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Welfare and Poverty Impacts of India's National Rural Employment Guarantee Scheme

Evidence from Andhra Pradesh

Klaus Deininger
Yanyan Liu

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Abstract

This paper uses a three-round 4,000-household panel from Andhra Pradesh together with administrative data to explore short and medium-term poverty and welfare effects of the National Rural Employment Guarantee Scheme. Triple difference estimates suggest that participants significantly increase consumption (protein and energy intake) in the short run and accumulate more nonfinancial assets in the medium term. Direct benefits exceed program-related transfers and are most pronounced for scheduled castes and tribes and households supplying casual labor. Asset creation via program-induced land improvements is consistent with a medium-term increase in assets by nonparticipants and increases in wage income in excess of program cost.

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The authors may be contacted at kdeininger@worldbank.org and y.liu@cgiar.org.

Welfare and poverty impacts of India's National Rural Employment Guarantee Scheme: Evidence from Andhra Pradesh

Klaus Deininger[1] and Yanyan Liu[2]

[1]The World Bank; [2]International Food Policy Research Institute

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1. Introduction

Persistently high rates of extreme poverty and gender inequality, together with increased frequency of natural and man-made disasters, have increased policymakers' interest in public work programs as a form of productive safety nets. The ability to set wages in a way that is self-targeting and fosters gender equality, combined with the opportunity to construct physical infrastructure that can enhance growth and wages in the long term, makes such programs very attractive compared to available alternatives. At the same time, however, there has been concern that implementing these programs successfully carries high administrative requirements and that where these controls are not in place, large amounts of resources may be wasted or end up lining the pockets of local officials.

With a budget of US$7.8 billion in 2011/12 alone, India's Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS) is one of the largest programs of this kind globally.[1] It guarantees employment for up to 100 days per fiscal year at wages that are equal for men and women, thus serving not only as an insurance substitute but also having the potential to enhance female empowerment. Implementation, at least in some states, also includes some innovative features, such as making all project-related data available on the internet, directly depositing payments into beneficiaries' accounts, and regular *social audits* to minimize corruption.[2] While this suggests that NREGS could herald a new generation of such programs, the implementation has not been uncontroversial.

Supporters point to awareness and high participation rates by females (greater than 50 percent) and the poor, that are significantly above those in earlier or comparable programs, as well as anecdotal evidence suggesting that the program has made clear contributions to decentralization, transparency of political

[1] Since 2009, the program is referred to as Mahatma Gandhi National Rural Employment Guarantee Scheme. We use the earlier name throughout the paper

[2] Social audits are audits conducted with active involvement by primary stakeholders. They resulted in discovery of frauds on a significant scale. Some officials embezzled money by "creating fake muster rolls, inflated bills, exaggerated measurements, and non-existent works, all through bribes and cuts from wage seekers" (http://125.22.8.66/SocialAudit/).

processes, and female empowerment (Dreze and Khera 2011; Khera and Nayak 2009). Critics note the program's high cost, inefficiency in transferring resources, and serious corruption (Niehaus and Sukhtankar 2012). They point out that high program wages not only cause leakage and corruption that may undermine program impacts but, instead of helping people move out of agriculture, may in fact encourage return migration to rural areas. To better understand whether these arguments are justified, empirical analysis of NREGS impacts is needed.

Beyond a large body of descriptive and case study evidence, most quantitative studies of NREGS have focused on general equilibrium impacts through price and wage effects, using the program's phased roll-out to identify treatment effects based on repeated cross-sections or administrative data. While this approach has provided important insights, it is not well suited to (1) assessing the extent to which the program and its benefits are targeted toward the poor, (2) identifying the channels through which program effects materialize, and (3) assessing behavioral responses to determine whether, for example, the program crowds out other forms of employment. The use of before/after household panel data allows assessment of program effects on the treated, to answer some of the policy issues raised in this context, but this assessment is demanding in terms of data.

This paper focuses on partial equilibrium impacts of NREGS on direct beneficiaries. We study NREGS impact on key welfare indicators and the channels through which potential impacts materialize, using a three-round panel of some 4,000 households in the Indian state of Andhra Pradesh. The fact that data were collected in 2004, before NREGS had been conceived; in 2006, when the implementation was just starting; and in 2008, when the program was fully operational throughout the state, together with the program's phased roll-out, allows us to distinguish short- and medium-term effects. Data at the household level are combined with administrative data on households' participation and work records.

We find that, in the setting studied, the program was reasonably well targeted and had significant impacts, the magnitude of which exceeded the value of direct transfers. While short-term effects focus on higher nutritional intake, accumulation of nonfinancial assets is visible in the medium term. Benefits are concentrated with scheduled castes and tribes and those relying on casual labor. Land-related investment is one plausible channel for medium-term benefits to materialize, and there is little evidence of NREGS crowding out other types of employment or investment.

The paper is structured as follows. Section 2 describes key features of NREGS, its implementation in Andhra Pradesh, and evidence on its impact from the literature. Section 3 introduces administrative and household survey data, descriptive statistics, and our methodology. Section 4 presents estimates of program impacts in the short and medium term, heterogeneity of impacts by caste and labor market

participation status, and impact pathways via labor markets and land-related investments. Section 5 concludes by drawing out implications for policy and further research.

2. Program Nature and Existing Evidence

Although NREGS quickly became a flagship program for India's government, states—which by law are responsible for implementing it—diverged widely in their approaches and ability to use the program as a tool to improve the welfare of the poor, with some states using innovative ways to increase transparency and accountability and ultimately the program's welfare impacts. Andhra Pradesh is of interest in this respect due to a number of innovative features. Still, partly due to data limitations, evaluations of direct program effects are scant, often based on doubtful identification assumptions, and unable to fully appreciate the heterogeneity of impacts.

2.1 Program Design and Implementation Modalities

Following passage of the Mahatma Gandhi National Rural Employment Guarantee Act in 2005, the NREGS was rolled out across all of India's rural areas, proceeding from the poorest to more affluent districts in three phases that started in February 2006, September 2007, and April 2008, respectively. Program expenditure increased from the equivalent of US$2.1 billion in 2006/07 to US$8.9 billion in 2010/11, providing payment for nearly three billion workdays. While responsibility for the allocation of funds to specific projects lies with the states, the central government budget covers 100 percent of wage and 75 percent of nonwage expenditures.

Building on lessons from a long tradition of food-for-work schemes (Dutta et al. 2012b; Subbarao 1997), NREGS features important innovations (Khera and Nayak 2009). First, it establishes a legal right for households to be employed for up to 100 days per year; in fact, individuals who apply but do not receive work within a period of two weeks are entitled to unemployment compensation. Second, the minimum wage rate, set at the state level, applies equally to males and females, making the program particularly attractive to women, who normally receive significantly lower wages than men (Deininger, Jin, and Nagarajan 2013). Amenities such as crèches, which by law must be provided at the work sites to encourage women's participation, can further reduce gender discrimination. Third, to improve productive capacity of rural areas in the long term and thus make the program sustainable, there is a desire to focus work on productive infrastructure such as irrigation systems, minor roads, and land improvement.

Local governments (*gram panchayats*) and village assemblies (*gram sabhas*) have far-reaching responsibilities regarding implementation and supervision of the program. These include preparation of a list of projects to be undertaken, supervision of ongoing projects, identification of potential interested workers, assignment of these workers to specific work sites, and management of financial flows. To

participate in NREGS, rural households first need to be registered at the local *gram panchayat*, which results in the issuance of a job card and entry of the applicants' names into a register of all job seekers called the *muster roll*.[3] Once work has been performed, workers are to be paid within a period of two weeks or less. In practice, these regulations are not always followed, and performance varies enormously across states (Comptroller and Auditor General of India 2008). Reviews of the program found that many job seekers were unable to obtain the desired level of work, at least initially (Dutta et al. 2012b). Local decisionmakers were found to use NREGS strategically to maximize rents (Niehaus and Sukhtankar 2012), consistent with wide variation in quality and transparency of implementation across the country.

An emphasis on promoting self-help groups among poor women that started in the late 1990s is likely to have allowed Andhra Pradesh a head start in implementing NREGS. Strong self-help group coverage, a federated structure, and various efforts to promote convergence with local government (Deininger and Liu 2013) allowed quick mobilization of the target group once NREGS became effective. Importantly, all self-help groups had elaborated "livelihood plans" to identify opportunities for small-scale labor-intensive investment that could be used as the starting point for a list of projects to be implemented under the program.

After implementation of a predecessor program was marred by high levels of corruption (Deshingkar and Johnson 2003), the state of Andhra Pradesh took distinct measures to hold officials more accountable (Aiyar and Samji 2009). First, key program information (muster rolls, lists of work performed and wages paid) is made available online for access by the public, making it easy to trace participants, work sites, and payments. Second, bank accounts were opened for all participants, and modern payment systems are used to reduce fraud and transaction costs while at the same time encouraging saving. By ensuring that payments are made to the individuals who did the work, the use of smartcards has, according to some observers, improved female empowerment (Johnson 2008). Finally, to quickly identify deviations from the rules and hold responsible officials to account, social audits featuring active involvement by a wide range of stakeholders are conducted regularly in all the state's administrative units.[4] These features, many of which were subsequently incorporated into the national regulations for program implementation, have led the state to be considered one of a few 'star performers' in terms of the quality of program implementation (Dreze and Khera 2011).

[3] As per the program regulations, the job card, which must contain photographs of all the adult members of the household, is to be delivered to applicants free of charge within 15 days of application. In principle, once a household has a job card, that household is expected to indicate demand for work (less than or equal to 100 days) under NREGS for the following year. Based on household demand as ratified by the village meeting (*gram sabha*), a work plan at the *gram panchayat* is elaborated and submitted upward for consolidation. Projects are sanctioned at the district level, and the *gram panchayats* are responsible for the allocation of work among job seekers. In practice, the process is often more top-down, based on central budget allocations, and even information about available projects is not always available to job seekers.

[4] During social audits, several frauds were uncovered. The responsible parties were punished, and funds amounting to Rs. 130 million (US$2.5 million) were recovered. Some officials embezzled money by "creating fake muster rolls, inflated bills, exaggerated measurements, and non-existent works, all through bribes and cuts from wage seekers" (www.socialauditap.com).

2.2 Approaches to and Evidence from NREGS Evaluations

Not surprising, in light of the program's size and importance, a large literature aims to assess the impacts of NREGS. Descriptive evidence suggests that the quality of implementation varied across states (Liu and Barrett 2013) but that the program seems to have allowed households to mitigate the impacts of consumption shocks arising, for example, from rainfall variations (Coffey, Papp, and Spears 2011), and to deal with large and covariant swings in asset prices (Johnson 2009). As program wages, relative to what could be obtained in the market, are more attractive for females than for males, it is not too surprising to see positive program impacts on females at the descriptive level, with knock-on effects on their offspring (Dev 2011).

While the program effectively targets the poor, significant rationing remains (Dutta et al. 2012a), so that some benefits may be captured by elites (Niehaus and Sukhtankar 2012), thereby reducing the program's effectiveness in transferring resources to the poor (Shankar, Gaiha, and Jha 2011). This is consistent with the finding that access to information significantly affected poor people's ability to benefit from the program (Jha, Bhattacharyya, and Gaiha 2011b), and the presence of a positive association between landholding and NREGS that results in less poverty targeting (Jha et al. 2009).

Establishing a control group to more rigorously assess impacts is made difficult by the fact that the program now operates nationally and that phasing in was not random but instead gave preference to poorer districts. A number of studies use the phasing in of the program to assess district-level impacts of the program, often relying on repeated cross-sections of National Sample Surveys (NSSs). To the extent that the underlying assumptions are justified, this would provide an estimate of the *intention to treat* effect of the program on wages or employment at the district level.

One contribution using this approach (Imbert and Papp 2011) finds that the program provided direct and indirect benefits and that direct gains and indirect gains (via general equilibrium effects) are of similar magnitude. The quality of implementation varies significantly across states, as indicated by the fact that estimated program effects almost double (to 9 percent) in the states with the best implementation performance. Increases were focused on low-wage, low-skilled public employment; in fact, wages for better-paying jobs decreased. Seasonality in wage labor demand was also important: the average daily earnings of casual laborers increased by 4.5 percent during the dry season but were unaffected in the rainy season. This result implies that program-induced wage increases redistribute income from net buyers to net suppliers of labor but that the impact on labor force participation remains limited. Using the same framework, Azam (2012) finds that increases in female wages are larger than increases in male wages. Wages for female casual workers were estimated to have increased 8 percent more in NREGS districts as compared to non-NREGS districts.

One issue that might affect the credibility of these estimates is that with nonrandom program placement, the assumption of parallel trends between treatment and control may not hold. To account for this, some studies have used an index by the Planning Commission that in some sense defines program eligibility by ranking districts by poverty. Using this index in a discontinuity framework suggests that NREGS has had limited impact on male wages and levels of employment but some effects on females, with wage impacts concentrated in the agricultural off-season (Zimmermann 2012). NSS consumption data from 2005 and 2008, used to construct a nationwide district panel (188 in phase 1 and 103 in phase 2), allows researchers to assess the program's impact on poverty gaps and consumption patterns, suggesting effects on welfare, especially for scheduled castes and tribes, and nonfood spending (Klonner and Oldiges 2012).[5] As NSS data lack information on wages in agriculture, use of administrative data on gender-specific wages for agricultural and unskilled tasks (at the district level) could allow more direct inference of agricultural wage gaps, which are most relevant for the poor. Results from this analysis suggest that the program affects unskilled wages but leaves the gender wage gap unaffected (Berg et al. 2012).

Beyond possible general equilibrium effects, impacts on participating households can help identify ways in which benefits from NREGS participation materialize. Such effects can, in principle, be identified using panel data for households unable to participate initially. Relying on a small household panel dataset in one district in Andhra Pradesh, Ravi and Engler (2012) apply a pipeline to compare participants to households that were denied access and find that NREGS had large impacts on total, food, and nonfood per capita expenditure (found to have increased by 9.6 percent, 23 percent, and 17 percent, respectively). NREGS participation is estimated to have increased the likelihood of a household having a savings account by 21 percent and total savings by Rs. 19 (Ravi and Engler 2012). Because the baseline survey was conducted at a time when the program was already available, the credibility of the results depends on the fact that initial rationing of nonparticipants was indeed exogenous. Another study that relies on a large sample focuses on education and social outcomes instead and finds that higher female participation in NREGS (instrumented by *mandal*-level rainfall shocks) increased girls' time spent in school, grade progression, and female bargaining power (Afridi, Mukhopadhyay, and Sahoo 2012).

3. Data, Descriptive Evidence, and Approach

To assess program impacts, we combine panel household survey data from before and after the program became available, together with administrative data on participation, using double and triple difference estimates together with propensity score matching. The phased introduction of the program allows us to distinguish short-term from medium-term effects, overall as well as for subgroups in the population.

[5] Both the Planning Commission's "backwardness" index and the intensity of implementation (as measured by the number of days actually worked) are used to control for this.

3.1 Data Sources and Variable Construction

We combine a three-round panel household survey with administrative data. The household survey includes information on some 4,000 households in 480 villages from five districts in Andhra Pradesh that were interviewed in 2004, 2006, and 2008.[6] This allows us to use the 2004 and 2006 household survey rounds as a preprogram baseline to obtain double and triple difference estimates of program effects.[7] Moreover, three of the sample districts were covered by NREGS in 2006 under phase 1, so the 2008 survey data can be used to draw inferences regarding medium-term effects of NREGS. The remaining districts were included in phases 2 and 3, so the 2008 data are indicative of short-term program effects.

The household survey includes information on demographic status, participation in casual labor markets (including NREGS), spending on food and nonfood items,[8] asset endowments, and investments in land. Survey information plus a qualitative exercise was used to assign households by poverty status either to the poorest of the poor, the poor, the not-so-poor or the non-poor.[9] Inclusion of the job card number in the 2008 survey provides a link between our household-level data and administrative records on program participation. Administrative data, available online, include job card information for all (registered) wage-seeking households; muster roll information such as wage rate, total workdays, and payments for each worker; and characteristics of all NREGS works, including their completion status.

The household data allow us to use changes in nutritional intake as a measure of short-term program effects and asset endowments and land-related investment to capture medium-term effects. We measure nutritional intake by multiplying physical quantities of the more than 30 food items in the questionnaire's consumption section with their caloric and protein content based on India's main reference (Gopalan, Rama Shastri, and Balasubramanian 2004) to compute calories and protein consumed.[10] Nonfinancial assets include consumer durables, equipment, and livestock.[11] Consumption and assets are in per capita terms based on adult equivalent measures throughout.[12] Finally, we have information on whether or not a

[6] Villages were randomly selected in these districts, and then households in these villages. The number of sampled households is 4,759 in 2004, 4,693 in 2006, and 4,533 in 2008. The attrition rate is 3.1 percent from 2004 to 2006 and 3.4 percent from 2006 to 2008. We have a total of 4,460 panel households across the three rounds.

[7] Although the 2006 round was collected from August to October, shortly after the launch of NREGS in early 2006, contamination in 2006 is minimal, as only 29 of the 2,467 sample households with NREGS information in 2008 (all in the treatment group) actually worked under NREGS.

[8] Although the survey instrument is less disaggregated than that of the NSS, it follows the overall structure used there.

[9] The poorest of the poor are defined as those who can eat only when they get work and who lack shelter, proper clothing, social respect, and means to send their children to school. The poor have no land, live on daily wages, and need to send school-age children to work in times of crisis. The not-so-poor have some land, have proper shelter, send their children to public schools, are recognized in society, and have access to bank credit as well as public services. The nonpoor have more than 5 acres of land; have no problem obtaining food, shelter, and clothing; can hire laborers, send their children to private schools, use private hospitals, and lend rather than borrow money; and have considerable social status.

[10] For fruits or vegetables where the survey includes only aggregate spending, we use the 55th round of the NSS to derive the price and caloric content of a representative basket of these consumed in Andhra Pradesh.

[11] Asset values were measured as of December 2003 in the 2004 survey, as of June 2006 in the 2006 survey, and as of June 2008 in the 2008 survey. Financial assets were excluded due to concerns about misreporting.

[12] The adult equivalent measures for caloric and protein consumption are obtained using nutritional requirements by sex and age as weights, that is, weights are 1.2 for adult males, 0.9 for adult females, 1.0 for adolescents (12 to 21 years), 0.8 for children aged 9 to 12, 0.7 for children aged 7 to 9, 0.6 for children aged 5 to 7, 0.5 for children aged 3 to 5, and 0.4 for children younger than 3 (Gopalan, Rami Shastri, and Balasubramanian 2004). For income and overall consumption, we assign the weight of 0.78 for anyone older than 60 or younger than 14.

range of land-related investments were undertaken on the household's land and whether, in the case of a positive response, NREGS had contributed to such activity.

3.2 Descriptive Evidence

Table 1 summarizes the evolution of access to job cards and NREGS participation by program phase and household poverty status. In phase 1 districts, some 55 percent of the two poorest groups, as well as 44 percent and 30 percent of not-so-poor and nonpoor households, respectively, had received job cards by 2008. Rates of job card issuance overall were, at 36 percent and 40 percent, slightly lower in phase 2 and phase 3 districts, respectively, but there was greater emphasis on the poor, especially in phase 3 districts, where about 43 percent of the poorest held job cards, compared to 15 percent of not-so-poor households. Actual participation was, at 41 percent of the total (46 percent of the poorest and the poor) and some 50 days supplied by the average household in 2008, higher in phase 1 than in phase 2 (30 percent) and phase 3 districts (19 percent).

We do not find significant differences in wages between males and females, consistent with program regulations. It is thus not surprising to see female levels of participation that are much higher than those of males (63 percent in phase 1, 60 percent in phase 2, and 50 percent in phase 3 districts in 2008). The average total NREGS payment to participant households is higher in Phase 1 districts than that in Phase 2 and 3 districts in 2008 (Rs 4103 in Phase 1 versus Rs 1540 in Phase 2 and Rs 955 in Phase 3).

Table 2 reports percentage of villages with different types of NREGS projects having been completed by 2006, 2007, and 2008 for all phase 1, phase 2, and phase 3 villages in our five sampled districts. A total of 88 percent, 55 percent, and 1 percent of phase 1, 2, and 3 villages, respectively, had completed at least one NREGS project by 2008. The most common types of works were irrigation and land improvements, which, by 2008, had been taken up by 76 percent and 75 percent, respectively, of phase 1 villages.

Appendix Table A1 summarizes household welfare indicators in phase 1 districts by participation status in 2004, 2006, and 2008. Participant households are those with at least one member participating in NREGS. NREGS participants had lower consumption, assets, and energy and protein intakes than nonparticipants in each of the three years. Appendix Table A2 report the results from logit regressions of NREGS participation in Phase 1 districts and Phases 2 and 3 districts, clustered at the village level. The results suggest higher participation by the poor, scheduled castes and tribes, casual laborers, and those with lower initial consumption. Literacy, male headship, and holding a leadership position in the village are associated with higher participation levels in Phase 1 districts. While this suggests pro-poor targeting, village leaders are likely to affect the allocation of work, and a lack of program awareness by illiterate people seems to constrain participation in phase 1 villages.

3.3 Analytical Approach

NREGS participation can, in principle, yield three types of direct benefits. First, a transfer effect will directly increase income by either paying higher wages than those received in the market, especially for females, or providing employment at times when there is no demand from other sources. The size of the effect will depend on the extent to which supply to the casual labor market increases compared to the without-program situation. Second, some of the income gained can be channeled toward savings and investment to strengthen households' resilience in the longer term, an effect that may be enhanced if wages are deposited into a savings account. Third, as NREGS aims to increase the productivity of resource use and small-scale works on participants' own land that is eligible for program support, investment on marginal lands to enhance agricultural productivity can be another avenue for program impacts to materialize, though these would not directly benefit the landless.

We define direct beneficiaries as eligible households that had at least one member work under NREGS and use difference-in-difference (DID) and triple difference (DDD) methods, together with propensity score matching (PSM), to provide estimates of NREGS impacts on these beneficiaries. To illustrate the DID approach, let *t* = 0, 1, 2 indicate year 2004, 2006, and 2008, respectively. Let $T_{it} = 1$ if a household *i* is treated at *t*, and $T_{it} = 0$ otherwise. With Y_{it}^T as the outcome under treatment and Y_{it}^C the counterfactual outcome, the gain from being treated is $(Y_{i2}^T - Y_{i2}^C)$. Our interest is in the average effect of treatment on the treated (ATT), $E(Y_2^T - Y_2^C \mid T_2 = 1)$, that is, the expected difference between actual and counterfactual outcomes, Y_2^T and Y_2^C, for treated households ($T_2 = 1$). Since Y_2^C is unobservable, we cannot estimate ATT directly.

DID estimates, $E(Y_2 - Y_1 \mid T_2 = 1) - E(Y_2 - Y_1 \mid T_2 = 0)$, provide an unbiased estimate of ATT if the parallel trend assumption, $E(Y_2^C - Y_1 \mid T_2 = 1) = E(Y_2 - Y_1 \mid T_2 = 0)$, holds. Defining the selection bias at *t* as $B_t = E(Y_t^C \mid T_2 = 1) - E(Y_t^C \mid T_2 = 0)$, the parallel trend assumption is equivalent to $B_1 = B_2$, or selection bias being constant in 2006 and 2008. This condition will not hold if household characteristics or initial conditions affect subsequent changes of the outcome variables so that their distributions in the treatment and control groups differ from each other.

Combining DID with PSM can address the bias from observables and time-invariant unobservables but not time-variant unobservables. Two rounds of pre-intervention data allows us to test if the parallel trend assumption holds for 2004–2006. The null hypothesis is $E(Y_1 - Y_0 \mid T_2 = 1) = E(Y_1 - Y_0 \mid T_2 = 0)$, or

$B_0 = B_1$. The rationale is that if the selection bias was constant in 2004 and 2006, we can be confident that it was also constant in 2006 and 2008.

To match participants, we use a propensity score (PS)-matched kernel method, which estimates $\left[\sum_{D_i=1}(Y_i - \sum_{D_j=0} W_{ij}Y_j)\right]/N_1$, where N_1 is the number of treated villages, W_{ij} is the weight for villages *i* (treated) and *j* (untreated), and $W_{ij} = G[(P(X_j) - P(X_i))/b_n] / \left[\sum_{D_k=0} G[(P(X_k) - P(X_i))/b_n]\right]$, where $G(.)$ is a kernel function and b_n is a bandwidth parameter. We use bootstrapping with 200 replications to estimate the standard errors for the PS-matched kernel method. We choose the PS-matched kernel method instead of the more commonly used nearest-neighbor matching to obtain valid bootstrapped standard errors (Abadie and Imbens 2006a, 2006b). We also trim off the observations with a PS lower than 0.1 or higher than 0.9, following Crump et al. (2009).

For the DDD estimator, we compute

$$DDD = E[(Y_2^T - Y_1) - (Y_1 - Y_0) \mid T_2 = 1] - E[(Y_2 - Y_1) - (Y_1 - Y_0) \mid T_2 = 0]. \quad (1)$$

This can be rewritten as $DDD = E[Y_2^T - Y_2^C \mid T = 1] + (B_2 - B_1) - (B_1 - B_0)$. The identification assumption of DDD is thus $B_2 - B_1 = B_1 - B_0$, that is, the selection bias between period 1 and period 2 is equal to that between period 0 and period 1. In contrast to the identification assumption of DID, we allow subsequent changes of counterfactual outcomes to differ between the treated and the control households and only assume the difference of the subsequent change over the two periods to be identical between the treated and the control households. This assumption is at least as good as the assumption for DID with the parallel trend test passed. To see this, note that the assumption for the latter is that $B_0 = B_1$ implies $B_1 = B_2$, which is a sufficient though not a necessary condition for DDD. In other words, the DDD condition holds if the assumption for DID with the parallel trend test is satisfied and may hold even if the latter is rejected. To account for possible remaining bias due to the interaction between observables and the difference of subsequent changes over the two periods, we combine DDD with PSM as described.

4. Empirical Results

Our results point toward a program-induced rise in energy and protein intake in the short term and increased accumulation of nonfinancial assets in the medium term. Both effects are more pronounced for scheduled castes and tribes, in line with pro-poor targeting of NREGS. They are also more evident for

households with at least one casual laborer, with potential spillover effects—in terms of asset accumulation, which may be explained by higher levels of investment in land improvement by program participants—to households with no casual laborers. This could be a channel for NREGS to trigger a sustained increase in labor demand, consistent with our rejection of the hypothesis that NREGS crowds out other types of informal employment or private land investment activities.

4.1 Estimates of Program Impacts

Results from DID estimates with and without PSM for medium-term (in phase 1 districts) and short-term (in phase 2 and 3 districts) impacts are in the two top and bottom panels of Table 3. The 2006–2008 data provide an estimate of program impacts, while 2004–2006 data serve as a test for the assumption of parallel trends.[13] For the medium term, DID results from 2006-2008 data point toward a positive impact on consumption and formation of assets, while results from DID plus PSM suggest a significant impact on accumulation of nonfinancial assets only. For 2004–2006, DID and DID plus PSM reject the parallel trend assumption: both suggest that pre-program changes in consumption and nutritional intake were lower for participants than for nonparticipants, a finding that should not come as too much of a surprise, as participants self-selected into NREGS. As a result, DID may underestimate actual program impacts, motivating use of the DDD approach. In the short term (phase 2 and 3 districts in the bottom panels), DID and DID plus PSM suggest positive effects of NREGS participation on protein intake and fail to reject the parallel trend assumption, possibly due to lack of power.

The results from simple DDD and DDD plus PSM, as reported in the top panel of Table 4, point toward significant and positive medium-term impact of NREGS participation on consumption expenditure and asset accumulation. With an annual increase of 11 or 7 percentage points, or Rs. 1,261 or 943, for the two methods, respectively, estimated gains exceed the magnitude of NREGS-related cash transfers to participants in per capita term (Rs. 570 overall or Rs. 672 in 2008). The estimated NREGS-induced increase in asset accumulation of 35 to 40 percent is large as well. Evidence from phase 2 and 3 districts points toward significant NREGS impacts—of 11 percent and 12 percent—on energy and protein intake, respectively, supporting the notion that most immediate program impacts involve improving nutrition, as suggested by others (Jha, Bhattacharyya, and Gaiha 2011a), possibly followed by asset accumulation in the medium term.

To explore whether, as suggested by other studies and descriptive data, NREGS disproportionally benefits the marginalized, we repeat the above analysis for scheduled castes and tribes compared to others. We

[13] Dependent variables include total value of consumption, caloric and energy intake, and nonfinancial assets in levels and logs, with the latter providing an estimate of the percentage change in the outcomes of interest. One advantage of the latter, in addition to being more robust to local inflation, is that it pulls in outliers and changes the distribution of outcomes so as to give poorer households greater weight.

indeed find that significant medium-term effects on energy intake and accumulation of nonfinancial assets (as well as protein intake) emerge for scheduled castes and tribes (Table 5 top panel). Medium-term benefits to other castes are limited to higher levels of consumption (Table 5, panel 2).. Similarly, in the short term, DDD plus PSM points toward higher levels of growth in consumption, energy, and protein intake due to NREGS benefits that are exclusively concentrated among scheduled casts and tribes (Table 5, panel 3). In addition to supporting the notion of effective self-targeting, this outcome points to the need for detailed exploration of pathways through which program-induced impacts may come about.

4.2 Impact Pathways through Labor Markets

As direct program effects will be transmitted through casual labor markets, we can check the plausibility of our results by using DDD methods to analyze medium- and short-term direct impacts on households with and without members primarily engaged in casual laborer in the initial period (Table 6). The top panel suggests that, in the medium term, NREGS benefits households that rely on casual labor through higher levels of consumption, intake of energy and to some extent protein, and asset accumulation. By comparison, the only medium-term impact on households that did not participate in casual labor markets was via asset creation, in line with the program's goal to create assets that, by increasing agricultural productivity, can provide a basis for greater demand for casual labor in the long term. In the short term, we find that the only significant impact is an increase, of about 12 percent, in protein intake by participants relying on casual labor (Table 6, bottom panel) with no evidence of short-term effects on those who do not rely on casual labor.

As an additional test of the extent to which NREGS participation works through the casual labor channel, we use 2006 and 2008 panel data to estimate

$$\Delta y_i = \beta_0 + \beta_1 NREGS_i + x_i^0 \gamma + \sum_{(i)} D_{(i)} + u_i, \quad (2)$$

where Δy_i is the change in casual labor income (including NREGS wages) between 2006 and 2008 for household *i*; $NREGS_i$ is an indicator variable that equals 1 if the household participated in NREGS between July 2007 and June 2008, the reference period for casual labor income in the 2008 survey, and 0 otherwise; x_i^0 is a vector of initial period control variables that includes caste, poverty category, literacy, female headship, household size, number of adults, and land holdings; $D_{(i)}$ denotes district dummies; and u_i is a random error term. We also estimate equation (2) using 2004 and 2006 panel data as a falsification test to examine the plausibility of the DID identification. This allows us to not only assess the contribution of NREGS to total casual labor income at the household level but also distinguish households' casual labor income by gender to explore links between changes in casual labor income and NREGS participation for male and female individuals separately.

Regression results in Table 7 suggest that NREGS participation led to a significant increase in casual labor income overall and for male and female participants separately, with estimated magnitudes of Rs. 3,304, 1,797, and 1,522 for total, female, and male casual labor income, respectively. In all cases, the falsification tests using pre-program income levels support the parallel trend assumption. Administrative data put mean NREGS-induced transfers to program participants in the July 2007 to June 2008 period at Rs. 3,340 per household, close to the increase in total casual labor income estimated here (Rs. 3,304). This suggests that NREGS work is unlikely to have crowded out other forms of casual employment and that, contrary to what was found at the national level (Niehaus and Sukhtankar 2012), leakage was limited.

4.3 Impact Pathways through Land Investment

The fact that our survey includes information on land-related investment allows us to explore the extent to which NREGS helped increase agricultural investment. Descriptively, Table 8 displays the share of households that invested in improving their land in the two pre-program periods (June 2000 to December 2003 and January 2004 to June 2006) and when the program was active (July 2006 to June 2008), for those who did and did not participate in NREGS.[14]

Levels of investment were uniformly higher during the program period compared to before, possibly reflecting the impact of NREGS-related investment incentives. While we find no significant pre-program differences in the propensity to invest in land improvement between program participants and nonparticipants in the pre-program periods, this changed markedly once NREGS became available, when the probability of making land investment was 46 percent for participants versus 30 percent for nonparticipants in phase 1, 72 percent versus 36 percent in phase 2, and 55 percent versus 40 percent (though not significantly different) in phase 3 districts. This suggests not only that NREGS fostered land-related investments overall but also that program participation increased the likelihood of undertaking such investments. As the survey asked if investment was supported by NREGS, we can compare the share of land improvement activities with and without NREGS support between households that did or did not participate. We note that 38 percent versus 23 percent in phase 1, 66 percent versus 21 percent in phase 2, and 50 percent versus 34 percent (again insignificant) in phase 3 districts were supported by NREGS.[15]

To explore the impacts of NREGS participation on land investment, we estimate equation (2) using as a dependent variable the change in land investment between 2006 and 2008. Most other variables are as defined above, and we also conduct a falsification test using 2004 and 2006 data. As earlier, we estimate this for the total sample and subsamples of scheduled castes and tribes and other castes. The results in

[14] Land improvement activities include silt application, borewell creation, land leveling or terracing, establishing an orchard, bunding for erosion control, soil and water conservation, deepening a well or tank, installing a dug well, building or repairing channels, and cleaning bushes and other vegetation.
[15] This may be due to the fact that, as illustrated in Table 2, most projects in phase 3 were still unfinished.

Table 9 suggest, in line with descriptive evidence, that NREGS participation increases the propensity to make land-related investment. The size of the estimated effect, 22.2 percent for all households versus 22.6 percent and 22.0 percent for scheduled castes and tribes and other caste households, respectively, does not vary much across subgroups, suggesting that scheduled castes and tribes do not suffer disadvantages. Lack of significance for the program participation dummy in the regressions with pre-program investment supports the notion that these are program effects rather than pre-program differences.

Appendix Table A3 reports regression results on NREGS-supported land investment. We use the same specification as in equation (2), where the dependent variable becomes the dummy variable indicating NREGS-supported land investment from 2006 to 2008. The results point to a 22.9 percent increase in propensity of NREGS-supported land investment for all NREGS-participating households, 22.6 percent increase for scheduled castes and tribes, and 22.8 percent for other castes. Again, we note that the magnitude of the estimated effect on NREGS-supported investment is close to the magnitude of the estimated effect on all land investment, suggesting that NREGS did not crowd out private investment or investment supported by other projects. We also note that scheduled caste and scheduled tribe participants were not discriminated against in land-related investment activities supported by NREGS.

5. Conclusion and Implications

Our study complements a large literature on general NREGS impacts by exploring effects on participants in one Indian state with a good implementation record. Methodologically, two rounds of pre-program data allow the use of an identification strategy that combines triple difference estimates with propensity score matching. Substantively, we contribute to the literature in a number of ways. First, we show that short-term direct impacts on energy and protein intake differ from the more general investment impacts observed in the medium term. Second, we find that direct impacts are almost exclusively concentrated within scheduled casts and tribes and those with members relying primarily on the casual labor market. Third, the hypothesis of NREGS crowding out other employment types is rejected; program expenditures translate almost directly into additional employment. Finally, investment in land improvement, partly on participants' fields, emerges as a potential pathway for NREGS effects to materialize.

While all this suggests that NREGS is well targeted and has significant impacts, Andhra Pradesh is generally considered to be one of the better-performing states in terms of NREGS implementation. Using similar pre- and post-program data at the household level to extend the analysis to other states where implementation is much weaker could allow researchers not only to measure the aggregate impact of the program but also to better understand the impact of specific implementation arrangements (for example, social audits or electronic funds transfers), an area that would be of great interest to policymakers.

Table 1: Summary of actual NREGS participation by household poverty status

		Phase 1		Phase 2		Phase 3
	2006	2007	2008	2007	2008	2008
Having a job card						
All households	0.438	0.492	0.503	0.322	0.356	0.399
Poorest of poor households	0.492	0.537	0.547	0.295	0.338	0.429
Poor households	0.493	0.540	0.554	0.422	0.463	0.470
Not-so-poor households	0.350	0.427	0.436	0.252	0.273	0.293
Nonpoor households	0.233	0.288	0.302	0.266	0.269	0.146
Participation in NREGS work						
All households	0.049	0.328	0.414	0.035	0.298	0.188
Poorest of poor households	0.056	0.381	0.457	0.038	0.286	0.211
Poor households	0.045	0.355	0.460	0.047	0.382	0.226
Not-so-poor households	0.052	0.260	0.356	0.021	0.230	0.128
Nonpoor households	0.060	0.056	0.102	0.051	0.077	0.000
Number of observations	2,397	2,397	2,397	838	838	751
Female participation (% of time)						
All households	0.536	0.590	0.631	--	0.599	0.503
Poorest of poor households	0.446	0.607	0.650	--	0.632	0.520
Poor households	0.597	0.580	0.606	--	0.586	0.415
Not-so-poor households	0.655	0.595	0.633	--	0.639	0.650
Nonpoor households	0.403	0.456	0.633	--	0.411	0.444
Female wages received (Rs/day)						
All households	84	79	81	--	52	84
Poorest of poor households	78	80	82	--	53	87
Poor households	82	79	81	--	54	78
Not-so-poor households	99	79	82	--	48	84
Nonpoor households	36	78	76	--	47	91
Male wages received (Rs/day)						
All households	80	79	81	--	43	83
Poorest of poor households	85	82	82	--	46	83
Poor households	77	76	81	--	47	83
Not-so-poor households	77	78	80	--	34	80
Nonpoor households	67	79	79	--	32	75
Total amount received per household (Rs)						
All households	796	2,623	4,103	1,907	1,540	995
Poorest of poor households	710	2,674	4,335	2,045	1,558	1,065
Poor households	717	2,665	4,182	1,728	1,703	962
Not-so-poor households	973	2,360	3,687	1,887	1,314	925
Nonpoor households	1,342	2,849	3,027	2,480	1,147	639
Number of observations	117	787	993	29	250	141

Source: Authors' computation from household survey and administrative data.
Note: --: not reported due to small number of observations.

Table 2: Percentage of villages with at least one NREGS project completed by year and phase

	Phase 1			Phase 2		Phase 3
	2006	**2007**	**2008**	**2007**	**2008**	**2008**
Irrigation	0.24	0.60	0.76	0.13	0.46	0.01
Land and soil conservation	0.18	0.61	0.75	0.15	0.37	0.00
Road	0.04	0.12	0.20	0.00	0.00	0.00
Other	0.17	0.54	0.62	0.00	0.02	0.00
Any project	0.32	0.79	0.88	0.25	0.55	0.01

Source: Authors' computation from administrative data.

Table 3: Double difference estimates of impacts from program participation

	DD			DD and PSM		
PHASE 1 DISTRICTS						
2006 and 2008 panel						
Consumption (Rs/year)	711	(288)	**	324	(295)	
Energy intake (Kcal/day)	76	(47)		10	(49)	
Protein intake (g/day)	1.11	(0.87)		0.26	(0.89)	
Nonfinancial assets (Rs/year)	392	(349)		446	(333)	
Consumption (log)	0.079	(0.022)	***	0.026	(0.024)	
Energy intake (log)	0.024	(0.020)		-0.002	(0.021)	
Protein intake (log)	0.019	(0.018)		0.002	(0.020)	
Nonfinancial assets (log)	0.360	(0.062)	***	0.203	(0.062)	***
Number of observations	1,017 + 1,410 = 2,427			967 + 1,249 = 2,216		
2004 and 2006 panel						
Consumption (Rs/year)	-528	(216)	**	-341	(196)	*
Energy intake (Kcal/day)	-184	(43)	***	-86	(40)	**
Protein intake (g/day)	-2.34	(0.83)	***	-1.14	(0.93)	
Nonfinancial assets (Rs/year)	-272	(298)		-211	(227)	
Consumption (log)	-0.027	(0.023)		-0.019	(0.023)	
Energy intake (log)	-0.074	(0.021)	***	-0.036	(0.024)	
Protein intake (log)	-0.044	(0.019)	**	-0.021	(0.020)	
Nonfinancial assets (log)	-0.053	(0.071)		-0.134	(0.077)	*
Number of observations	998 + 1,344 = 2,342			983 + 1,289 = 2,282		
PHASE 2 and 3 DISTRICTS						
2006 and 2008 panel						
Consumption (Rs/year)	80	(367)		144	(354)	
Energy intake (Kcal/day)	66	(68)		115	(72)	
Protein intake (g/day)	1.73	(1.23)		2.50	(1.30)	*
Nonfinancial assets (Rs/year)	15	(376)		30	(393)	
Consumption (log)	0.011	(0.034)		0.016	(0.037)	
Energy intake (log)	0.025	(0.029)		0.051	(0.032)	
Protein intake (log)	0.036	(0.027)		0.058	(0.028)	**
Nonfinancial assets (log)	0.168	(0.135)		0.056	(0.143)	
Number of observations	439 + 1,435 = 1,874			403 + 1,091 = 1,494		
2004 and 2006 panel						
Consumption (Rs/year)	120	(308)		-274	(300)	
Energy intake (Kcal/day)	-16	(57)		-75	(60)	
Protein intake (g/day)	-0.32	(1.12)		-1.76	(1.08)	
Nonfinancial assets (Rs/year)	713	(332)	**	394	(317)	
Consumption (log)	0.048	(0.033)		-0.018	(0.032)	
Energy intake (log)	-0.010	(0.027)		-0.037	(0.028)	
Protein intake (log)	-0.014	(0.026)		-0.043	(0.027)	
Nonfinancial assets (log)	0.303	(0.128)	**	0.185	(0.132)	
Number of observations	430 + 1,297 = 1,727			417 + 1,139 = 1,556		

Source: Authors' computation from household survey and administrative data.

Notes: All figures in per capita terms. As explained in the text, the estimates in the lower panel test the parallel trend assumption. DD = double difference estimation, PSM = propensity score matching. Robust standard errors are in parentheses. Significance level: *: 10%, **: 5%, ***: 1%.

Table 4: Triple difference estimates of impacts from program participation

	DDD			DDD and PSM		
PHASE 1 DISTRICTS						
Consumption (Rs/year)	1261	(428)	***	943	(377)	**
Energy intake (Kcal/day)	252	(79)	***	152	(94)	
Protein intake (g/day)	3.25	(1.44)	**	2.07	(1.54)	
Nonfinancial assets (Rs/year)	623	(443)		772	(403)	*
Consumption (log)	0.107	(0.039)	***	0.068	(0.042)	
Energy intake (log)	0.094	(0.036)	***	0.056	(0.036)	
Protein intake (log)	0.060	(0.032)	*	0.034	(0.037)	
Nonfinancial assets (log)	0.405	(0.111)	***	0.359	(0.118)	***
Number of observations	1,017+1,410=2,427			1,000 + 1,345 = 2,345		
PHASE 2 and 3 DISTRICTS						
Consumption (Rs/year)	-24	(594)		822	(621)	
Energy intake (Kcal/day)	100	(112)		253	(116)	**
Protein intake (g/day)	2.47	(2.11)		5.80	(2.07)	***
Nonfinancial assets (Rs/year)	-412	(507)		113	(481)	
Consumption (log)	-0.032	(0.059)		0.072	(0.062)	
Energy intake (log)	0.043	(0.051)		0.108	(0.049)	**
Protein intake (log)	0.060	(0.048)		0.127	(0.050)	**
Nonfinancial assets (log)	-0.144	(0.229)		-0.113	(0.241)	
Number of observations	439 + 1,345 = 1,784			424 + 1,165 = 1,589		

Source: Authors' computation from household survey and administrative data.

Notes: All figures in per capita terms using the 2004, 2006, and 2008 panel data. DDD = triple difference estimation; PSM = propensity score matching. Robust standard errors are in parentheses. Significance level: *: 10%, **: 5%, ***: 1%.

Table 5: Triple difference estimates of program participation impacts on scheduled castes and tribes versus other castes

	DDD			DDD and PSM		
PHASE 1 DISTRICTS						
Scheduled castes and tribes						
Consumption (Rs/year)	592	(617)		456	(571)	
Energy intake (Kcal/day)	307	(131)	**	324	(152)	**
Protein intake (g/day)	4.56	(2.43)	*	4.46	(2.66)	*
Nonfinancial assets (Rs/year)	1,346	(489)	***	1,323	(599)	**
Consumption (log)	0.092	(0.061)		0.064	(0.066)	
Energy intake (log)	0.128	(0.062)	**	0.142	(0.067)	**
Protein intake (log)	0.093	(0.057)		0.097	(0.062)	
Nonfinancial assets (log)	0.832	(0.186)	***	0.697	(0.202)	***
Number of observations	438 + 413 = 851			432 + 388 = 820		
Other castes						
Consumption (Rs/year)	1,602	(581)	***	1,259	(608)	**
Energy intake (Kcal/day)	188	(99)	*	18	(96)	
Protein intake (g/day)	1.92	(1.80)		-0.11	(1.68)	
Nonfinancial assets (Rs/year)	442	(634)		548	(627)	
Consumption (log)	0.108	(0.048)	**	0.067	(0.051)	
Energy intake (log)	0.058	(0.043)		-0.009	(0.047)	
Protein intake (log)	0.023	(0.039)		-0.018	(0.040)	
Nonfinancial assets (log)	0.218	(0.139)		0.114	(0.137)	
Number of observations	579 + 997 = 1,576			568 + 953 = 1,521		
PHASE 2 and 3 DISTRICTS						
Scheduled castes and tribes						
Consumption (Rs/year)	939	(720)		1,700	(717)	**
Energy intake (Kcal/day)	298	(152)	**	399	(163)	**
Protein intake (g/day)	4.50	(2.95)		7.72	(2.88)	***
Nonfinancial assets (Rs/year)	-465	(577)		-89	(534)	
Consumption (log)	0.107	(0.080)		0.200	(0.087)	**
Energy intake (log)	0.122	(0.071)	*	0.168	(0.077)	**
Protein intake (log)	0.100	(0.070)		0.171	(0.070)	**
Nonfinancial assets (log)	-0.500	(0.272)	*	-0.419	(0.285)	
Number of observations	271 + 517 = 788			263 + 495 = 758		
Other castes						
Consumption (Rs/year)	-1,387	(978)		-682	(1019)	
Energy intake (Kcal/day)	-203	(171)		-5	(170)	
Protein intake (g/day)	-1.96	(3.01)		2.04	(3.06)	
Nonfinancial assets (Rs/year)	-13	(909)		198	(868)	
Consumption (log)	-0.212	(0.082)	***	-0.143	(0.091)	
Energy intake (log)	-0.085	(0.073)		-0.006	(0.076)	
Protein intake (log)	-0.039	(0.066)		0.033	(0.068)	
Nonfinancial assets (log)	0.359	(0.381)		0.214	(0.386)	
Number of observations	168 + 828 = 996			151 + 632 = 783		

Source: Authors' computation from household survey and administrative data.

Notes: All figures in per capita terms. DDD = triple difference estimation; PSM = propensity score matching. Robust standard errors are in parentheses. Significant level: *: 10%, **: 5%, ***: 1%.

Table 6: Triple difference estimates of NREGS participation impacts for households with and without casual laborers

	DDD			DDD and PSM		
PHASE 1 DISTRICTS						
Households with a casual laborer						
Consumption (Rs/year)	1,748	(456)	***	1,293	(479)	***
Energy intake (Kcal/day)	343	(95)	***	223	(95)	**
Protein intake (g/day)	5.31	(1.78)	***	3.54	(1.76)	**
Nonfinancial assets (Rs/year)	1,014	(461)	**	787	(528)	
Consumption (log)	0.157	(0.044)	***	0.094	(0.045)	**
Energy intake (log)	0.138	(0.045)	***	0.091	(0.048)	*
Protein intake (log)	0.099	(0.041)	**	0.062	(0.046)	
Nonfinancial assets (log)	0.459	(0.137)	***	0.360	(0.135)	***
Number of observations	779 + 768 = 1,547			767 + 742 = 1,509		
Households with no casual laborer						
Consumption (Rs/year)	429	(824)		333	(788)	
Energy intake (Kcal/day)	-69	(140)		-120	(129)	
Protein intake (g/day)	-2.92	(2.66)		-3.41	(2.79)	
Nonfinancial assets (Rs/year)	1,674	(860)	*	1,871	(848)	**
Consumption (log)	0.012	(0.067)		0.002	(0.066)	
Energy intake (log)	-0.057	(0.059)		-0.069	(0.059)	
Protein intake (log)	-0.060	(0.058)		-0.065	(0.059)	
Nonfinancial assets (log)	0.521	(0.190)	***	0.409	(0.205)	**
Number of observations	238 + 636 = 874			231 + 582 = 813		
PHASE 2 and 3 DISTRICTS						
Households with a casual laborer						
Consumption (Rs/year)	-58	(642)		293	(669)	
Energy intake (Kcal/day)	51	(126)		209	(127)	
Protein intake (g/day)	2.43	(2.28)		5.78	(2.29)	**
Nonfinancial assets (Rs/year)	-503	(500)		-359	(535)	
Consumption (log)	-0.026	(0.066)		0.037	(0.069)	
Energy intake (log)	0.014	(0.058)		0.086	(0.062)	
Protein intake (log)	0.047	(0.054)		0.122	(0.055)	**
Nonfinancial assets (log)	-0.263	(0.286)		-0.253	(0.305)	
Number of observations	315 + 742 = 1,057			308 + 704 = 1,012		
Households with no casual laborer						
Consumption (Rs/year)	461	(1192)		1,531	(1300)	
Energy intake (Kcal/day)	170	(211)		242	(221)	
Protein intake (g/day)	2.81	(4.15)		3.65	(4.35)	
Nonfinancial assets (Rs/year)	216	(1015)		1,445	(1014)	
Consumption (log)	0.009	(0.116)		0.129	(0.145)	
Energy intake (log)	0.087	(0.094)		0.115	(0.099)	
Protein intake (log)	0.090	(0.091)		0.099	(0.100)	
Nonfinancial assets (log)	0.129	(0.334)		-0.069	(0.371)	
Number of observations	124 + 599 = 723			106 + 371 = 477		

Source: Authors' computation from household survey and administrative data.

Notes: All figures in per capita terms. DDD = triple difference estimation; PSM = propensity score matching. Robust standard errors are in parentheses. Significance level: *: 10%, **: 5%, ***: 1%.

Table 7: Double difference regression to assess impact of participation on casual labor income

	All Individuals		Females		Males	
	2006/08	**2004/06**	**2006/08**	**2004/06**	**2006/08**	**2004/06**
Household participated in NREGS in 2008	3,304.4***	-47.68	1,796.9***	56.16	1,522.0**	-212.9
	(4.53)	(-0.11)	(5.71)	(0.29)	(2.55)	(-0.55)
Household located in hamlet	-683.3	631.8	-209.4	90.09	-748.5	655.9
	(-0.99)	(0.96)	(-0.71)	(0.36)	(-1.24)	(1.10)
Being very poor	4,167.5***	-1,141.1**	1,265.4***	79.56	3,213.7***	-1,241.2***
	(5.18)	(-2.03)	(3.72)	(0.34)	(4.70)	(-2.60)
Being poor	2,731.3***	-248.9	967.9***	263.7	1,950.8***	-582.5
	(3.72)	(-0.48)	(2.94)	(1.24)	(3.11)	(-1.31)
Scheduled tribe	79.91	1,835.3***	164.1	624.0**	165.3	1,194.0**
	(0.09)	(3.04)	(0.39)	(2.25)	(0.21)	(2.44)
Scheduled caste	267.4	-1,098.4	67.10	-280.8	98.24	-983.0*
	(0.25)	(-1.55)	(0.16)	(-0.90)	(0.11)	(-1.79)
Non backward caste	-2,693.2***	-318.5	-1,166.7***	32.48	-2,010.8***	-305.6
	(-3.33)	(-0.59)	(-3.31)	(0.13)	(-2.89)	(-0.67)
If any member can write	915.2	-1,187.3**	-226.3	-456.5**	945.0	-1,013.2**
	(1.05)	(-2.30)	(-0.56)	(-1.98)	(1.33)	(-2.32)
Female headed	-2,151.3**	1,400.2*	-556.3	506.9	738.6	1,543.5*
	(-2.48)	(1.92)	(-1.27)	(1.46)	(0.71)	(1.78)
Number of female adults	392.1	-1,215.1***	658.8**	-468.4**	-108.7	-820.6**
	(0.70)	(-2.79)	(2.19)	(-2.31)	(-0.22)	(-2.22)
Number of male adults	-752.0	527.3	-417.2**	132.8	-511.8	299.0
	(-1.48)	(1.42)	(-2.04)	(1.00)	(-1.07)	(0.91)
Household size	2,251.1***	1,932.4***	706.7***	810.8***	1,369.1*	1,036.3**
	(2.64)	(3.49)	(2.65)	(3.37)	(1.69)	(2.34)
Household size squared	-178.4**	-164.6***	-52.28**	-70.06***	-112.2	-87.02*
	(-2.10)	(-2.86)	(-2.31)	(-2.83)	(-1.40)	(-1.95)
Amount of irrigated land owned (ac.)	-386.3**	176.8	-283.4***	104.1**	-133.3	88.61
	(-2.58)	(1.47)	(-4.57)	(2.16)	(-1.04)	(0.88)
Amount of rainfed land owned (ac.)	-346.0***	45.82	-133.5**	4.388	-227.4**	42.64
	(-2.61)	(0.32)	(-2.25)	(0.08)	(-2.14)	(0.38)
Total land owned square	3.965***	-3.279	1.710***	-0.899	2.449**	-2.424
	(2.82)	(-0.96)	(2.75)	(-0.64)	(2.18)	(-0.89)
District dummies	YES	YES	YES	YES	YES	YES
Number of observations	3,621	3,711	3,468	3,632	3,195	3,358

Source: Authors' computation from household survey and administrative data.
Notes: t statistics are in parentheses. Significance level: *: 10%, **: 5%, ***: 1%.

T**able 8: Investment in land improvement by phase, year, and participation status**

	Phase 1 Participant			Phase 2 Participant			Phase 3 Participant		
	No	**Yes**	**Diff**	**No**	**Yes**	**Diff**	**No**	**Yes**	**Diff**
Period 3 (2006–08)									
Invested	0.30	0.46	***	0.26	0.72	***	0.40	0.55	
…with NREGS support	0.23	0.38	***	0.21	0.66	***	0.34	0.50	
Number of observations	1,305	977		554	260		664	159	
Period 2 (2004–06)									
Invested	0.11	0.10		0.09	0.08		0.06	0.08	
Number of observations	1,192	872		506	236		663	160	
Period 1 (2000–03)									
Invested	0.16	0.15		0.08	0.08		0.48	0.60	
Number of observations	1,367	1,000		581	258		734	163	

Source: Authors' computation from household survey and administrative data.

Notes: Survey periods are July 2006 to June 2008; Jan. 2004 to June 2006; and June 2000 to Dec. 2003. Significance level: *: 10%, **: 5%, ***: 1%.

Table 9: Double difference regression of land investment effects of NREGS participation

	Entire Sample		Scheduled Castes and Tribes		Other Castes	
	2006/08	**2004/06**	**2006/08**	**2004/06**	**2006/08**	**2004/06**
If household participated in NREGS	0.222***	-0.00754	0.226***	-0.00469	0.220***	-0.0128
	(-8.87)	(-0.42)	(-6.21)	(-0.18)	(-6.92)	(-0.53)
If household located in hamlet	-0.00595	-0.00763	0.00458	-0.0402	-0.0116	0.0164
	(-0.20)	(-0.33)	(-0.12)	(-1.20)	(-0.32)	(-0.54)
Being very poor	0.0341	-0.0579**	0.0525	-0.0780*	0.0249	-0.0554*
	(-1.37)	(-2.42)	(-1.09)	(-1.75)	(-0.79)	(-1.83)
Being poor	0.0288	-0.0386*	0.0383	-0.0481	0.0274	-0.0397
	(-1.25)	(-1.84)	(-0.79)	(-1.12)	(-0.97)	(-1.58)
Scheduled tribe	0.0393	0.0102	0.0957**	-0.0311		
	(-1.47)	(-0.47)	(-2.07)	(-1.00)		
Scheduled caste	-0.0544	0.0206				
	(-1.15)	-0.57				
Nonbackward caste	-0.0718**	-0.0408			-0.0619*	-0.0391
	(-2.30)	(-1.56)			(-1.95)	(-1.48)
If any member can write	-0.0552**	0.0133	-0.0343	0.0107	-0.0734**	0.0164
	(-2.12)	(-0.67)	(-0.85)	(-0.39)	(-2.24)	(-0.60)
Female headed	-0.0216	0.0871***	-0.0472	0.0838**	0.000008	0.0959***
	(-0.70)	(-3.71)	(-0.98)	(-2.15)	(0.00)	(-3.36)
Number of female adults	0.0195	-0.0214	0.00101	-0.0169	0.0279	-0.0214
	(-1.06)	(-1.28)	(-0.030)	(-0.69)	(-1.14)	(-1.02)
Number of male adults	0.0155	0.0141	0.0401*	0.0524**	0.0014	-0.00812
	(-1.06)	(-1.1)	(-1.72)	(-2.48)	(-0.08)	(-0.53)
Household size	0.027	-0.0530***	0.00548	-0.0560**	0.0418	-0.0405*
	-1.31	(-3.22)	(-0.17)	(-2.42)	(-1.57)	(-1.89)
Household size squared	-0.0021	0.00482***	-0.0000219	0.00500**	-0.0034	0.00353*
	(-1.16)	(-3.00)	(-0.01)	(-2.23)	(-1.47)	(-1.72)
Irrigated area of land owned	-0.0226***	-0.0354***	-0.0724**	-0.0758***	-0.0142*	-0.0305***
	(-2.91)	(-4.50)	(-2.46)	(-2.98)	(-1.75)	(-3.69)
Nonirrigated area of land owned	-0.0197***	-0.025***	-0.00622	-0.0371**	-0.0202**	-0.0235**
	(-2.77)	(-3.07)	(-0.39)	(-2.16)	(-2.55)	(-2.51)
Square term of total land owned	0.00020**	0.00090***	0.000151	0.0032***	0.000061	0.00079***
	(-2.23)	(-3.33)	(-1.00)	(-3.41)	(-0.74)	(-2.71)
District dummies and constant term	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,390	3,547	1,291	1,338	2,099	2,209

Source: Authors' computation from household survey and administrative data.

Notes: Robust standard errors are in parentheses. Significance level: *: 10%, **: 5%, ***: 1%.

Appendix: Supplementary Tables

Table A1:Means of household outcomes by participation status and year, phase 1 districts only

	2004 Participant		2006 Participant		2008 Participant	
	No	**Yes**	**No**	**Yes**	**No**	**Yes**
Phase 1 districts						
Consumption per capita (Rs/year)	7,401	6,576	9,972	8,549	13,125	12,533
Energy intake per capita (Kcal/day)	2,014	2,004	2,446	2,246	2,542	2,444
Protein intake per capita (g/day)	45	44	50	47	53	51
Total nonfinancial assets per capita (Rs/year)	3,092	2,140	4,446	3,038	6,499	5,601
Number of households	1,383	1,008	1,344	998	1,410	1,017
Phase 2 and 3 districts						
Consumption per capita (Rs/year)	7,401	6,576	9,972	8,549	13,125	12,533
Energy intake per capita (Kcal/day)	2,014	2,004	2,446	2,246	2,542	2,444
Protein intake per capita (g/day)	45	44	50	47	53	51
Total nonfinancial assets per capita (Rs/year)	3,092	2,140	4,446	3,038	6,499	5,601
Number of households	1,383	1,008	1,344	998	1,410	1,017

Source: Authors' computation from household survey and administrative data.

Table A2: Summary statistics and logit regression of NREGS participation, using 2006 data

	Sample Means				Logit Regressions			
	Phase 1		Phases 2 &3		Phase 1		Phases 2 & 3	
	Part.	Nonp.	Part.	Nonp.	Coeff.	t-stat.	Coeff.	t-stat.
Household lives in hamlet	0.34	0.36	0.39	0.30	-0.0591	(-0.61)	0.341**	(2.66)
Household is POP	0.43	0.36	0.40	0.39	0.402**	(3.09)	0.0410	(0.23)
Household is Poor	0.33	0.26	0.38	0.29	0.490***	(3.86)	0.253	(1.48)
Prim occ. casual labor	0.72	0.56	0.77	0.60	0.432***	(4.09)	0.568***	(3.91)
Household is SC	0.34	0.20	0.33	0.14	0.251*	(2.16)	0.978***	(5.84)
Household is ST	0.09	0.08	0.28	0.24	0.0280	(0.16)	0.604***	(3.66)
Household is OC	0.13	0.27	0.13	0.24	-0.309*	(-2.23)	-0.104	(-0.54)
Somebody can write	0.81	0.76	0.75	0.72	0.377**	(3.00)	0.146	(0.94)
Household size	4.27	4.03	4.09	3.93	0.00304	(0.09)	-0.00276	(-0.06)
Head female	0.08	0.12	0.14	0.20	-0.602***	(-3.71)	-0.270	(-1.46)
Leader in VC or SHGs	0.13	0.10	0.10	0.07	0.330*	(2.30)	0.425	(1.95)
Consumption p.c. (Rs/year)	8,549	9,972	8,297	9,177	-0.0000257*	(-1.98)	-0.00000096	(-0.06)
Non-financial assets p.c. (Rs)	3,038	4,446	2,633	2,900	-0.00000547	(-0.75)	0.0000057	(0.57)
Energy intake p.c. (Kcal/d)	2,246	2,446	2,239	2,324	-0.000197	(-1.08)	0.0000320	(0.16)
Protein intake p.c. (g/day)	46.66	50.03	45.20	47.67	0.00791	(0.81)	-0.00500	(-0.47)
Number of observations	998	1,344	340	1,295	2,342		1,635	
Pseudo R-squared					0.076		0.083	

Source: Authors' computation from household survey and administrative data.
Notes: t statistics are in parentheses. Significance level: *: 10%, **: 5%, ***: 1%.

Table A3: Double difference regression of effects of participation on NREGS-supported land investment

	All	Scheduled Castes and Tribes	Other
If household participated in NREGS	0.229***	0.226***	0.228***
	(10.46)	(6.97)	(8.86)
If houshold located in hamlet	0.00279	0.0196	-0.00753
	(0.12)	(0.59)	(-0.28)
Being very poor	0.00837	0.0196	0.0264
	(0.44)	(0.46)	(1.15)
Being poor	0.00558	0.0395	-0.00774
	(0.32)	(0.98)	(-0.39)
Scheduled tribe	0.0539**	0.0986**	
	(2.31)	(2.56)	
Scheduled caste	-0.0111		
	(-0.26)		
Non backward cates	-0.0529**		-0.0364
	(-2.08)		(-1.41)
If any member can write	-0.0268	-0.0283	-0.0377
	(-1.22)	(-0.85)	(-1.35)
Female headed	-0.0104	-0.00298	-0.0215
	(-0.45)	(-0.08)	(-0.76)
Number of female adults	0.0115	0.00378	0.0163
	(0.84)	(0.18)	(0.91)
Number of male adults	0.0165	0.0392**	0.00323
	(1.55)	(2.19)	(0.25)
Household size	0.00489	0.00306	0.0160
	(0.43)	(0.11)	(1.20)
Square term of household size	-0.000541	-0.00148	-0.000881
	(-0.81)	(-0.60)	(-1.32)
Irrigated area of land owned	0.00585	0.00930	0.00177
	(1.27)	(0.48)	(0.39)
Non-irrigated area of land owned	0.0125**	0.0180	0.00555
	(1.98)	(0.79)	(0.85)
Square term of total land owned	-0.000204***	0.000882	-0.000122
	(-2.66)	(0.42)	(-1.65)
District dummies and constant term	Yes	Yes	Yes
Number of observations	3904	1535	2369

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