.9%*	46.3	*6.2%*
Punjab	97.76	*0.3%*	103.7	*22.4%*	na	*na*
Rajasthan	75.25	*3.1%*	78.3	*9.0%*	71.6	*10.8%*
Tamil Nadu	80.00	*0.0%*	85.7	*9.1%*	63.5	*17.6%*
Tripura	70.83	*18.1%*	90.0	*11.7%*	na	*na*
Uttar Pradesh	89.67	*54.6%*	67.1	*8.0%*	56.1	*10.8%*
West Bengal	71.20	*3.8%*	61.1	*7.7%*	53.7	*7.0%*

Notes: The highlighted States are those with prescribed NREGS wages higher than market wages for both male and females. Data Sources: MRD (2008c); Indiastat.com (2008).

Table 3a: Economic Descriptive Statistics

	May-July 2007 survey		Nov.-Dec. 2008 survey	
	Participants (n=359) mean (st.dev) frequency	**Nonpart.** (n=707) mean (st.dev) frequency	**Participants** (n=146) mean (st.dev) frequency	**Nonpart.** (n=168) mean (st.dev) frequency
Economic situation				
Monthly per capita consumption (in Rs.)	444.9 (190.5)	425.6 (205.8)	461.6 (214.7)	547.5 (299.5)
Food	61.7%	61.5%	59.8%	59.9%
Nonfood	38.3%	38.5%	40.2%	40.1%
Annual per capita income (in Rs.)	3869.9 (3370.1)	3711.6 (6537.4)	-	-
From agricultural self-employment	1.6%	1.0%	1.6%	2.9%
From agricultural labor	58.4%	54.4%	47.3%	68.1%
From non-agric. self-employment	2.1%	10.2%	4.8%	10.1%
From non-agric. labor	23.2%	16.4%	17.1%	13.9%
From formal emploment	2.5%	1.8%	2.9%	5.0%
From others sources	12.2%	16.0%	-	-
From NREGS	-	-	26.4%	0.0%
Monthly saving, per capita (in Rs.)	9.1 (10.3)	9.6 (17.6)	43.8 (45.3)	53.7 (63.1)
% of hhd. which save through some channel	69.5%	55.3%	99.4%	98.7%
Total outstanding loan, per capita (in Rs.)	2005.4 (3430)	2660.9 (5989.1)		
% from relatives/ neighbours/ friends	12.7%	23.6%		
% from money lenders	29.8%	37.8%		
% from self-help or other savings groups	49.1%	27.3%		
Household assets				
% of households owning land	45.1%	33.0%		
% of households owning their home	71.0%	71.2%		
% of hhd. which house does not leak during rain	24.0%	19.6%		
% of hhd. which use electricity for lightning	47.8%	41.0%		
Size of household				
Total number of household members	3.4 (1.7)	3.2 (1.7)		
Number of members $\geq$ 14 years	2.2 (1.3)	2.1 (1.2)		
Number of members <14 years	1.2 (1.1)	1.1 (1.2)		

Notes: Prices are for May 2007. Other income sources include governmental assistance (which might include NREGS payments), remittances, pensions, gifts, as well as rental income and revenues from the sale of land.

Table 3b: Demographic and Health Descriptive Statistics

	May-July 2007 survey		Nov.-Dec. 2008 survey	
	Participants (n=359) mean (st.dev) frequency	**Nonpart.** (n=707) mean (st.dev) frequency	**Participants** (n=146) mean (st.dev) frequency	**Nonpart.** (n=168) mean (st.dev) frequency
Socio-demographic characteristics				
Religion				
% of Hindu households	69.1%	72.8%		
% of Muslim households	5.6%	9.9%		
% of Christian households	25.4%	17.3%		
Educational status				
% of illiterate households	95.8%	94.1%		
Political and communal participation				
% of hhd. part. in elections of the village council	91.6%	93.7%		
% of hhd. which have approached village mayor about town or village issues	54.5%	53.9%		
Health situation				
Physical health				
% of hhd. with improving health in prev. year	27.0%	21.4%	-	-
% of hhd. with worsening health in prev. year	24.8%	30.1%	-	-
% of hhd. with health-rel. work inabilities	37.6%	40.5%	-	-
Indicator of physical health problems, scale from 0 (best) - 42 (worst)	2.7 (4.2)	3.3 (5.1)	-	-
Average height of children < 14 years (in cm)	-	-	127.6 (20.4)	127.4 (23.5)
Average weight of children < 14 years (in kg)	-	-	24.4 (8.8)	24.8 (9)
Mental health				
% of hhd. who felt worried/tense/anxious for at least one month in previous year	22.0%	33.8%	-	-
Indicator of mental health problems, scale from 0 (best) - 15 (worst)	3.9 (5.2)	4.0 (5.0)	-	-

Notes: The indicator of physical health problems summarizes the level of difficulty (not difficult/ difficult but able to do without help/ able to do only with help/ not able to do) to perform 14 daily physical activities such as dressing, bathing, climbing a small hill, or drawing water from a well. A value of 0 means that a household, on average, has no problem with any of these activities, while 42 indicates that its members are not able to carry out any of these activities. Similarly, the indicator of mental health problems summarizes the frequency (never, hardly ever, sometimes, always) of feelings of sadness, bouts of weeping, loss of appetite, lack of work motivation, and sleeplessness. The scale reaches from 0 to 15 with 0 indicating that a household does not know any of these problems.

Table 4: Logit regression to generate the propensity score, 2007 sample

	Estimate (std. error)	
(Intercept)	-1.027 (0.639)	
Kalher commune (mandal)	0.292 (0.292)	
Kantgi commune	0.396 (0.324)	
Manoori commune	0.082 (0.300)	
Narayankhedi commune	-0.222 (0.302)	
Nyakali commune	-0.177 (0.337)	
Raikodei commune	0.663 (0.336)	*
Regodei commune	0.723 (0.305)	*
Shankarampeti commune	0.596 (0.298)	*
Tekmali commune	0.954 (0.455)	*
Muslim	-0.148 (0.187)	
Christian	0.163 (0.151)	
Other backward class (OBC)	-0.048 (0.199)	
Scheduled caste (SC)	0.203 (0.136)	
Scheduled tribe (ST)	0.280 (0.344)	
Number of household members	0.045 (0.032)	
Household head aged 20-40	0.575 (0.346)	.
Household head aged 40-60	0.478 (0.356)	
Household head aged $>$ 60	0.203 (0.842)	
Share of household members $<$ age 13	-0.112 (0.206)	
Unmarried	0.634 (0.410)	
Divorced	-0.015 (0.207)	
Widowed	-0.005 (0.187)	
Literate	-0.221 (0.215)	
Agricultural laborer	-0.106 (0.209)	
Non-agricultural laborer	-0.682 (0.315)	*
Salaried employee	-0.541 (0.734)	
Self-employed (non-agricultural)	-0.761 (0.319)	*
Housewife, studying, unemployed	-0.373 (0.402)	
Living in rented house	0.365 (0.252)	
Living in relatives' house	0.104 (0.111)	
House type = *Kuchha* (medium quality)	-0.225 (0.327)	
House type = Thatched (low quality)	-0.312 (0.344)	
House leaks during rains	-0.077 (0.111)	
Ownership of land	0.207 (0.103)	*
Ownership of livestock	0.044 (0.141)	
Participation in elections	-0.197 (0.174)	
Participation in SKS Ultra-Poor Program	0.194 (0.094)	*
Number of observations	956	
LR Chi^2	126.5	
Prob $>$ Chi^2	0.000	
Pseudo R^2	0.104	
Log likelihood	-547.5	
Significance codes: * 0.05 . 0.1		

Notes: The dependent variable is the binomial indicator whether a household participated in the NREGS (1 = participation).

Table 5: Average impact of the NREGS on monthly per capita expenditures, 2007 sample (n=1066)

	Participants $\overline{Y_{2007}}\|D_{2007}=1$	Nonparticipants $\overline{Y_{2007}}\|D_{2007}=0$	Single Difference $[\overline{Y_{2007}}\|D_{2007}=1]$-$[\overline{Y_{2007}}\|D_{2007}=0]$	
	abs. value	abs. value	abs. value (std. error)	in % of the average expendit. of the nonpart.
Total consumption (in Rs.)	448.3	422.9	25.3 * (12.0)	6.0%
Food	277.6	260.2	17.5 . (9.2)	6.7%
Tobacco, alcohol	14.7	13.0	1.7 (1.9)	13.1%
Non-food consumables	32.3	23.1	9.2 *** (2.1)	39.8%
Clothing	29.6	26.8	2.8 * (1.5)	10.5%
Energy	12.5	13.2	-0.7 (1.0)	-5.3%
Transport	10.7	13.1	-2.3 * (1.1)	-17.6%
Health	41.2	44.8	-3.5 (4.2)	-7.8%
Education	12.6	13.5	-0.9 (3.8)	-6.7%
Other expenses	17.0	15.5	1.5 (1.4)	9.7%
Saving, level (in Rs.)	8.8	15.5	-0.8 (1.0)	-7.9%
Saving, probability	0.68	0.59	0.09 * (0.04)	15.3%

Notes: This table shows single differences between expenditures of the NREGS participants and expenditures of the propensity-score matched and kernel-weighted nonparticipants. ***/**/*/. stand for significant changes on a 0.001/0.01/0.05/0.10 level based on t-statistics bootstrapped with 500 repetitions. Non-food consumables include expendable articles of daily use such as toiletry, cleaning agents, or detergents. Other expenses include: rates and taxes, expenses for personal services (e.g. haircut and shave), entertainment, phone calls, household and kitchen equipment.

Table 6: Change in monthly per capita expenditures between the 2007 and 2008 survey

(in Rs., prices May 2007)	Food	Non-food consumables	Clothing	Energy	Transport	Health	Education	Other expenses	Total Expenditures
Nonparticipants, both surveys (n=119)	55.2	25.3	-3.8	16.2	17.4	-16.4	-0.9	12.6	105.6
	(18.4)	(4.8)	(2.6)	(3.2)	(4.2)	(9.6)	(5.9)	(3.5)	(28.9)
Participants, only 1st survey, "leavers" (n=49)	53.7	38	-5.6	15.6	44.9	-10.7	-22.4	10.3	123.9
	(23.7)	(9.6)	(4.7)	(3.8)	(9.3)	(9.7)	(11.3)	(3.7)	(43.2)
Participants, only 2nd survey, "new joiners" (n=80)	43	19.1	-2.2	13.8	22.3	-22.9	-3.6	6.6	76.1
	(16.1)	(6.2)	(3.3)	(3)	(6.5)	(5.6)	(3.4)	(2.3)	(24.8)
Participants, both surveys, "stayers" (n=66)	5.6	7.3	-5.1	16.4	31.3	-25.6	6.3	11.1	47.4
	(18.3)	(5.5)	(3.4)	(4.6)	(8.6)	(10.2)	(5.4)	(2.7)	(31.6)
Involuntary nonparticipants who failed to get work (n=48)	-6.2	15.8	-3.7	20.4	20	-0.1	5.4	9.5	61.1
	(26.8)	(6.7)	(5)	(5.3)	(8.3)	(21.4)	(9.4)	(3.2)	(51.9)
Voluntary nonparticipants with enough other work (n=43)	86.2	30.7	-1.1	13.6	15	-31.1	-9.3	13.7	117.8
	(29.2)	(9.4)	(3.4)	(5.9)	(4.9)	(9.9)	(12.6)	(8.4)	(41.9)
Involuntary leavers who failed to get work (n=20)	25.4	13.1	-10.8	15.5	23.6	-18.1	-11.6	10.2	47.3
	(26.7)	(14.2)	(7.9)	(4.9)	(5.7)	(15.2)	(10.1)	(6)	(43.4)
Voluntary leavers with enough other work (n=19)	106.2	53.4	-4.8	23.9	66.3	-23.3	-42.9	14.6	193.5
	(49.3)	(16.1)	(8.3)	(7.7)	(19.9)	(16.4)	(26.9)	(5.6)	(94.5)

Notes: Numbers in parentheses are standard deviations. Non-food consumables include expendable articles of daily use such as toiletry, cleaning agents, or detergents. Other expenses include: rates and taxes, expenses for personal services (e.g. haircut and shave), entertainment, phone calls, household and kitchen equipment.

Table 7: Impact on monthly per capita food expenses, double-differences

			DD_1	DD_2	DD_3	DD_4	DD_5	DD_6	DD_7	DD_8
(1)	$[\overline{\Delta Y}\|D_{08}$=1 or D_{07}=1]	"part. in 2007 or 2008	-40.1 .							
	$-[\overline{\Delta Y}\|D_{08}=D_{07}=0]$	vs. nonparticipants"	(21.3)							
(2)	$[\overline{\Delta Y}\|D_{08}$=1 & D_{07}=0]	"new joiners vs.		45.1 .						
	$-[\overline{\Delta Y}\|D_{08}=D_{07}=1]$	stayers"		(26)						
(3)	$[\overline{\Delta Y}\|D_{08}$=1 & D_{07}=0]	"new joiners vs.			-24.8					
	$-[\overline{\Delta Y}\|D_{08}=D_{07}=0]$	nonparticipants"			(27.4)					
(4)	$[\overline{\Delta Y}\|D_{08}$=1 & D_{07}=0]	"new joiners vs.				14.6				
	$-[\overline{\Delta Y}\|D_{08}=D_{07}=0$ &	involuntary nonpart."				(30.8)				
	demanded work in 2008]									
(5)	$[\overline{\Delta Y}\|D_{08}=0$ & D_{07}=1]	"leavers vs.					71.9 *			
	$-[\overline{\Delta Y}\|D_{08}=D_{07}=1]$	stayers"					(31.5)			
(6)	$[\overline{\Delta Y}\|D_{08}=0$ & $D_{07}=1$ &	"involuntary leavers						71.9		
	demanded work in 2008]	vs. stayers"						(39.8)		
	$-[\overline{\Delta Y}\|D_{08}=D_{07}=1]$									
(7)	$[\overline{\Delta Y}\|D_{08}=0$ & D_{07}=1]	"leavers vs.							-23.2	
	$-[\overline{\Delta Y}\|D_{08}=D_{07}=0]$	nonparticipants"							(35.6)	
(8)	$[\overline{\Delta Y}\|D_{08}=0$ & D_{07}=1	"voluntary leavers vs.								81.5
	& no interest in 2008]	voluntary nonpart."								(69.3)
	$-[\overline{\Delta Y}\|D_{08}=D_{07}=0$ &									
	no interest in 2008]									
	Number of observations		314	146	199	128	115	86	168	62
	F-statistic		3.132	2.023	2.721	3.905	2.624	1.955	2.147	1.303
	p-value		0.000	0.023	0.002	0.000	0.003	0.038	0.014	0.245
	R^2		0.064	0.091	0.087	0.182	0.144	0.150	0.083	0.150

Notes: $\overline{\Delta Y} = Y_{t=2008} - Y_{t=2007}$; D_t denotes the NREGS participation in t. Household controls are place of residence (*mandal*), household size, occupation, and landownership. More extensive models come at the cost of further losses of observations. Numbers in parentheses are standard errors. ***/**/*/. stand for significant changes on a 0.001/0.01/0.05/0.10 level.

Table 8: Average impact of the NREGS on monthly per capita food expenditures, triple-difference estimates

	New participants	Matched nonparticipants	Stayers	Matched nonparticipants	Involuntary nonparticipants	Matched nonparticipants
	$\overline{Y}_t^T \mid (D_{08}=1, D_{07}=0)$	$\overline{Y}_t^C \mid (D_{08}=D_{07}=0)$	$\overline{Y}_t^T \mid (D_{08}=D_{07}=1)$	$\overline{Y}_t^C \mid (D_{08}=D_{07}=0)$	$\overline{Y}_t^{NT^*} \mid (D_{08}=D_{07}=0)$	$\overline{Y}_t^C \mid (D_{08}=D_{07}=0)$
t = 2007	239.9	240.5	261.9	229.5	288.8	250.2
	(12.4)	(14.4)	(14.8)	(13.9)	(25.5)	(16.1)
t = 2008	282.9	307.0	267.5	294.0	282.7	364.2
	(15.2)	(17.3)	(15.8)	(17.3)	(25.0)	(25.6)
Single Differences	43 **	66.5 ***	5.6	64.5 ***	-6.2	114.0 ***
$SD = \overline{Y_{t=2}} - \overline{Y_{t=1}}$	(16.1)	(18.3)	(18.3)	(18.2)	(26.8)	(27.1)
Double Differences	-23.5		-58.9 ***		-120.1 ***	
$DD = SD^T - SD^C$	(17.4)		(18.2)		(27.0)	
Triple Difference I, new joiners vs. stayers	35.4 *					
$DDD_I = DD\mid(D_{08}=1,D_{07}=0) - DD\mid(D_{08}=D_{07}=1)$	(17.8)					
Triple Difference II, new joiners vs. involuntary nonparticipants	96.6 ***					
$DDD_{II} = DD\mid(D_{08}=1,D_{07}=0) - DD\mid(D_{08}=D_{07}=0$, involuntary)	(21.4)					

Notes: Values in parentheses are standard errors. ***/**/*/. stand for significant differences on a 0.001/0.01/0.05/0.10 level. The nonparticipants are propensity-score matched based on their place of residence (*mandal*), household size, occupation, and landownership. Involuntary nonparticipants (NT^*) are households which reported in the second survey that they demanded NREGS work but were not provided one.

Table 9: Average impact of the NREGS on monthly per capita expenditure subcategories, triple-difference estimates

	DDD_I new joiners vs. stayers		DDD_{II} new joiners vs. involuntary nonpart.	
	in Rs. (std. error)	in % of the average 2007 expendit. of the new joiners	in Rs. (std. error)	in % of the average 2007 expendit. of the new joiners
Total Consumption (excl. alcohol & tobacco)	16.5 (29)	4.4%	45.8 (34.8)	12.1%
Food	35.4 * (17.8)	14.8%	96.6 *** (21.4)	40.3%
Non-food consumables	11.4 * (5.4)	48.8%	16.3 ** (5.9)	69.8%
Clothing	11.2 *** (2.9)	43.9%	5.2 (3.3)	20.4%
Energy	-0.4 (3.6)	-3.8%	-5.9 (3.9)	-56.8%
Transport	-11.8 * (5.5)	-99.2%	2 (5.6)	16.8%
Health	-11.9 (9.5)	-28.9%	-61.1 *** (11.8)	-148.3%
Education	-8.7 . (4.7)	-62.8%	-5.2 (6.3)	-37.6%
Other expenses	-8.7 * (3.3)	-67.4%	-2.1 (3.3)	-16.3%

Notes: The triple differences for the different expenditure categories are calculated as illustrated in Table 8 for the category of food. ***/**/*/. stand for significant differences on a 0.001/0.01/0.05/0.10 level. Non-food consumables include expendable articles of daily use such as toiletry, cleaning agents, or detergents. Other expenses include: rates and taxes, expenses for personal services (e.g. haircut and shave), entertainment, phone calls, household and kitchen equipment.

Table 10: Attitudes towards the NREGS, 2008 survey

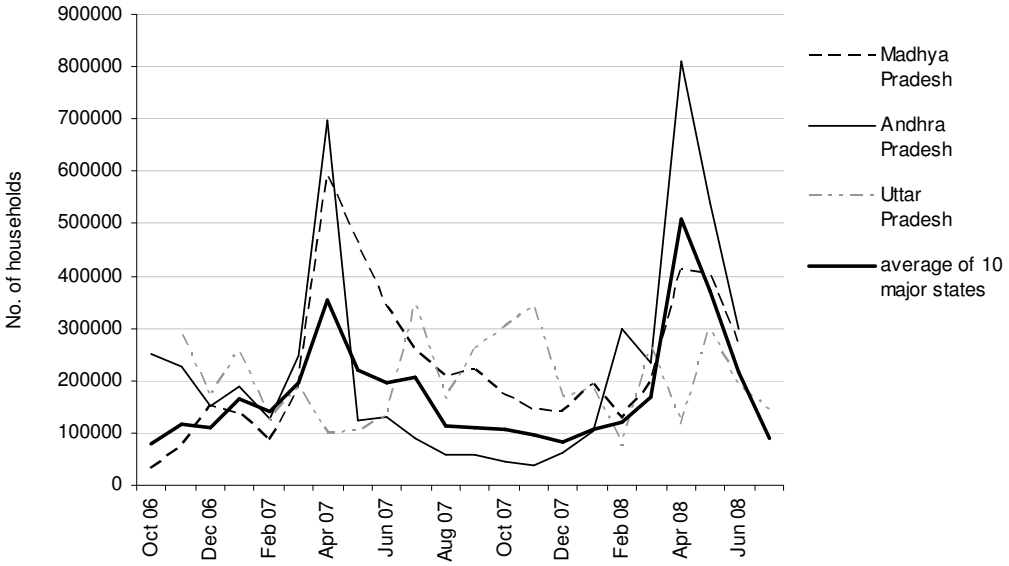
Share of participants who consider the NREGS as a useful program	96.3%	
What are the merits of the NREGS?		
Income substitute when no other work is available	67.4%	
Increased income as more household members can work	12.1%	
Provision of jobs close-by	8.3%	
Improvement of the local infrastructure	6.8%	
Low search efforts to get a job	5.3%	
Changes that would make the NREGS more useful?		
Provision of more working days (no restriction to 100 days)	37.3%	
Faster (daily) payment of wages	35.8%	
Higher wages	11.9%	
Prevention of irregularities and fraud	9.0%	
Higher reliability in providing work (within the entitlement of 100 days)	5.2%	
Share of participants who can influence which projects are realized under the NREGS	52.8%	
What would have been the alternative time use and annual per capita income if the NREGS did not exist?		(mean income)
Non-agricultural employment	78.0%	2329 Rs.
Agricultural self-employment, tending own livestock	15.2%	2436 Rs.
Agricultural employment, tending other's livestock	5.3%	1502 Rs.
Other	1.5%	2170 Rs.
Mean per capita income in 2007		3870 Rs.
What are the reasons for not participating?		
Demanded work but was not provided one	42.6%	
Have enough other work	36.4%	
Health reasons, the work offered is too hard	17.9%	
Payment of the salary is not reliable	1.9%	
Salary is too low	1.2%	

Table 11: Average impact of the NREGS on physical and mental health, 2007 sample (n=1066)

	Participants $\overline{Y_{2007}}\|D_{2007}=1$	Nonparticipants $\overline{Y_{2007}}\|D_{2007}=0$	Single Difference $[\overline{Y_{2007}}\|D_{2007}=1]$-$[\overline{Y_{2007}}\|D_{2007}=0]$	
	abs. value	abs. value	abs. value (std. error)	in % of the average value of the nonpart.
Physical health				
Probability that health status improved in prev. year	0.263	0.212	0.051 (0.037)	24.1%
Indicator of physical health problems, scale from 0 (best) - 42 (worst)	2.61	2.49	0.12 (0.31)	4.9%
Mental health				
Probability that respondent felt worried/ tense/anxious for at least 1 month	0.233	0.310	-0.08 * (0.03)	-24.5%
Indicator of mental health problems, scale from 0 (best) - 42 (worst)	3.98	3.78	0.203 (0.378)	5.4%

Notes: This table shows single differences between health status indicators of the NREGS participants and health status indicators of the propensity-score matched and kernel-weighted nonparticipants. ***/**/*/. stand for significant changes on a 0.001/0.01/0.05/0.10 level based on t-statistics bootstrapped with 500 repetitions. For the definition of the summarizing physical and mental health indicators see the notes in Table 10.

Figure 1: Seasonal patterns of the NREGS employment



Notes: Authors' calculations based on data of the NREGS implementation status reports, MRD (2008b)

Figure 2: Cumulative distribution of the NREGS participants' monthly per capita consumption expenditures, 2007