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Economic and Social Impacts of Self-Help Groups in India

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Abstract

Although there has been considerable recent interest in micro-credit programs, rigorous evidence on the impacts of forming self-help groups to mobilize savings and foster social empowerment at the local level is virtually non-existent, despite a large number of programs following this pattern. The authors use a large household survey to assess the economic and social impacts of the formation of self-help groups in India. They find positive impacts on empowerment and nutritional intake in program areas overall and heterogeneity of impacts between members of pre-existing and newly formed groups, as well as non-participants. Female social and economic empowerment in program areas increased irrespective of participation status, suggesting positive externalities. Nutritional benefit was more pronounced for new participants than for members of pre-existing groups. Evidence of higher consumption—but not income or asset formation—by participants suggests that at the time of the survey, the program's main economic impact had been through consumption smoothing and diversification of income sources rather than exploitation of new income sources. Evaluation of such programs in ways that allow heterogeneity of program impact can yield highly policy-relevant insights.

This paper—a product of the Sustainable Rural and Urban Development Team, Development Research Group—is part of a larger effort in the department to better understand the impacts of decentralized governance and local development. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The authors may be contacted at kdeininger@worldbank.org or yliu3@worldbank.org.

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1. Introduction

In an effort to improve the effectiveness of efforts at reducing poverty, programs that fall under the broad rubric of "community driven development" (CDD) have recently seen tremendous expansion. The amount of World Bank loans under this category alone is estimated to have increased from US$ 0.3 billion per year in the late 1990s to some US$ 7 billion annually in 2006 (Mansuri and Rao 2004, Mansuri and Rao 2007). As a result, CDD is now applied in areas ranging from delivery of agricultural technology and inputs to health care, education, micro-finance, and insurance. The motive for such expansion is a belief in local actors' informational advantages regarding local needs and their ability to monitor outcomes at lower cost. However, especially in stratified societies with few countervailing institutional arrangements, local groups' informational advantages may be more than compensated by the dangers of elite capture. This implies that empirical assessment of the impact of such programs and the factors conducive to attaining outcomes will be desirable.

This paper focuses on a large CDD program in the Indian state of Andhra Pradesh (AP) that is innovative in combining social empowerment with micro-finance intervention. The program focuses exclusively on women and draws on self-help groups (SHGs) as the primary channel for delivering interventions. To do so, it builds on a large infrastructure of pre-existing SHGs that largely focused on micro-credit that was established in the state during the 1990s. To expand coverage of the poor and generate synergies between social and economic development, SHGs' traditional focus on micro-credit is complemented by an emphasis on empowering women socially and economically. The first step in doing so, which is a focus of the evaluation in this paper, is to foster formation of SHGs by targeting the "leftover poor". This is to be followed by efforts to build the capacity of existing SHGs, and establishment of second tier institutions at the village and mandal (county) levels to capitalize on economies of scale in capacity building, credit and insurance, and to ensure that public programs reach the poor.

We aim to evaluate the impact of the program at the household level. To do justice to program objectives, we consider economic and non-economic outcomes. We draw on a 2004 survey of more than 6,000 households in program districts where the program became available in 2001 and control districts where it became available in late 2003. Our data provide rich information on female empowerment, nutritional status, consumption, income, and assets that can be used as measures of the economic and social impacts of the program.

Methodologically, we combine three elements, namely (i) a pipeline setting to create groups that are arguably comparable in unobservables; (ii) propensity score matching on observables over an appropriate

area of common support at village and household levels; and (iii) difference-in-difference estimates for information on assets and empowerment that can credibly be obtained via recall. To allow for (indirect) effects on households in program areas who did not join the program and to distinguish the effects of the program from those attributable to the formation of SHGs prior to its coming into force, we estimate heterogeneous program impact on three sub-groups of households in program areas, namely (i) those who joined new groups under the program (new participants); (ii) those who already participated in an SHG before the program started but converted into a program group subsequently (converted participants); and (iii) those who did not join the program (non-participants). To control for household self-selection into program SHGs, we form control groups using households who were potentially new, converted, and non-participants in the control districts based on their participation status three years after the program being available. Our key identification assumption is that self-selection into different groups of participants is identical in treatment and control districts. Propensity score matching is then used to balance treatment and control on observables. We also estimate mean program effect for households in treatment areas irrespective their participation status. This analysis is free of worries on household self-selection and can be used as a robustness check.

There are three main findings. First, social capital and economic empowerment increased equally for participants and non-participants in program areas, consistent with the notion that the program generated positive social externalities. In fact, our inability to reject the hypothesis of equality of such increases among the three groups implies that the program's impact on increased social empowerment was independent of group participation, pointing towards the project's emphasis on social aspects as an improvement over past efforts at SHG formation. Second, we find a significant impact on nutritional status that differed between new and converted participants. While the former increased their intake of calories and protein by about 9 and 17 percentage points, respectively, the latter show a significant (and, with about 9 points lower) increase only for protein consumption. The fact that these increases were achieved despite prevalence of a severe drought in the state at the time of the survey suggests that the program's provision of mechanisms to smooth consumption and diversify consumption sources together with groups' efforts at improving the quality with which public safety net programs are implemented did have a tangible impact. Third, there is evidence of significant gains in consumption for participants in new groups (17 percentage points) though this is not matched by a commensurate increase in income or assets. The estimation of mean impact suggests positive effects on female empowerment, nutritional status, and consumption, which is consistent with our former findings.

The paper is structured as follows. Section two describes key features of the program, introduces data and descriptive statistics, and discusses questions to explore. Section three describes methodology. Section

four presents the empirical results and the implications for the program. Section five concludes by linking results to the overall literature and drawing out implications for follow-up research.

2. Program description

To understand the intervention at hand and potential synergies between social and economic outcomes that the program was expected to foster, this section discusses key benefits and challenges of a CDD approach, describes key characteristics of the program, and discusses questions to explore.

2.1 Key issues in CDD interventions

To take advantage of superior options for monitoring at the local level and at the same time empower local people, the CDD approach is now used in a wide range of areas that range from delivery of agricultural technology and inputs to health care, education, and micro-finance. Not surprisingly, in view of this wide coverage, there is a considerable variety in approaches, in particular the level of institutional development preceding group formation and its expected trajectory thereafter, the way in which decisions are made, the mechanisms used to allocate project resources between capacity building and provision of public as compared to private goods and the associated outcomes, as well as the level of external checks or independent control mechanisms. Still, the unifying theme in this expansion is a belief in local actors' informational advantages regarding local needs and their ability to monitor at lower cost. Transferring resources to local communities is expected to empower them to take control of their own affairs, improve governance and inclusiveness, build capacity, and increase the efficiency with which resources are used to benefit the target groups (Narayan 2002).

A number of studies note that this assumption may not necessarily hold in practice. For example, it has been observed that in some cases, CDD-type approaches can reinforce existing power structures and rent-seeking rather than empower the poor. In some cases, the ability of those who are better networked to determine the nature and location of projects has allowed them to privately appropriate the majority of resources, and silence any by threatening to deny of future project benefits (Platteau and Abraham 2002). Also, if, instead of channeling support through institutions that run parallel to elected local governments who are responsible for service delivery, project resources may undermine rather than strengthen local capacity and contribute to establishing parallel structures characterized by lower levels of sustainability and downward accountability (Platteau 2004). Even if abuse is avoided, high monitoring cost and low human capital endowments by the poor may limit participation and create opportunities for elites to monopolize access to appropriable benefits (Gugerty and Kremer 2006). While locals will have better access to information on outcomes that can be observed with little effort, they may lack the know-how or

time to carefully monitor the quality of service provision in complex settings, e.g. if quality is not immediately obvious and monitoring characterized by scale economies (Olken 2006, Olken 2007).

All this implies that, without an institutional structure that provides checks, balances, and ways to translate information into sanctions at low-cost, knowledge about abuse may be deflected by those responsible through a combination of co-optation and bullying so that, instead of helping to improve service quality, it may lead to frustration and intra-community strife. Although quantitative assessments are rare, there is evidence that abuse of project resources can have large impact on outcomes; a recent study finds that up to two-thirds of the resources transferred to communities were dissipated in rent seeking. Together with potential negative externalities -in one case project managers could, after taking their cut, only afford to provide sick goats that infected and eventually killed much of the healthy ones- this can almost wipe out project benefits (Ensminger 2007). Careful assessment of the impact of CDD programs and the way in which these relate to complex local institutions will thus be of great importance (Conning and Kevane 2002).

2.2 Program characteristics

We aim to evaluate the first three years' implementation of the first phase of the IKP (*Indhira Kranthi Patham*) program in Andhra Pradesh, also known as DPIP.[1] The program, which aims to build on AP's tradition of SHGs to promote economic and social empowerment in a mutually reinforcing way, was supported by a US$ 110 million World Bank loan. A first phase, launched in October 2000, aimed to promote and strengthen self-help groups in the state's six poorest districts.[2] A second phase, called RPRP (Rural Poverty Reduction Project), drew on a US$ 150 million loan to expand coverage to the state's remaining 16 districts starting from July 2003.

At the start of the program, significant efforts were exerted in social mobilization to expand the outreach of SHGs beyond existing members to include the poor. To identify the target population, the state's 2001 "below poverty line" census -routinely used to determine eligibility for government programs- was complemented by a large effort of participatory identification of the poor (PIP) that added vulnerability and social exclusion to quantitative census indicators and had the resulting lists confirmed by village assemblies. To enable participation by marginalized women from tribal areas and untouchable castes, campaigns on social issues to overcome caste and class barriers within the villages were launched and

[1] DPIP is the acronym for District Poverty Initiatives Project. The program was implemented by mandal, an administrative unit between village and district in the six districts of Chittoor, Srikakulam, Adilabad, Vizianagaram, Mahabubnagar and Anantapur. It is implemented through an NGO, the Society for the Elimination of Rural Poverty (SERP) and builds on earlier interventions, in particular the South Asia Poverty Alleviation Program (SAPAP) which, through a focused formation of SHGs, had implied that in 2002, AP was home to 60% of all SHGs in India.
[2] SHGs typically comprise some 15 women members who meet regularly on a weekly or monthly basis. A key purpose of such meetings, in addition to discussing social and economic issues, is for each member to make a small deposit (R 10-20, equivalent to US$0.25) into a common account. After an initial period of building up its capital stock, normally about six months, the SHG can access loans from the banking sector, normally in amounts proportional to the group's equity.

economic opportunities for their advancement identified by mostly female community activists specifically hired for this purpose. At the same time, existing SHGs set up under earlier initiatives were encouraged to convert to SHGs under this program.

A typical program SHG consists of 10-15 members who meet regularly to discuss social issues and activities, deposit a small thrift payment into a joint account, and make decisions on loans. Loans come from four sources: commercial bank, internal saving, grants provided by the program under a "community investment fund" (CIF), and other line departments. To cater to the needs of the poor, the earlier focus on micro-credit was expanded to include in-kind credit for food, provision of insurance, and empowerment of the most marginalized. Since 2002, the program started to support the federated SHG structure at the village level that gradually took over the distribution of subsidized foodgrain under the public distribution scheme from private operators who had failed to make it available to intended beneficiaries.[3] In addition to improving access to food and risk-coping, this offered a way to attract new members. Furthermore, providing grain as an in-kind credit was expected to help in establishing a discipline of periodic meeting attendance, saving, and (re)payment.

2.3 Data sources and descriptive statistics

We use a survey conducted simultaneously in DPIP and RPRP areas from February to June of 2004. Districts were chosen to represent all the state's macro-regions. Villages were randomly selected in these districts, and then households in these villages with weights applied based on their poverty status from the census of PIP.[4] The household questionnaire which was complemented by questionnaires at group, village, and mandal levels, consisted of male and female parts administered separately -and as far as possible simultaneously- to the main male or female person in the household, normally the head and spouse.[5] This yielded useable information on 2,516 households from 250 revenue villages in DPIP districts, and 3,824 households from 409 revenue villages in RPRP districts. This was complemented by census information from the PIP exercise on education and access to assets at the village level, responses from a village schedule and an SHG questionnaire that was administered to 1,239 randomly selected program SHGs in DPIP areas. To interpret these figures, note that the survey followed on a major drought.[6]

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[3] Widespread leakages and mis-targeting associated with the public distribution system are well documented (Kochar 2005) and to the extent that they were able to overcome such inefficiencies in program administration, VOs could actually generate modest resources from implementing this program.

[4] Sample districts are Anantapur, Adilabad and Srikakulam for DPIP and Kadapa, Warangal, Nalgonda, Nellore, and Visakhapatnam for RPRP.

[5] For example, information on health, consumption, and female empowerment, among others, was obtained from the female while information on agricultural production was obtained from the male. The survey also included SHG, VO and MS questionnaires which are not used here.

[6] Data by the Indian Institute of Tropical Meteorology at http://www.tropmet.res.in/ suggest that, for two of the three seasons preceding the survey, i.e. the 2003/4 rabi (Nov. 2003 - Feb. 2004) and the 2003 summer crop (March 2003-May 2003) rainfall in AP had been less than half of the historical long-term (1871-2005) average.

At the household level, we construct outcome variables in three areas, i.e. changes in female empowerment; nutritional status; per capita income, consumption, and assets. For the first, social capital, economic empowerment and political participation are distinguished. Female social capital is measured by their self-reported level of trust in individuals of the same or different caste or religion from within or outside the village as well as in government officials and police, all on a 1-5 scale. Our measure of economic empowerment is based on whether a woman can set aside money for her own use, go to the market, to the clinic or the community center, visit friends, or work on fields outsides the village, without asking permission from her husband or other males in the family. In either case, we use principal component method to generate an index based on a single factor.[7] In addition, we measure female political participation based on the frequency of their attendance at village meetings (*gram sabhas*). As these variables were also obtained retrospectively for 2000, our empirical measure is the change over time so as to sweep out fixed effects.

Household consumption includes food and non-food consumption over the past 30 days and more lumpy items over the past year.[8] We compute the amount of calories and protein consumed by multiplying physical quantities of more than 30 food items in the questionnaire's consumption section each with their caloric and protein content based on the standard reference for Indian foods (Gopalan *et al.* 2004).[9] Income includes crop production revenue in each of three seasons, wages, self-employment profits, and sales and consumption of livestock and by-products. Consumption and income are expressed in per capita terms based on adult equivalent measures.[10]

Village characteristics and initial conditions in terms of undesirable social practices in table 1 support the notion that DPIP focused on more backward areas. With 22% as compared to 16% in RPRP areas, the share of tribal households is significantly higher. Only about half (53%) of the DPIP villages compared to some three-quarters (74%) under RPRP have access to a bus and 46% compared to 60% a telephone. While levels of schooling were not significantly different between the two areas, level of female economic activity (from the PIP exercise) was much higher than in RPRP areas and initial social conditions less favorable in DPIP areas as indicated by significantly higher levels of prevalence of caste panchayats (49% vs. 32%) temple prostitution (8% vs. 4%) and untouchability (32% vs. 24%).

Household demographics, initial conditions, and outcome variables are illustrated in table 2 for the whole

[7] The Kaiser-Meyer-Olkin measure, a measure for the sampling adequacy or homogeneity of variables, is 0.91 for the social capital and 0.86 for the economic empowerment index. As measures larger than 0.80 are considered meritorious, this seems appropriate (Kim and Mueller 1978).
[8] Although the survey instrument is less disaggregated than that used by the NSS, it follows the overall structure used there.
[9] For fruits or vegetables where the survey includes only aggregate spending, we use the 55th round of the National Sample Survey (NSS) to derive the price and caloric content of a representative basket of these consumed in Andhra Pradesh.
[10] The adult equivalent measures for caloric and protein consumption are obtained using nutritional requirements by sex and age as weights, i.e., 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 *et al.* 2004). For income and overall consumption, we assign weights to be 0.78 for anyone older than 60 or younger than 14.

sample, DPIP and RPRP areas separately, and our sub-groups of new, converted and non-participants in IKP. In DPIP areas, household sizes are larger and a greater proportion was tribal or lived in hamlets with associated lower levels of access to social amenities. At the same time, there is little difference in literacy. Initial conditions, based on recall, point towards lower quality of housing and lower levels of consumer durables per capita in DPIP areas although more households have access to land, in line with a more agricultural orientation of the latter. DPIP areas' relative social backwardness is indicated by significantly lower initial levels of women's initial social empowerment and social capital. Summary statistics for outcome variables, which may of course be affected by the project, are presented in the bottom panel. Total per capita spending and income were R 6,951 and 5,858 (about US$ 175 and 150), respectively with households in the DPIP having lower income but higher spending than those in RPRP areas.[11] Average per capita caloric and protein intake were 2,052 calories and 47 grams of protein per day, both slightly higher in DPIP than in RPRP areas.

We use a summary of group activities, disaggregated for newly formed and converted SHGs at the start of the program and in 2003, respectively, to motivate hypotheses regarding program impact as well as the need for an evaluation to distinguish between new and converted groups. The elements of greatest interest relate to group discipline as well as the two types of activity, on micro-credit and social activity, advanced by the groups. Improvements in the level of group activity and adherence to rules are most clearly visible from the increased number of groups who met at least monthly, which rose from 48% to 70% overall. While only 23% of newly formed groups then in existence met at this level of frequency in 2001, this had increased to 72% in 2003. Although the share of converted groups adhering to the rule of monthly meetings had also increased, the initial level of 55% was already much higher and, with 65%, the share of groups who met regularly, remained below that of newly formed ones in 2003.

Data regarding the lending portfolio point towards a marked increase in recorded internal lending and access to bank loans and project resources (CIF) while at the same time illustrate the fact that these sources cover very different needs. The share of groups that maintained records for internal lending increased from 28% in 2001 to 55% in 2003, a rise that was largest in the case of new groups (11% to 56%) but also observed in converted ones (32% to 55%), in line with the notion that the project helped to enhance improve group discipline and accountability. The share of groups with access to (project-funded) CIF resources increased in equal measure for converted and newly formed ones. Access to bank loans increased significantly for newly formed groups, from 16% to 37%, but remained virtually unchanged (49% and 50%, respectively) in the two time periods for converted ones. In fact, data point towards clear differences not only in access to but also in size and purpose of loans; with an average size of R 6,000 and

[11] The fact that income was lower than consumption may be due to drought conditions prevailing at the time of the survey that forced households

an estimated 46% of loans that are used for consumption smoothing, internal lending is clearly distinct from bank loans, the size of which is, with R 25,000, more than four times larger on average, as well as CIF resources which amount to almost R 40,000 per loan. Also, with 55%, the majority of internal loans is devoted to consumption smoothing (defined as consumption for food, non-food, health, and life cycle events), compared to much lower shares (of 15% and 23% in 2003, respectively) in bank and CIF loans.

Finally, the data also point towards activities that had the potential to generate positive externalities that would benefit all villagers rather than (as credit access would do) group members only. In addition to awareness-raising events in all program villages to induce group formation (i.e. in 2001 for treatment and in 2003 for control villages), about 21% of groups implemented specific activities in the social sphere to counter discriminatory practices and enhance female empowerment. Specific projects benefiting the whole community to make available in-kind credit for food and marketing services were implemented by 36% and 10% of groups, respectively, in 2003.

2.4 Hypotheses to be explored

The brief review of group activities reinforces the need to account for heterogeneity of program impacts and leads us to formulate a number of hypotheses regarding program impacts. The heterogeneity of expected program impacts across different groups forms a key element of our identification strategy, based on a number of considerations.

First, we expect program impacts on female empowerment for group members and, possibly to a lesser degree, non-participants. Two channels will be most relevant for such impact. One, in terms of social empowerment, is likely to benefit everybody in the community irrespectively of their group membership. It would operate through social mobilization at the start of the program to include poor and marginalized women as well as activities by SHGs to stigmatize undesirable social practices at the community level. A second channel, the impact of which is more likely to be limited to group members, is through regular discussion of social issues and exchange of information in group meetings. Having savings and access to credit may improve women's position within the family and give them more freedom to be involved in social, economic, and political activities.

Second, impacts on increasing consumption are expected to be more pronounced for group members, especially in newly formed groups, but could, for example through the interventions of in-kind credit for food, also arise for non-participants. Participants' nutritional status could, of course, also be improved by higher levels of income or assets (in particular livestock which was one of the main productive investments promoted under the CIF) from activities aiming at income generation under the program.

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to borrow or use savings to smooth consumption. Alternatively, it could be due to under-reporting of some income component.

However, given the apparent slow disbursement of CIF funds, we expect that impacts in terms of increased income and asset accumulation, which would be limited to group members to start with, will be less pronounced.

3. Empirical method

As a phased random roll-out originally envisaged for DPIP areas was not implemented, our identification strategy has to rely on weaker assumptions. We do so by combining pipeline, propensity score (PS) matching and difference-in-difference estimation methods. The pipeline derives from the fact that DPIP and RPRP are two phases of the same program but with distinct geographic scope. At the time of the survey, DPIP had been available to survey villages for about three years while RPRP was just starting, implying that under fairly general assumptions, villages or households in RPRP areas can serve as a control for those in DPIP areas.[12] We use PS matching to deal with differences in observables between treatment and control groups. Furthermore, we complement estimates of the program effect in levels for income, consumption, and nutritional status with those from difference-in-difference estimates based on recall to assess the program's impact on changes in female empowerment and asset endowments.

Before estimating heterogeneous effects for sub-groups of participants and non-participants in DPIP areas, we start with an estimation of mean program effects for households in program areas. In this analysis, our treatment group includes households in DPIP areas irrespectively of their participation status, and the control group includes households in RPRP areas. In order to control for possible non-random placement of DPIP, we use PS matching to deal with the difference in observable village characteristics between DPIP and RPRP.

To make unbiased inferences on the heterogeneous impact for sub-groups, we need to consider the fact that unobservable household characteristics are likely to lead to self-selection into the program. To do so, we draw on a listing that provides us with the 2006 participation status of all the RPRP households included in the earlier survey to identify three groups in both areas, namely (i) *new participants*, i.e. households who did not have a member participate in a SHG when the program was started (i.e. in 2000 in DPIP areas and 2003 in RPRP areas) but joined during the subsequent three year-period of initial program implementation (i.e. in 2003 for DPIP areas and in 2006 for RPRP areas); (ii) *converted participants* who participated in a non-program SHG at the start of the program and subsequently joined the program; and (iii) *non-participants*, i.e. households who participated neither at the start nor three years into the program. We then use new participants in the RPRP areas to form a control group for new

[12] Due to delays in field work organization, the bulk of data collection started between 4 and 6 months after the start-up of RPRP. To the extent that RPRP had affected any of our outcome indicators in the control area by the time of survey implementation, this would lead to a downward bias on our estimates, thus leading us to underestimate the program's true impact. In practice, much of this time was spent recruiting staff for the massive scale-up required.

participants in the DPIP areas. Similarly, converted participants in the RPRP areas form a control for converted participants in the DPIP areas, and non-participants in the RPRP areas form a control for non-participants in the DPIP areas.

After identifying the control groups, we use PS matching to evaluate program impact for each sub-group separately. In addition to village characteristics, we also include a set of household level variables as matching variables. These household level variables are expected to affect outcome and/or program participation. In the appendix, we justify the inclusion of these household variables and derive our key identification assumption: the distribution (conditional on observables) of unobservables that affect both outcome and participation is the same in DPIP and RPRP areas for each of the three groups. In other words, the self-selection is the same in DPIP and RPRP. The fact that DPIP and RPRP constitute two phases of the same IKP program and the equivalence in survey timing provide a strong argument in favor of this condition to hold. Knowledge spillovers whereby households in RPRP areas learn about the program and plans to expand its coverage to include them could prompt adjustment in their behavior accordingly, thus invalidating the condition. However, survey data suggest that this is not a concern; only 6% of respondents in RPRP areas indicated that they had heard about the IKP program at all, but not plans for expanding it, in 2004.

To illustrate the PS matching and trimming method we use, let D=1 if a household was treated and D=0 otherwise and denote corresponding outcomes by (Y_1, Y_0). Letting X be a vector of observables, the underlying assumption of PS matching is that, conditional on X, the outcome if not treated is independent of the actual treatment, i.e., $(Y_0 \perp D \mid X)$. It has been shown that $(Y_0 \perp D \mid X)$ implies $Y_0 \perp D \mid P(X)$ (Rosenbaum and Rubin 1983) where $P(X)$ is the propensity score, defined as $P(X) = \Pr(D = 1 \mid X)$. We use a PS-matched kernel method and a PS-weighted regression method (Hirano *et al.* 2003). The PS-matched method estimates

$$\left[\sum_{D_i=1} (Y_i - \sum_{D_j=0} W_{ij} Y_j) \right] / N_1, \tag{6}$$

where N_1 is the number of treated villages, W_{ij} is the weight corresponding to villages i (treated) and j (untreated), and

$$W_{ij} = G[(P(X_j) - P(X_i))/b_n] / \left[\sum_{D_k=0} G[(P(X_k) - P(X_i))/b_n] \right], \tag{7}$$

where $G(.)$ is a kernel function and b_n is a bandwidth parameter. We use bootstrapping with 100

replications to estimate the standard errors for the PS-matched Kernel method.

The PS-weighted method recovers an estimate of the ATT as the parameter β in a weighted least square regression of the form

$$Y_i = \alpha + \beta D_i + \varepsilon_i, \tag{8}$$

where weights equal one for treated and $\hat{P}(Z)/[1-\hat{P}(Z)]$ for non-treated observations. See Chen *et al.* (2007), van de Walle and Mu (2007) for empirical applications of these two methods.

Since average treatment effect (ATE) can only be estimated consistently in the common support region of *X*, the choice of trimming method is important. We follow Crump *et al.* (2007) to determine the common support region by

$$A_{10} = \{X \mid P(X) \le \lambda\} \tag{9}$$

where $\lambda = 1$ if

$$\sup_X \frac{1}{1-P(X)} \le 2E\left[\frac{1}{1-P(X)} \mid D=1\right], \tag{10}$$

and otherwise solves

$$\frac{1}{1-\lambda} = 2E\left[\frac{1}{1-P(X)} \mid D=1, P(X) \le \lambda\right] \tag{11}$$

This method minimizes the variance of the estimated average treatment effect on the treated (ATET).

4. Estimation results

Applying the methodological framework discussed above to estimate mean program effects suggests that the program did contribute to greater empowerment, improved nutritional diversity and higher levels of consumption, but not improved income. Followed by this, heterogeneous estimates of impact at sub-groups allow us to draw inferences on variation in the incidence of benefits between participants and non-participants within the program areas.

4.1 Average treatment effects

At the village level, the vector *X* includes demographics such as presence of hamlet, caste, size, headship, and literacy; aggregate initial economic conditions such as availability of public bus and telephone, and adherence to undesirable social conditions including caste panchayats, child marriage, untouchability, and temple prostitution. Results from estimating propensity scores across villages as reported in table 4

suggest that DPIP villages have higher shares of low-caste and tribal populations and females who are economically active. Similarly, access to infrastructure is significantly lower and the incidence of caste panchayats much higher. The pseudo R^2 of 0.31 suggests satisfactory predictive power.

While there is considerable overlap in the middle of the distribution, high densities close to 1 and 0, respectively, highlight the need to trim and re-weight to obtain consistent estimates of program impact by focusing on a common support area. Appendix figure A1 plots densities of the estimated propensity score in treatment (DPIP) and control (RPRP) villages as well as the cut-point of the PS values above which villages are trimmed. To illustrate the impact of doing so, table A1 in the appendix displays simple differences between the DPIP and RPRP samples (col. 1) and the PS-weighted and kernel-matched trimmed samples (cols. 2 and 3). Although simple differences between the groups are large and statistically significant, trimming and matching based on the propensity score eliminates any significant differences, balancing all the variables of interest between the two samples.

Table 5 reports estimates for average treatment effects on households in the project area as well as average outcomes for treatment group and weighted average outcomes for controls based on the PS-weighted and kernel matched methods for the trimmed samples, respectively.[13] Results, which are very consistent between the two methods, point to statistically significant effects on female social capital, economic empowerment, political participation, protein intake, and per capita consumption, but not on calorie intake, per capita income or assets.

We find evidence of an economically and statistically significant program impact on female social capital, economic empowerment, and political participation. Estimated impacts on political participation suggest that 5% women attended village meetings (*gram sabhas*) more frequently due to the program. To interpret results for social and economic empowerment, col. 1 of table A3 provides estimated effects using the PS-weighted sample disaggregated by individual index components. Noting that these are double differences, they suggest that the program contributed to an estimated increase of trust in other villagers, elected representatives, or government representatives of between 7 and 22 percentage points. Similarly, the project is estimated to have expanded the share of women who are able to set aside money for themselves by 16 percentage points and of the share of women who have greater freedom to participate in other economic and social activities by between 5 and 11 points, depending on the indicator.

Concerning nutrition, estimates point towards a significant impact on intake of protein – estimated to have increased by about 4 grams or 8% as compared to the counterfactual – due to the program but not on calorie intake. A possible explanation is that the program helped households diversify their diet and

[13] The trimming and weights used for matching are based on estimated PS of villages. Thus all households in the trimmed villages are eliminated and all households in a given village are assigned village-level weights. We also use villages as clusters to control for village random effects.

access food of higher quality, either through interventions aiming to directly improve nutrition or by allowing households to consume products (e.g. milk) from livestock acquired through the CIF funds which the project had made available to about one quarter of SHGs in the treatment area. Alternatively, access to savings and credit could have provided better options for consumption smoothing which may have been important in view of the fact that the survey followed immediately on a drought.

We also find an economically and statistically significant impact on consumption which is higher by about R 500 or 8% compared to the counterfactual. This is remarkable because, to the extent that the project reduced prices (e.g. by cutting margins of monopolistic traders who had earlier exploited distress situations), monetary expenditures would tend to understate true consumption and this is an average effect over participants and non-participants in program villages. At the same time, our estimates do not point to any significant impact on income or accumulation of assets over time. Exploring the heterogeneity of program impacts would be of great interest not only to assess the extent to which estimated program effects differ across different groups of participants but also to make inferences on the nature of such effects and the channels through which they materialize.

4.2 Heterogeneous impact on new, converted and non- participants

We use the matching and trimming methods described earlier but add household-level explanatory variables. Specifically, we assume that household (location in a hamlet, caste, size, headship, and literacy) and village level demographics (caste composition, population, mean educational attainment, and female economic activity) as well as village level social conditions (existence of caste panchayats, child marriage, untouchability, and temple prostitution) explain participation and outcomes. Initial economic conditions at household (female social capital and economic empowerment, housing quality, land ownership, and value of durable assets) and village levels (availability of public transport and telephone) are assumed to affect outcomes but not participation.[14] Villages are used as clusters throughout to control for village-level random effects.[15]

Table 6 reports results from logit regressions of the three types of treatment (new, converted, and non-participants) in DPIP and RPRP areas, respectively.[16] The effect of using either trimmed PS-weighted or kernel-matched instead of simple differences between the two samples is illustrated in table A2. Similar to what was observed earlier for village-level matching, large and statistically significant differences disappear and the balancing property is satisfied throughout. Figure A2 illustrates the distribution of

[14] While we aim to obtain 2001 values for variables in *T*, variables in *Z* are measured at the start of program, i.e. in 2001 in DPIP areas and 2004 in RPRP areas. Variables in both *T* and *Z* are generally time-invariant.
[15] We owe this point to Emanuela Galasso.
[16] Contrary to village level regression reported earlier, regression results do not have an immediate economic interpretation and the main purpose is to statistically balance explanatory variables.

estimated propensity scores for the three sub-groups, reinforcing the need for trimming and reweighting especially in the case of new participants.[17]

Estimates of average treatment effects based on the PS-weighted and kernel matched differences for trimmed samples are reported in table 7. The estimates suggest consistency in results between the two methods and those obtained using village-level matching. As one justification for allowing heterogeneity across groups is to explore heterogeneity in program impact, we discuss results together with evidence from pair-wise *t* tests for the significance of inter-group differences in ATETs as reported in table 8.

A first finding of interest is that increases in female social capital, economic empowerment and political participation are significantly higher for the treated compared to the untreated in all three groups, in line with the notion that social mobilization provides benefits independently of households' participation in SHGs as was indeed intended by the project. For social capital and economic empowerment, the point estimate of the ATT is higher for converted participants than for new and non- participants although the difference is never significant at the 10% level. For political participation, the impact is significantly higher for new or converted participants compared to non-participants. To help interpret the indexes for social capital and economic empowerment, table A.3 compares the differences in their components across groups using the PS-weighted method. It illustrates that the impacts on two of the eight components in economic empowerment are significantly higher for converted participants than for non-participants.

Compared to the lack of significance for calorie intake in the village-level analysis reported earlier, making allowance for heterogeneity of program participants points towards significant increases in the nutritional status of caloric intake as well as protein consumption by new participants but not for those in groups that had been converted or those who did not participate in SHGs. With about 185 kcal and 7.4 grams of protein per day, our estimate of the ATET for new participants points towards a program-induced increase of about 9% and 17%, respectively. By comparison, we find no impact on calorie intake and a smaller effect of about 8% on protein intake for members of converted groups. This is consistent with the notion that households in this group would have had access to consumption-smoothing credit (e.g. through internal lending and/or bank loans) even without the program but that the program allowed households to diversify diets and access food of higher quality. We note that levels of nutritional intake for converted participants in the control group were already higher than new participants. Similarly, we do not find significant program impacts on nutritional intake of non-participants whose consumption levels in the control are already somewhat higher than those of new participants, pointing towards a pro-poor program impact that helped to improve the nutrition of (poor) participants lagging behind initially.

[17] In our data, λ is 0.82 for the group of new participants, 0.74 for converted participants, and 0.76 for non-participants. The households with PS larger the above critical values are trimmed.

Table 8 implies that the difference between new and non-participants in calorie and protein intake was significant. Consistent with village level findings, our results point towards statistically and economically significant program impacts on mean per capita spending for new participants but not for converted participants or those who did not join SHGs. The estimated ATET is about 1,000 R or 17 points for the new participants.[18]

Although the program explicitly aims to increase participants' income and assets, we find no significant differences between treated and controls in these variables for either new or converted participants. This result, which is in line with village-level evidence, implies that, by 2003, the program had not led to a significant increase in incomes or assets of either group of participants. We can think of a number of ways of reconciling this with the evidence of significant impacts on consumption. A first possibility is that the result is due to a short-lived consumption caused by participants consuming the one-time capital injection by the program. Our data which point towards high levels of repayment on project loans, a path of asset accumulation by most groups, and the fact that extrapolating the estimated increase in consumption over the whole project area leads figures that are larger than the net inflow of project resources implies that, although it is a possibility for individual groups that were then disbanded, this is unlikely to be the reason at an aggregate level.[19]

A second possible explanation is that, at the time of the survey, exogenous shocks may have prevented participants from realizing their potential. Given that, as noted earlier, two of the three agricultural seasons preceding the survey were droughts characterized by large crop failures, and as program areas are more dependent on agriculture and less diversified than control areas, this explanation has some plausibility.[20] In addition, noise in income data could bias results towards zero and thus make it impossible to detect program effects on these variables at standard levels of confidence.[21] A third possibility is that, in view of the target group, a key objective of the project is to help smooth consumption while income effects may be expected to materialize in the future. If this were the case, data from a later stage in the program would be needed to assess whether income effects did materialize or not.

5. Conclusion and policy implications

Rather than repeating our results, we conclude by highlighting how this paper adds to the literature and point out the scope for follow-up efforts to provide additional insights. By considering outcomes beyond narrowly defined economic variables, we show that community driven development programs can have

[18] Note that the slightly lower significance level (of 10% as compared to 5%) for new participants could be due to the smaller sample size.
[19] During the period covered by our survey (i.e. June 2003 to June 2006), the average annual net loan receipt per household for program SHG participants was about R 330, which is significantly lower than the estimated increase in consumption.
[20] This is supported by the fact that 1,123 of agricultural households out of the total 2,516 households in the DPIP areas reported zero agricultural income in either of the two seasons.
[21] In our case, the income aggregate has to be constructed using 116 variables rather than only 84 as in the case of consumption.

social as well as economic effects. Allowing for heterogeneity of program impacts points not only towards a significant *average* impact on female empowerment but highlights that this effect is indeed an externality that accrues equally to all residents in program villages, irrespectively of whether or not they participate in a self-help group. The fact that benefits accrue even to non-participants suggests that the program's institutional structure has avoided elite capture at least in this respect. On the other hand, finding a significant empowerment-effect even for those who had earlier participated in self-help groups implies that the focus on social issues and women's empowerment is indeed one of the features distinguishing the program at hand from earlier initiatives. Exploring the interaction between social and economic effects would be of great interest, in particular relating to questions such as whether expectation of financial benefits helps provide incentives for group formation or maintenance; whether groups' provision of social benefits has economic impacts, e.g. by increasing loan repayment or the quality of micro-credit projects implemented; or whether a minimum level of empowerment and social cohesion is needed for successful group functioning.

Our results also suggest that, by helping to improve consumption and nutritional status of the poor, a CDD program can benefit the poor even in the absence of measurable impacts on income and asset accumulation. In addition to the reasons discussed earlier, such as exogenous shocks, a short time of program exposure, and various factors that might exert a downward bias on estimates of impact, this raises the possibility that for some in the target group, consumption smoothing rather than greater income, will be a key benefit from program participation. Use of additional data to explore the nature and magnitude of income- and asset accumulation effects, the heterogeneity of such effects, and reasons that might underlie our finding, will be of great interest to better understand the impact of SHG formation and to make inferences on their longer-term trajectory and economic sustainability. These issues are left for future research.

Table 1: Sample means of village explanatory variables

Variables	Total	RPRP	DPIP
Village Demographics			
Number of households (1000s)	0.61	0.64	0.55
Scheduled tribe (%)	0.18	0.16	0.22
Scheduled caste (%)	0.18	0.21	0.14
Other backward caste (%)	0.45	0.42	0.51
Other caste (%)	0.18	0.21	0.14
Economically active females (%)	0.10	0.02	0.24
Schooling by household head (years)	2.41	2.37	2.49
Initial village conditions			
Availability of public bus	0.66	0.74	0.53
Availability of telephone	0.55	0.60	0.46
If child marriage existed	0.48	0.47	0.48
If untouchability existed	0.27	0.24	0.32
If temple prostitution system existed	0.06	0.04	0.08
If caste panchayats existed	0.39	0.32	0.49
Number of observations	659	409	250

Source: Own computation from CESS impact evaluation survey.

Table 2: Means of explanatory and outcome variables at household level overall and for sub-populations

	Total	RPRP	DPIP	New participants		Conv. participants		Non-participants	
				RPRP	DPIP	RPRP	DPIP	RPRP	DPIP
Explanatory Variables									
Household Demographics									
Household in hamlet	0.21	0.19	0.24	0.20	0.26	0.14	0.26	0.23	0.23
Scheduled tribe	0.17	0.16	0.20	0.19	0.30	0.16	0.30	0.18	0.11
Scheduled caste	0.21	0.24	0.16	0.31	0.18	0.28	0.14	0.19	0.16
Other backward caste	0.45	0.41	0.51	0.33	0.45	0.40	0.47	0.39	0.56
Other caste	0.17	0.20	0.13	0.17	0.07	0.15	0.09	0.24	0.17
Household size	4.39	4.14	4.76	4.27	5.06	4.51	4.98	3.93	4.54
Household female headed	0.12	0.12	0.12	0.12	0.10	0.11	0.11	0.13	0.13
Some member can write	0.76	0.73	0.80	0.75	0.82	0.82	0.84	0.67	0.78
Household Initial Conditions									
Female social capital	0.00	0.75	-1.14	0.44	-1.19	0.68	-1.12	0.62	-1.12
Female econ. empowerment	-0.02	0.09	-0.19	0.14	0.08	0.11	-0.08	0.06	-0.36
Female political participation									
Mud floor	0.54	0.55	0.52	0.55	0.59	0.54	0.62	0.56	0.45
Unprotected water only	0.24	0.18	0.33	0.18	0.38	0.19	0.38	0.16	0.29
Owned any land	0.49	0.40	0.62	0.37	0.66	0.45	0.70	0.39	0.58
Durables p.c. (1,000Rs)	0.54	0.53	0.56	0.43	0.45	0.50	0.45	0.55	0.65
Outcome Variables									
Female Empowerment (change)									
Social capital	0.05	-0.65	1.10	-0.60	1.28	-0.67	1.52	-0.66	0.86
Economic empowerment	0.04	-0.39	0.68	-0.39	0.85	-0.34	0.88	-0.43	0.53
Political participation	0.05	0.03	0.08	0.03	0.09	0.04	0.13	0.03	0.05
Household Consumption, Income (level), and Assets (change)									
Spending p.c. (Rs/year)	6951	6871	7072	6325	6741	6848	7241	6851	7169
Income p.c. (Rs/year)	5858	6795	5462	5843	5163	6104	5434	6168	5673
Asset (Rs)	2532	2278	4406	2806	2502	2806	2502	3253	1685
Nutritional Intake per capita (Level)									
Energy (cals/day)	2052	2031	2104	1956	2075	2031	2097	2046	2095
Protein intake (g/day)	47	46	49	44	49	45	50	46	48
Number of observations	6340	3824	2516	583	656	1149	502	1446	1358

Source: Own computation from CESS impact evaluation survey.

Table 3: Summary of program SHG activities

	2001			2003		
Variable	**Total**	**New**	**Conv.**	**Total**	**New**	**Conv.**
Internal discipline						
Meet at least monthly	0.48	0.23	0.55	0.70	0.72	0.69
Members make savings in meetings	0.89	0.57	0.97	1.00	0.99	1.00
Micro-credit activities						
Maintain records for internal lending	0.28	0.11	0.32	0.55	0.56	0.55
if yes, median loan size	5000	1715	5302	6000	4100	7400
share for consumption smoothing	0.41	0.67	0.39	0.46	0.53	0.41
Have access to CIF	0.04	0.03	0.04	0.23	0.22	0.23
if yes, median loan size	38657	23510	40126	34000	32350	35000
share for consumption smoothing	0.00	0.00	0.00	0.03	0.04	0.02
Have access to bank loans	0.43	0.16	0.49	0.45	0.37	0.50
if yes, median loan size	15000	15000	15000	25000	15000	30000
share for consumption smoothing	0.42	0.48	0.41	0.15	0.04	0.20
Activities with external effects						
Activities for female empowerment	0.01	0.06	0.00	0.21	0.21	0.21
Activities to reduce vulnerability	0.11	0.08	0.11	0.47	0.49	0.46
if access to in-kind food credit	0.00	0.00	0.00	0.36	0.42	0.32
Marketing activities undertaken	0.04	0.01	0.05	0.09	0.07	0.11
No. of observations	775	144	626	1239	490	743

Table 4: Logit regression of treatment at village level

Variables	Coeff.	(s.e.)	Sig.
Village Demographics			
Number of households (1000s)	-0.34	(0.27)	
Scheduled tribe/caste (%)	1.28	(0.67)	*
Other backward caste (%)	2.39	(0.67)	***
Mean education by head (years)	-0.15	(0.15)	
Economically active female	14.26	(2.77)	***
Initial village characteristics			
Availability of public bus	-1.24	(0.22)	***
Availability of telephone	-0.40	(0.22)	*
If child marriage existed	-0.55	(0.24)	**
If untouchability existed	0.23	(0.26)	
If temple prostitution system existed	-0.27	(0.50)	
If caste panchayats existed	1.20	(0.23)	***
Pseudo R-squared	0.31		
Number of observations	659		

Significance of coefficient is as follows: * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Impact of DPIP on households in the project area

Variables	Trimmed sample, PS weighted				Trimmed sample, Kernel matched			
	Treated	Control	Diff.		Treated	Control	Diff.	
Female social capital	1.25	-0.63	1.88	***	1.25	-0.65	1.90	***
Female econ. empowerment	0.52	-0.37	0.89	***	0.52	-0.39	0.91	***
Female political participation	0.07	0.02	0.05	***	0.07	0.02	0.05	***
Energy intake p.c. (kcal/day)	2097	2068	30		2097	2071	27	
Protein intake p.c. (g/day)	49.64	46.04	3.60	***	49.64	46.14	3.50	***
Consumption p.c (Rs/year)	7426	6901	525	***	7426	6910	517	**
Per capita income (Rs/year)	5887	5930	-43		5887	5927	-40	
Asset (Rs)	348	452	-104		348	435	-87	
No. of observations	1670	3766			1670	3766		

Significance of difference is as follows: * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Logit regression of treatment using household and village explanatory variables

Variables	New participants		Converted participants		Non-participants	
Household characteristics						
Household lived in hamlet	0.78	***	1.18	***	0.21	
Scheduled tribe/caste	0.14		-0.03		0.03	
Other backward caste	0.50		0.50	*	0.23	
Household size	0.27	***	0.13	***	0.10	***
Household female headed	0.29		0.39		0.05	
Some member can write	0.12		0.55	**	0.35	**
Initial household conditions						
Female social capital index	-0.33	***	-0.33	***	-0.32	***
Female econ. empowerment index	-0.07		-0.06		-0.20	***
Female political participation	0.13		0.58	***	0.22	
House had mud floor	0.11		0.30	*	-0.16	
House had only unprotected water	0.89	***	0.89	***	0.88	***
House owned any land	0.68	***	0.84	***	0.55	***
Value of consumer durables p.c. (1000Rs)	0.16	*	0.18	**	0.08	**
Village Demographics						
Number of households (1000s)	-1.04	***	-1.50	***	-0.04	
Scheduled tribe/caste (%)	1.35	**	1.93	***	0.43	
Other backward caste (%)	2.99	***	3.14	***	2.90	***
Economically active females (%)	-0.07		-0.23	**	0.01	
Mean education by head (years)	14.75	***	12.57	***	16.53	***
Initial village characteristics						
Availability of public bus	-1.16	***	-1.34	***	-1.17	***
Availability of telephone	-0.49	**	0.07		-0.37	***
If child marriage existed	-1.09	***	-0.82	***	-0.93	***
If untouchability existed	-0.45	*	-0.56	***	0.23	*
If temple prostitution system existed	-2.06	**	-0.47		0.77	***
If caste panchayats existed	1.80	***	1.35	***	1.24	***
Pseudo R-squared						
Number of observations	1239		1651		2804	

Significance levels of coefficients based on robust standard errors indicated by stars as follows: * significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Impact of DPIP on new participants, converted participants and non-participants in the project area

Variables	Trimmed sample, PS weighted				Trimmed sample, Kernel matched			
	Treated	Control	Diff.		Treated	Control	Diff.	
Change in female social capital								
New participants	0.71	-0.53	1.24	***	0.71	-0.47	1.18	***
Converted participants	1.24	-0.25	1.49	***	1.24	-0.18	1.42	***
Non-participants	0.82	-0.33	1.16	***	0.82	-0.39	1.21	***
Change in female economic empowerment								
New participants	0.33	-0.33	0.66	***	0.33	-0.32	0.65	***
Converted participants	0.72	-0.21	0.93	***	0.72	-0.19	0.91	***
Non-participants	0.30	-0.20	0.50	***	0.30	-0.26	0.56	***
Change in female political participation								
New participants	0.09	0.00	0.09	***	0.09	0.00	0.09	***
Converted participants	0.09	-0.01	0.09	***	0.09	-0.01	0.09	***
Non-participants	0.05	0.01	0.04	***	0.05	0.01	0.04	***
Energy intake p.c. (kcal/day)								
New participants	2172	1987	185	**	2172	1979	193	***
Converted participants	2076	2069	7		2076	2076	0	
Non-participants	2070	2049	21		2070	2060	10	
Protein intake p.c. (g/day)								
New participants	51.57	44.19	7.38	***	51.57	44.06	7.51	***
Converted participants	49.64	46.02	3.62	**	49.64	46.17	3.48	**
Non-participants	48.47	46.29	2.19		48.47	46.55	1.92	
Consumption p.c. (Rs/year)								
New participants	7271	6198	1073	***	7271	6274	997	**
Converted participants	7269	6810	459		7269	6863	406	
Non-participants	7107	7468	-360		7107	7349	-241	
Income p.c. (Rs/year)								
New participants	5224	5737	-514		5224	5658	-434	
Converted participants	5809	6026	-217		5809	6049	-240	
Non-participants	6347	5953	393		6347	5948	399	
Asset p.c. (Rs)								
New participants	691	379	313		691	410	282	
Converted participants	426	715	-289		426	680	-254	
Non-participants	549	743	-194		549	783	-234	

Significance of difference is as follows: * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8: Test for heterogeneity of impact among new, converted and non-participants

Variables	ATET for New part's (1)		ATET for Conv. (2)		ATET for Non-part's (3)		Differences (1) - (2)		Differences (1) – (3)	
Female social capital (change)	1.28	***	1.50	***	1.15	***	-0.22		0.12	
Female econ. empowerment (change)	0.71	***	0.92	***	0.54	***	-0.21		0.17	
Female political participation (change)	0.09	***	0.09	***	0.04	***	0.00		0.06	**
Energy intake p.c. (kcal/day)	185	**	7		21		178	*	164	*
Protein intake p.c. (g/day)	7.38	***	3.62	**	2.19		3.76		5.19	**
Consumption p.c. (Rs/year)	1073	***	459		26		614		1048	**
Income p.c. (Rs/year)	-514		-217		393		-296		-907	
Asset (Rs)	313		-289		-194		602		507	
Number of observations	846		1416		1964					

Significance of difference is as follows: * significant at 10%; ** significant at 5%; *** significant at 1%

Appendix

To illustrate our approach, let *D*=1 if a household was treated and *D*=0 otherwise and denote corresponding outcomes by (Y_1, Y_0). Then the gain from participation is $(Y_1 - Y_0)$. While we are interested in the average effect of treatment on the treated (ATT) $E(Y_1 - Y_0 \mid D = 1)$, the inability to observe treated households in state 0 implies that we can estimate only $E(Y_1 \mid D = 1) - E(Y_0 \mid D = 0)$ which carries a bias of $B = E(Y_0 \mid D = 1) - E(Y_0 \mid D = 0)$. With X being a set of observables and A_1 and A_0 the support of X for $D = 1$ and $D = 0$ respectively, the bias can be decomposed into three components, B_1, B_2 and B_3 (Heckman *et al.* 1997)

$$B = \int_{A_1} E(Y_0 \mid X, D = 1) f(X \mid D = 1) dX - \int_{A_0} E(Y_0 \mid X, D = 0) f(X \mid D = 0) dX = B_1 + B_2 + B_3, \quad (1)$$

Letting A_{10} be the common support of *X* for *D*=1 and *D*=0, these elements can be written as

$$B_1 = \int_{A_1 \setminus A_{10}} E(Y_0 \mid X, D = 1) f(X \mid D = 1) dX - \int_{A_0 \setminus A_{10}} E(Y_0 \mid X, D = 0) f(X \mid D = 0) dX, \quad (2)$$

$$B_2 = \int_{A_{10}} E(Y_0 \mid X, D = 0)[f(X \mid D = 1) - f(X \mid D = 0)] dX, \quad (3)$$

$$B_3 = \int_{A_{10}} [E(Y_0 \mid X, D = 1) - E(Y_0 \mid X, D = 0)] f(X \mid D = 1) dX, \quad (4)$$

with B_1 due to non-overlapping support of X, B_2 due to selection on observables, i.e. different distribution of X in the two populations, and B_3 due to selection on unobservables. If B_3 can be eliminated, we can obtain a consistent estimate for ATT over the common support area of X as

$$M(A_{10}) = E_{A_{10}}(Y_1 - Y_0 \mid D = 1) = \frac{\int_{A_{10}} E(Y_1 - Y_0 \mid X, D = 1) dF(X \mid D = 1)}{\int_{A_{10}} dF(X \mid D = 1)} \quad (5)$$

through matching methods over the region of common support and reweighing non-treated observations to equate the distribution of X between the two samples.

To derive the conditions under which we can use propensity score matching on observables to estimate program

effects, partition the vector of observable household and village characteristics *X* into two possibly overlapping sub-vectors T and Z entering outcome and participation functions, respectively, so that $X = T \bigcup Z$. Letting U be a vector of unobservable attributes that affect participation and outcomes, we can write the outcome function as

$$\begin{aligned} Y_0 &= g_0(T,U), \quad D=0 \\ Y_1 &= g_1(T,U), \quad D=1 \end{aligned} \tag{12}$$

Let $S=1$ if a household is a new participant, $S=2$ if a household is a converted participant, and $S=0$ otherwise. Then the participation function for the different groups is simply

$$\begin{aligned} S=1 \text{ if } \quad & U \in R_1(Z), \\ S=2 \text{ if } \quad & U \in R_2(Z), \\ S=0 \text{ if } \quad & U \in R_0(Z) \equiv \overline{R}_1(Z) \cap \overline{R}_2(Z), \end{aligned} \tag{13}$$

where $R_1(Z)$ and $R_2(Z)$ are some spaces defined by Z. For any *S*, the two terms in square brackets in (4) above can be rewritten as

$$\begin{aligned} E(Y_0 \mid X, D=1, S=s) &= \int g_0(T,U) dF(U \mid X, D=1, S=s) \\ E(Y_0 \mid X, D=0, S=s) &= \int g_0(T,U) dF(U \mid X, D=0, S=s) \end{aligned} \tag{14}$$

Matching on observables will yield a consistent estimate for ATET over the common support of X under the condition:

$$F(U \mid X, D=1, S=s) = F(U \mid X, D=0, S=s). \tag{15}$$

That is, in DPIP and RPRP areas, the distribution of unobservables (U) conditional on observables (X) is the same for each of the three groups. Note that we do not need additional assumptions to justify the use of PS, because (12) and (15) together imply $(Y_0 \perp D \mid X, S=s)$ which implies $[Y_0 \perp D \mid P(X), S=s]$.

Appendix Tables and Figures

Table A.1: Balance check for village explanatory variables

Variables	Non-trimmed, simple diff.	Trimmed PS- weighted diff.	Trimmed Kernel matched diff.
Number of households (1000s)	-0.09 *	-0.05	-0.06
Scheduled tribe/caste (%)	-0.01	0.03	0.03
Other backward caste (%)	0.08 ***	-0.02	-0.02
Economically active females (%)	0.13 **	-0.01	0.01
Mean education by head (years)	0.23 ***	0.00	0.00
Availability of public bus	-0.20 ***	-0.04	-0.03
Availability of telephone	-0.14 ***	0.02	0.02
If child marriage existed	0.01	-0.01	-0.02
If untouchability existed	0.07 **	0.00	-0.01
If temple prostitution system existed	0.04 **	-0.01	-0.01
If caste panchayats existed	0.17 ***	0.03	0.03
Number of observations	659	568	568

Table A.2 Balance check for household and village explanatory variables

	New Participants			Converted Participants			Non-Participants		
Variables	**Simple**	**PS wgt**	**Kernel.**	**Simple**	**PS wgt**	**Kernel.**	**Simple**	**PS wgt**	**Kernel.**
Household lived in hamlet	0.06**	-0.01	-0.01	0.13***	0.01	0.02	0.00	0.00	-0.01
Scheduled tribe/caste	-0.02	0.05	0.05	-0.01	0.02	0.02	-0.09***	0.03	0.01
Backward caste	0.12***	-0.02	-0.01	0.07	-0.02	-0.02	0.17***	-0.03	-0.02
Household size	0.79***	0.08	0.11	0.47***	-0.02	-0.01	0.59***	0.02	0.05
Household female headed	-0.02	-0.02	-0.02	0.00	-0.01	-0.01	0.00	0.00	0.00
Somebody can write	0.06***	0.00	0.00	0.02	-0.02	-0.02	0.11***	0.00	0.01
Female social capital index	-1.64***	0.15	0.13	-1.80***	0.16	0.10	-1.75***	0.03	-0.12
Female econ. empowerment index	-0.06	-0.08	-0.11	-0.19	0.08	0.04	-0.43***	-0.06	-0.06
Female political participation	0.02	0.05	0.05	-0.01	-0.01	-0.01	-0.05***	0.00	-0.01
House with mud floor	0.04	0.00	0.00	0.09**	0.03	0.03	-0.10***	0.00	-0.01
House with unprotected water	0.20***	0.04	0.04	0.19***	0.08	0.08	0.13***	0.01	0.03
Household owned any land	0.29***	0.05	0.05	0.24***	0.04	0.04	0.19***	0.03	0.04
Consumer durables p.c. (Rs)	0.03	0.02	0.02	-0.05	-0.04	-0.03	0.10	-0.03	-0.02
Number of households	-0.14***	-0.03	-0.04	-0.14***	-0.01	-0.03	0.03	0.03	0.02
Economically active females (%)	0.04	0.04	0.04	0.06*	0.02	0.02	-0.10***	-0.01	-0.02
Scheduled tribe/caste (%)	0.07**	-0.02	-0.01	0.01	-0.01	-0.01	0.18***	0.00	0.01
Other backward caste (%)	0.04	-0.10	-0.13	-0.05	-0.13	-0.14	0.26***	-0.06	-0.05
Avg. school years by head	0.19***	0.00	0.00	0.16***	0.01	0.01	0.29***	0.00	0.00
Availability of public bus	-0.21***	0.01	0.00	-0.28***	-0.02	-0.01	-0.16***	0.03	0.01
Availability of telephone	-0.20***	0.00	-0.01	-0.15***	-0.03	-0.04	-0.07	0.04	0.02
Child marriage existed	-0.03	0.04	0.04	-0.02	0.03	0.04	-0.02	0.02	0.01
Untouchability existed	-0.04	0.01	0.01	-0.03	-0.01	-0.02	0.12**	0.00	0.00
temple prostitution system existed	-0.01	0.00	0.00	0.00	0.00	0.00	0.08***	0.00	0.00
Caste panchayats existed	0.29***	-0.04	-0.03	0.15***	0.01	0.01	0.17***	0.00	0.02
Number of observations	1239	275 + 571 = 846		1651	285 + 1131 = 1416		2804	563+1401=1964	

Note: "Simple" refers to simple unmatched difference while PS-weighted and kernel matched differences are based on the trimmed sample. Numbers of observations are given in the bottom row.

Table A.3: Estimates of DPIP impact on female empowerment indicators (PS-weighted least square trimmed sample)

Variables	Households in project area	New vs. converted	New vs.non-participants	Converted vs non-participants
Changes in female social capital				
Increased trust in the same caste group in the same village	0.22 ***	-0.05	0.02	0.06
Increased trust in different caste in the same village	0.19 ***	-0.02	-0.01	0.01
Increased trust in the same religious group in the same village	0.21 ***	-0.04	0.02	0.06
Increased trust different religious group in the same village	0.12 ***	0.06	0.01	-0.05
Increased trust in males in the same village	0.19 ***	-0.05	0.01	0.06
Increased trust in females in the same village	0.20 ***	0.00	0.05	0.05
Increased trust in the same caste group from other villages	0.22 ***	-0.03	-0.01	0.02
Increased trust in different caste in different villages	0.20 ***	-0.09 *	-0.02	0.07
Increased trust in the same religious group in different village	0.19 ***	0.03	0.04	0.01
Increased trust in different religious group in different village	0.12 ***	-0.04	-0.01	0.03
Increased trust in elected representatives	0.13 ***	-0.04	0.00	0.04
Increased trust in government employees	0.15 ***	-0.06	-0.03	0.02
Increased trust in police	0.07 ***	-0.04	0.00	0.03
Female economic empowerment				
Greater ability to save	0.16 ***	-0.03	-0.03	0.00
Greater ability to go alone for marketing	0.09 ***	-0.09 **	-0.02	0.07 *
Greater ability to go alone for visiting friends	0.09 ***	-0.03	-0.01	0.02
Greater ability to go alone for visiting relatives	0.08 ***	-0.01	0.02	0.03
Greater ability to go alone for visiting local health center	0.08 ***	0.00	0.03	0.03
Greater ability to go alone to fields outside the village for work	0.04 ***	-0.04	0.03	0.07 **
Greater ability to go alone to community center in the village	0.11 ***	-0.01	0.01	0.02
Greater ability to go alone to community functions	0.06 ***	0.00	0.02	0.02

***Note*:** the components of decrease trust in alternative groups and lower ability in terms of economic empowerment are omitted here because they are typically small (less than 0.01).

Figure A.1. Distribution of estimated propensity scores for village matching

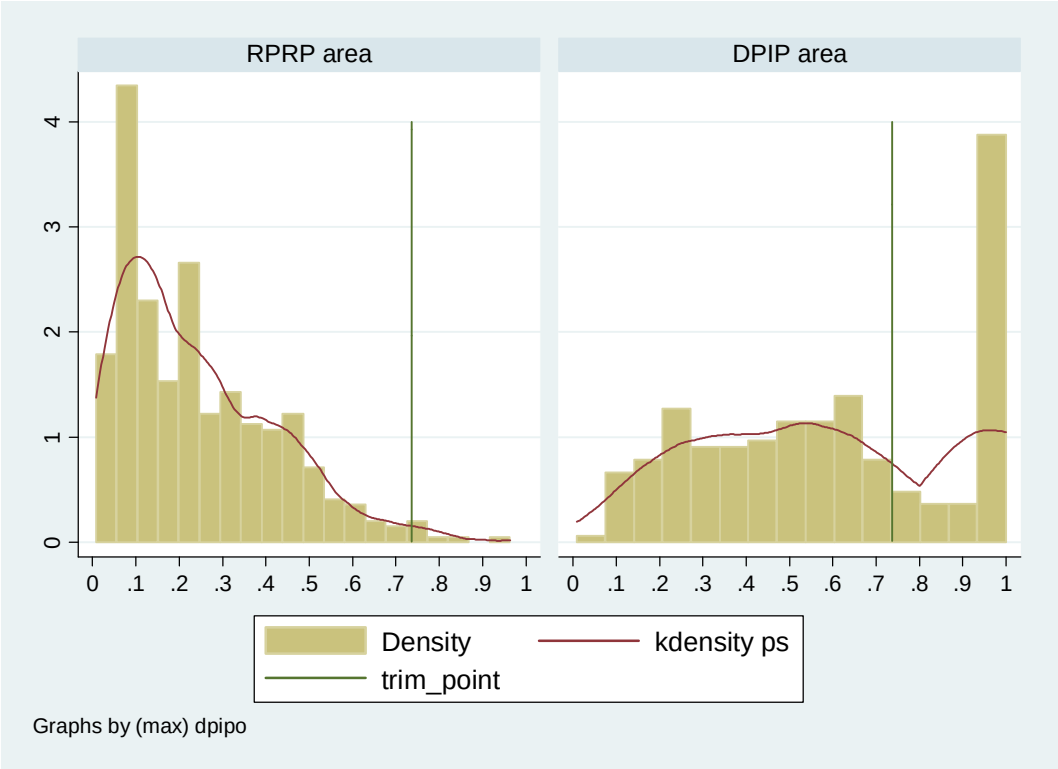
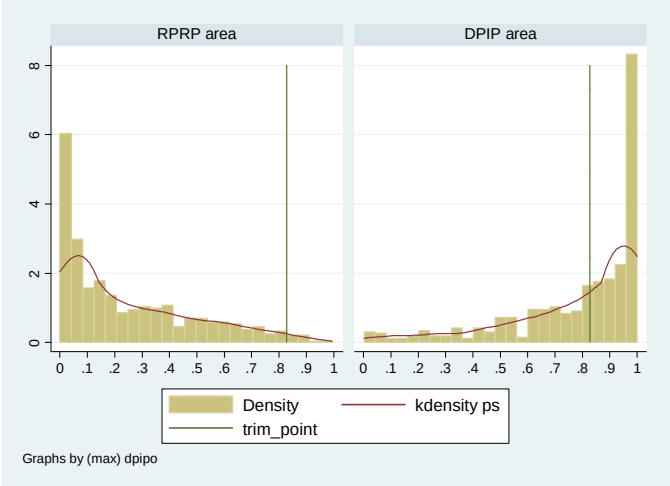
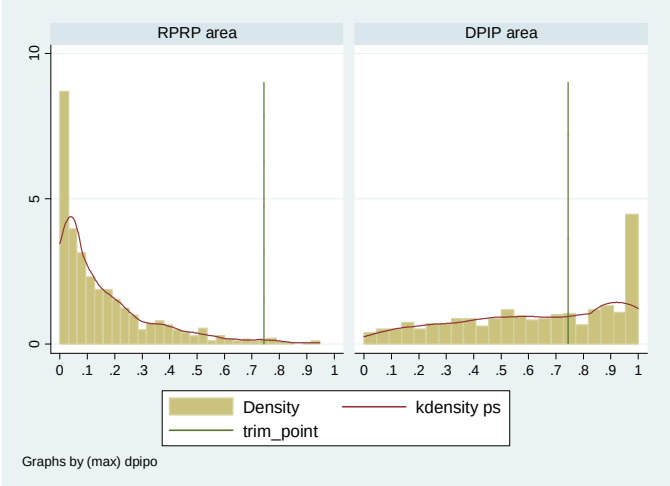


Figure A.2. Distribution of estimated propensity scores for new, converted, and non-participants

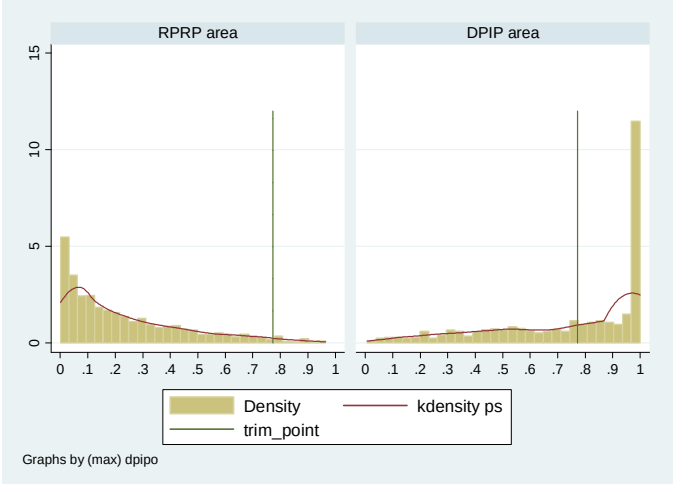
New participants



Converted participants



Non-participants



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