Economic and Social Impacts of Self-Help Groups in India

by

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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 combines a number of characteristics of interest. 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 established in the state during the 1990s. To expand coverage of the poor and to generate synergies between social and economic development, SHGs' traditional focus on micro-credit is complemented by an emphasis on empowering women socially and economically. Key instruments used to accomplish this are (i) targeted efforts to foster formation of SHGs by the "leftover poor"; (ii) capacity building for 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 interaction with the public and the private sectors in larger-scale programs; and (iii) a one-time injection of equity to the second tier institutions to provide the seed capital for them to take measures to remedy the multiple market and government failures encountered in rural India.

This paper aims to evaluate the impact of this 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 and control districts, i.e. districts where the program became available in 2001 and 2004, respectively. Our data provide a rich set of information on female empowerment, nutritional status, consumption, income, and asset endowments 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. At the village level, we use propensity score matching to control for selection on observables. Results point towards significant positive average impacts from the program on female empowerment, nutritional outcomes, and consumption, but fail to reject the hypothesis that the program had no impact on income and asset accumulation.

To allow for (indirect) effects on households in program areas who did not join the program and to prevent crediting the program with effects that are attributable to the formation of SHGs prior to its coming into force, we allow for heterogeneity of program impact across 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). This is possible as we have information on households' participation status three years after the program became available not only for the treatment but also for the control districts. Propensity score matching is then used to balance treatment and control on observables and derive estimates of impact that can be compared to those obtained at the village-level matching as a rough robustness check.

In addition to being supportive of the positive empowerment and nutritional effects that emerged at village-level matching, the evidence obtained in this way provides a number of additional insights. 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, while non-participants benefit only through increased social empowerment, 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 11 and 19 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 (14 percentage points) and converted (12 percentage points) groups though this is not matched by a commensurate increase in income or assets.

The paper is structured as follows. Section two describes key features of the program, introduces data and descriptive statistics, and highlights methodological issues. Section three describes matching and weighting methods and uses these to obtain estimates of average treatment effects. Section four provides a detailed discussion of the assumptions underlying the estimates of heterogeneous program impact for different sub-groups, 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, data sources, and identification strategy

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 and describes key characteristics of the program. These include the federation of established self-help groups, the focus on social action and outreach, and the infusion of financial resources to build up a corpus of resources at the group level. We discuss data sources and use our data to illustrate the success in terms of geographic targeting and outcome indicators. This provides the background for highlighting the methodological issues to be addressed in subsequent sections.

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 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 elected local governments 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 Main program characteristics

We aim to evaluate the first phase of the IKP (*Indhira Kranthi Patham*) program in Andhra Pradesh, also known as DPIP.[2] The program -supported by a US$ 110 million World Bank loan- started in October 2000 to promote and strengthen self-help groups in the state's six poorest districts.[3] 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. The IKP program aims to build on AP's tradition of SHGs to promote economic and social empowerment in a mutually reinforcing way. This is to be accomplished through three

components, namely (i) inducing formation of new groups among poor individuals who had not yet participated; (ii) strengthening of all SHGs by setting up and supporting a federated SHG structure at village and mandal (county) levels via village organizations (VOs) and Mandal Samakhyas (MSs);[4] and (iii) supporting the accumulation of capacity and equity among participating groups.

Establishing VOs and MSs aims to transform SHG networks into economically sustainable institutions. In addition to other committees according to need, VOs have an executive committee with two delegates from each member SHG and, in turn, send representatives to the MS' executive committee.[5] Economically, MSs and VOs are expected to act as financial intermediaries who on-lend funds from the banking sector and employ dedicated staff on a part- or full-time basis. The spreads earned in this way provide a source of revenue may be used for economic activities, e.g. aiming to add value to local agricultural production through marketing or processing. Having an economic basis would also enable groups to take on social tasks, in particular to embark on specific drives to organize "left over poor", revive defunct or dormant SHGs, take over implementation of government programs such as distribution of pensions or subsidized foodgrain, initiate campaigns to eliminate undesirable social and caste-based practices, and run training centers for their members and other locals.

On the social side, a key objective of the project is 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 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. Many VOs took over distribution of subsidized foodgrain under the public distribution scheme from private operators who had failed to make it available to intended beneficiaries.[6] In addition to improving access to food and risk coping and offering a way to attract potential new members, the fact that such grain was provided as an in-kind credit helped establish a discipline of periodic meeting attendance, saving, and (re)payment. 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 economic opportunities for their advancement identified by mostly female community activists specifically hired for this purpose.

Physical outcomes achieved considerably exceeded initial expectations. Compared to a target of covering 620,000 beneficiary households in 180 mandals, by December 2006 the program had organized 2.29 million households from 316 mandals in 171,618 SHGs, 9,872 VOs, and 316 MSs (World Bank 2007).With an average of US$ 700 per SHG, it had helped to generate savings of US$ 113 million. Some 90% of SHGs had been linked to banks, obtaining a total of US$ 425 million in loans. Every MS established a training center to educate local youth and SHG members on economic, social, and health issues and the federated structure was used as a conduit to deliver government programs; by 2006, 42% and 32% of VOs, respectively, had taken over distribution of pensions and subsidized rice. Moreover, 273 VOs and 42 MSs had started to operate centers for milk cooling and cereal procurement to link women, many of whom had used loans to acquire livestock, to markets and expose them to the modern economy.

2.3 Data sources and descriptive statistics

To assess whether and how much this tremendous outreach has affected household welfare, we

use a survey conducted simultaneously in the DPIP and RPRP areas from February to June of 2004. Districts were chosen to represent all the state's macro-regions, randomly selecting villages and then households in these.[7] The household questionnaire, which was complemented by questionnaires at group (SHG), village (VO), and mandal (MS) 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.[8] 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. At the village level, this was complemented by census information from the PIP exercise on education and access to assets. To interpret findings and descriptive statistics, note that the survey followed immediately on a major drought.[9]

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.[10] 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 and to ensure that our estimates are not subject to reverse causality whereby higher levels

of social capital cause program participation rather than the opposite.

Household consumption includes food and non-food consumption over the past 30 days and more lumpy items over the past year.[11] 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).[12] 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.[13]

Village demographics and initial conditions in terms of undesirable social practices based on PIP data and the village questionnaire 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 as 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 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.[14] 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.

2.4 Methodological issues

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 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 3 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.[15] Propensity score matching is used to deal with differences in observables and 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.

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. Of course, data limitations may allow estimation only over a properly defined area of common support as discussed below.

3. Estimating average effects at village level

Applying the methodological framework discussed above to estimate mean program effects at the village level for a range of social, nutritional, and economic indicators suggests that the program did contribute to greater empowerment, improved nutritional diversity and higher levels of consumption, but not improved income. While these estimates do not suffer from bias due to households' self-selection into the program, complementing them with household-level estimates will be useful to identify possible heterogeneity of program effects.

3.1 Matching and trimming methodologies

A combination of propensity score matching and double differences for selected variables is used to control for non-random placement and to assess average program impacts on households in program villages. This provides an estimate of the effect of having the program available irrespectively of households' participation status. As failure to control for differences in observables can lead to large biases (Ravallion 2007), we use propensity score (PS) matching. The underlying assumption is that, conditional on observables, 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 $P(X) = \Pr(D = 1 \mid X)$ which, by definition, takes a value between 0 and 1.

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

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

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.

To obtain consistent and efficient estimates, we 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 λ is a solution to

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

otherwise. It has been shown that under homoskedasticity this trimming method minimizes the variance of the estimated ATT (Crump *et al.* 2007).

3.2 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 3 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 4 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.[16] 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 8 and 25 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 9% 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 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 project. 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 600 or 9% 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. Heterogeneous impact on sub-groups

While the average program effects described above are instructive, properly measures 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. As self-selection by households is an important issue, we first explain the way in which we construct groups of households who are likely to be - similar in unobservables. This is followed by a discussion of the results for new and converted participants and households who did not participate in the program, respectively.

4.1 Methodological issues

To make unbiased inferences on the heterogeneous impact for sub-groups, we need to control for non-random program placement as well as 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 2001 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.

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 ATT over the common support of X under two conditions. First, the outcome function in (12) and participation function in (13)

need to be the same in the DPIP and RPRP areas, i.e. neither the economic environment facing populations in both areas nor the nature of the program differ between the two locations. 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 even if, as seems the case, RPRP spread more rapidly than had been the case for DPIP, something that would impart a downward bias on our results. 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.

The second condition that needs to hold is that, in DPIP and RPRP areas, the distribution of unobservables (U) conditional on observables (X) is the same for each of the three groups, i.e.,

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

While this condition can not be tested, it is much weaker than what is normally assumed in the literature on propensity score matching. In addition, we can perform an independent check on the results using the notion that the sum of all effects estimated here needs to add up to our earlier average treatment estimates. Note that we do not need additional assumptions beyond the above two conditions to justify the use of PS, because (12) and (15) together imply $(Y_0 \perp D \mid X, S = s)$ which implies $[Y_0 \perp D \mid P(X), S = s]$.

4.2 Estimated treatment effects

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 (i.e. belong to T and Z). Initial economic conditions at household (female social capital and economic empowerment, housing quality, land ownership, and value of durable assets) and village level (availability of public transport and telephone) are assumed to affect outcomes but not participation (i.e. T but not Z).[17] Villages are used as clusters throughout to control for village-level random effects.[18]

Table 5 reports results from probit regressions of the three types of treatment (new, converted, and non-participants) using T and Z as explanatory variables in DPIP and RPRP areas, respectively.[19] 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 estimated propensity scores for the three sub-groups, reinforcing the need for trimming and reweighting especially in the case of new participants.[20]

Estimates of average treatment effects based on the PS-weighted and kernel matched differences for trimmed samples are reported in table 6. It suggests high levels of consistency between results by the two methodologies and, despite the difference in approach, 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 ATTs as reported in table 7.

A first finding of interest is that increases in female social capital, economic empowerment

and political participation are significantly higher for the treated as compared to the untreated in all the three groups, consistent with the notion to have social mobilization provide 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 highest for converted participants and lowest for non-participants although the difference is never significant for social capital and only at 10% between converted and non-participants for economic empowerment. 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 three 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 206 kcal and 8.1 grams of protein per day, our estimate of the ATT for new participants points towards a program-induced increase of about 11% and 19%, respectively. By comparison, we find no impact on calorie intake and a smaller effect of about 10% on protein intake for members of converted groups. This is consistent with the notion that households in this group already have had access to small loans (e.g. through internal lending) to smooth consumption before the program but that the program, possibly by increasing their participation in market-related activities or by providing them with capital or livestock, allowed them to

diversify their diet 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 new participants', pointing towards a pro-poor program impact that helped to improve the nutrition of (poor) participants lagging behind initially. Table 7 implies that the difference between new and non-participants in both 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 and converted participants but not for those who did not join SHGs. With between R 926 and R 892, depending on the method, for new and R 825 and R 820 for converted participants, estimated ATTs are close and imply an increase of 14 and 12 percentage points as compared to counterfactual incomes of R 6,431 and R 6,768, respectively although table 7 suggests that the difference is not significant statistically.[21] The estimated ATT for non-participants is not different from zero, suggesting that there are no spillovers from the program.

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.[22]

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.[23] 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.[24] 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, additional data at 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 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, would be of great interest to not only the impact of SHGs but also to make inferences on their longer-term trajectory and economic sustainability all of which will hopefully be explored in 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
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: Probit regression of treatment at village level

Variables	Coeff.	(s.e.)	Sig.
Village Demographics			
Number of households (1000s)	-0.14	(0.14)	
Scheduled tribe/caste (%)	8.17	(0.38)	***
Other backward caste (%)	0.72	(0.37)	*
Economically active females (%)	1.40	(0.08)	***
Mean education by head (years)	-0.08	(1.52)	
Initial village characteristics			
Availability of public bus	-0.74	(0.13)	***
Availability of telephone	-0.26	(0.13)	**
If child marriage existed	-0.30	(0.14)	**
If untouchability existed	0.15	(0.15)	
If temple prostitution system existed	-0.16	(0.30)	
If caste panchayats existed	0.70	(0.13)	***
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 4: 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	1.42	-0.65	2.07	***	1.42	-0.67	2.09	***
Female econ. empowerment	0.59	-0.34	0.94	***	0.59	-0.36	0.96	***
Female political participation	0.08	0.03	0.05	***	0.08	0.03	0.05	***
Energy intake p.c. (kcal/day)	2089	2055	34		2089	2058	31	
Protein intake p.c. (g/day)	49.84	45.88	3.96	***	49.84	45.99	3.85	***
Consumption p.c (Rs/year)	7482	6886	596	***	7482	6888	594	***
Per capita income (Rs/year)	5873	5947	-74		5873	5949	-76	
Asset (Rs)	1815	1737	77		1815	1659	155	
No. of observations	1664	3755			1664	3755		

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

Table 5: Probit regression of treatment using household and village explanatory variables

Variables	New participants		Converted participants		Non-participants	
Household characteristics						
Household lived in hamlet	0.44	***	0.65	***	0.12	
Scheduled tribe/caste	0.01		-0.02		0.02	
Other backward caste	0.21		0.25		0.13	
Household size	0.16	***	0.07	***	0.06	***
Household female headed	0.20		0.19		0.05	
Some member can write	0.07		0.33	***	0.21	***
Initial household conditions						
Female social capital index	-0.18	***	-0.18	***	-0.18	***
Female econ. empowerment index	-0.04		-0.05	**	-0.12	***
House had mud floor	0.08		0.16	*	-0.08	
House had only unprotected water	0.51	***	0.48	***	0.51	***
House owned any land	0.40	***	0.44	***	0.32	***
Value of consumer durables p.c. (1000Rs)	0.10	*	0.11	**	0.05	**
Village Demographics						
Number of households (1000s)	-0.52	***	-0.73	***	0.01	
Scheduled tribe/caste (%)	8.12	***	6.93	***	9.34	***
Other backward caste (%)	0.70	**	0.95	***	0.22	
Economically active females (%)	1.63	***	1.63	***	1.67	***
Mean education by head (years)	-0.02		-0.11	*	-0.01	
Initial village characteristics						
Availability of public bus	-0.65	***	-0.77	***	-0.69	***
Availability of telephone	-0.29	**	0.01		-0.22	***
If child marriage existed	-0.56	***	-0.42	***	-0.57	***
If untouchability existed	-0.29	**	-0.31	***	0.16	**
If temple prostitution system existed	-1.26	***	-0.21		0.44	***
If caste panchayats existed	0.98	***	0.69	***	0.69	***
Pseudo R-squared	0.49		0.42		0.47	
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 6: 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	0.85	-0.35	1.19	***	0.85	-0.32	1.17	***
Converted participants	1.36	-0.27	1.63	***	1.36	-0.24	1.60	***
Non-participants	0.88	-0.37	1.25	***	0.88	-0.40	1.28	***
Change in female economic empowerment								
New participants	0.46	-0.19	0.65	***	0.46	-0.18	0.64	***
Converted participants	0.77	-0.16	0.93	***	0.77	-0.18	0.95	***
Non-participants	0.27	-0.24	0.51	***	0.27	-0.25	0.52	***
Change in female political participation								
New participants	0.09	0.02	0.07	***	0.09	0.02	0.07	***
Converted participants	0.11	0.02	0.09	***	0.11	0.02	0.09	***
Non-participants	0.05	0.03	0.02	*	0.05	0.03	0.02	*
Energy intake p.c. (kcal/day)								
New participants	2159	1953	206	***	2159	1951	208	***
Converted participants	2101	2056	45		2101	2056	45	
Non-participants	2049	2054	-4		2049	2059	-10	
Protein intake p.c. (g/day)								
New participants	51.62	43.53	8.09	***	51.62	43.49	8.13	***
Converted participants	50.61	46.05	4.56	**	50.61	46.00	4.61	**
Non-participants	48.24	46.35	1.89		48.24	46.50	1.74	
Consumption p.c. (Rs/year)								
New participants	7357	6431	926	*	7357	6464	892	*
Converted participants	7592	6768	825	**	7592	6772	820	**
Non-participants	7124	7303	-179		7124	7276	-152	
Income p.c. (Rs/year)								
New participants	5251	5734	-484		5251	5761	-511	
Converted participants	5923	6342	-420		5923	6303	-381	
Non-participants	6270	6052	218		6270	6031	239	
Asset (Rs)								
New participants	5203	1673	3531	*	5203	1738	3465	
Converted participants	2517	2574	-57		2517	2851	-334	
Non-participants	2593	3302	-710		2593	3311	-718	

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

Table 7: Test for heterogeneity of impact among new, converted and non-participants

Variables	ATT for			Differences		
	New part's (1)	Conv. (2)	Non-part's (3)	(1) - (2)	(1) – (3)	(2) – (3)
Female social capital (change)	1.19 ***	1.63 ***	1.25 ***	-0.44	-0.06	0.38
Female econ. empowerment (change)	0.65 ***	0.93 ***	0.51 ***	-0.28	0.14	0.42 *
Female political participation (change)	0.07 ***	0.09 ***	0.02 *	-0.02	0.05 **	0.07 **
Energy intake p.c. (kcal/day)	206 ***	45	-4	161 *	211 ***	49
Protein intake p.c. (g/day)	8.09 ***	4.56 **	1.89	3.53	6.20 ***	2.67
Consumption p.c. (Rs/year)	926 *	825 **	-179	101	1105 *	1004 **
Income p.c. (Rs/year)	-484	-420	218	-64	-702	-638
Asset (Rs)	3531 *	-57	-710	3588	4241 *	653
Number of observations	846	1416	2804			

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

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 (%)	0.23	***	0.00	0.00
Other backward caste (%)	-0.01		0.03	0.03
Economically active females (%)	0.08	***	-0.02	-0.02
Mean education by head (years)	0.13	**	-0.01	0.01
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	2516+3824=6340		1664+3755=5419	1664+3755=5419

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	-0.02	0.13***	0.01	0.00	0.00	0.01	0.01
Scheduled tribe/caste	-0.02	0.04	0.05	-0.01	-0.02	-0.01	-0.09***	0.02	0.01
Backward caste	0.12***	-0.01	-0.01	0.07	0.01	0.01	0.17***	-0.01	0.00
Household size	0.79***	0.15	0.18	0.47***	0.04	0.05	0.60***	-0.03	-0.01
Household female headed	-0.02	-0.02	-0.02	0.00	0.01	0.01	0.01	0.00	0.00
Somebody can write	0.07***	-0.03	-0.03	0.02	0.01	0.01	0.11***	0.00	0.01
Female social capital index	-1.63***	0.09	0.08	-1.80***	0.16	0.09	-1.74***	-0.04	-0.14
Female econ. empowerment index	-0.05	0.08	0.04	-0.19	0.17	0.16	-0.43***	-0.02	-0.01
House with mud floor	0.04	0.03	0.02	0.09**	0.01	0.01	-0.10***	0.01	0.01
House with unprotected water	0.20***	0.01	0.01	0.19***	0.01	0.01	0.13***	0.01	0.02
Household owned any land	0.29***	0.02	0.02	0.24***	0.02	0.04	0.19***	0.04	0.04
Consumer durables p.c. (Rs)	0.03	-0.02	-0.03	-0.05	-0.01	0.00	0.10*	0.00	0.01
Number of households	-0.14***	-0.04	-0.04	-0.14***	-0.02	-0.03	0.03	-0.01	-0.01
Economically active females (%)	0.19***	0.00	0.00	0.16***	0.00	0.00	0.28***	0.00	0.00
Scheduled tribe/caste (%)	0.04	0.05	0.05	0.06*	0.01	0.02	-0.10***	0.01	0.00
Other backward caste (%)	0.07**	-0.03	-0.03	0.01	-0.01	-0.01	0.18***	-0.01	0.00
Avg. school years by head	0.04	-0.01	-0.02	-0.05	-0.04	-0.04	0.26***	-0.03	-0.02
Availability of public bus	-0.21***	0.01	0.01	-0.28***	0.02	0.02	-0.15***	0.00	-0.01
Availability of telephone	-0.20***	0.02	0.00	-0.15***	-0.01	-0.02	-0.06	0.02	0.01
Child marriage existed	-0.03	-0.04	-0.04	-0.02	-0.04	-0.03	-0.02	0.02	0.02
Untouchability existed	-0.04	0.01	0.01	-0.03	0.01	0.01	0.12***	-0.01	0.00
temple prostitution system existed	-0.01	0.00	0.00	0.00	0.00	0.00	0.07***	0.00	0.00
Caste panchayats existed	0.29***	-0.01	-0.01	0.15***	0.05	0.06	0.17***	0.03	0.03
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	0.25	***	-0.07		0.00		0.07	
Increased trust in different caste in the same village	0.20	***	-0.03		-0.03		0.00	
Increased trust in the same religious group in the same village	0.24	***	-0.04		0.02		0.06	
Increased trust different religious group in the same village	0.13	***	0.05		0.00		-0.05	
Increased trust in males in the same village	0.21	***	-0.06		-0.01		0.05	
Increased trust in females in the same village	0.22	***	0.00		0.07	*	0.07	
Increased trust in the same caste group from other villages	0.25	***	-0.05		-0.03		0.02	
Increased trust in different caste in different villages	0.22	***	-0.09	*	-0.03		0.07	
Increased trust in the same religious group in different village	0.21	***	-0.02		0.01		0.03	
Increased trust in different religious group in different village	0.14	***	-0.06		-0.02		0.04	
Increased trust in elected representatives	0.14	***	-0.04		0.01		0.05	
Increased trust in government employees	0.16	***	-0.08	*	-0.05		0.04	
Increased trust in police	0.08	***	-0.03		-0.01		0.02	
Female economic empowerment								
Greater ability to save	0.16	***	-0.03		0.14		0.42	*
Greater ability to go alone for marketing	0.10	***	-0.09	**	-0.02		0.01	
Greater ability to go alone for visiting friends	0.10	***	-0.05		-0.01		0.07	**
Greater ability to go alone for visiting relatives	0.08	***	-0.02		-0.02		0.03	
Greater ability to go alone for visiting local health center	0.08	***	-0.02		0.02		0.04	
Greater ability to go alone to fields outside the village for work	0.05	***	-0.04		0.03		0.04	
Greater ability to go alone to community center in the village	0.11	***	0.01		0.03		0.07	***
Greater ability to go alone to community functions	0.06	***	0.00		0.03		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

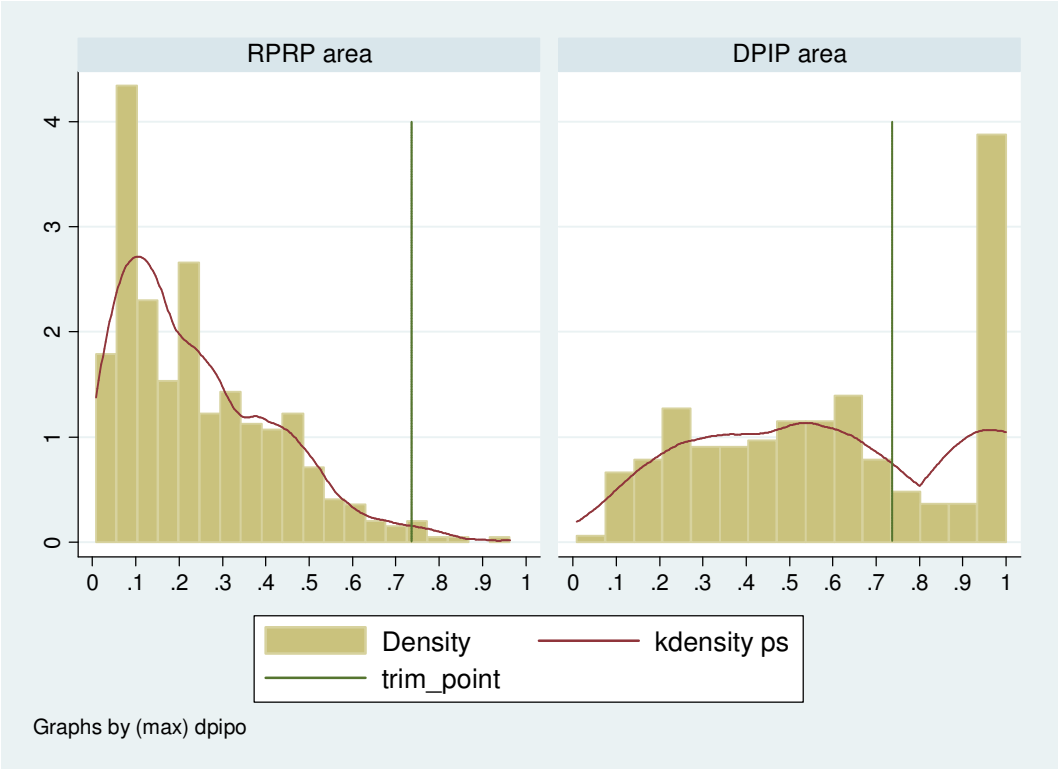
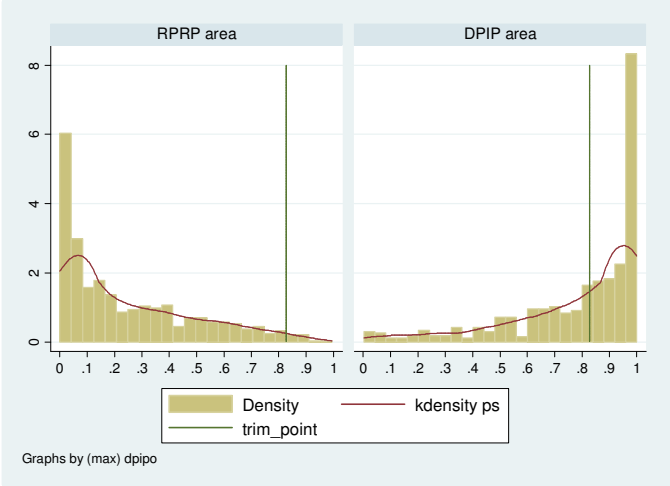
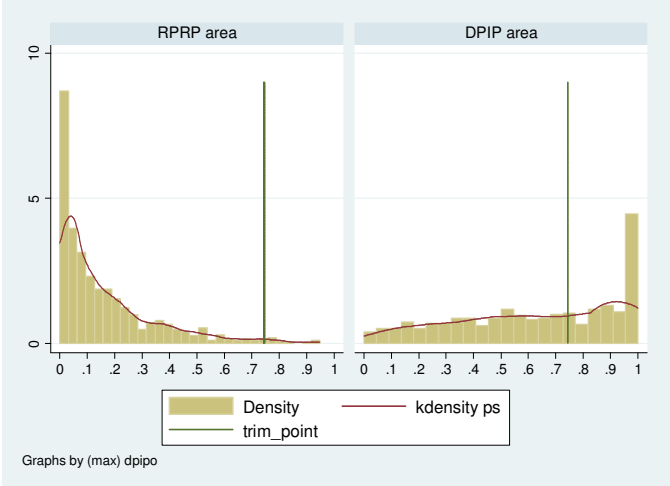


Figure A.2. Distribution of estimated propensity scores for new, converted, and non-participants

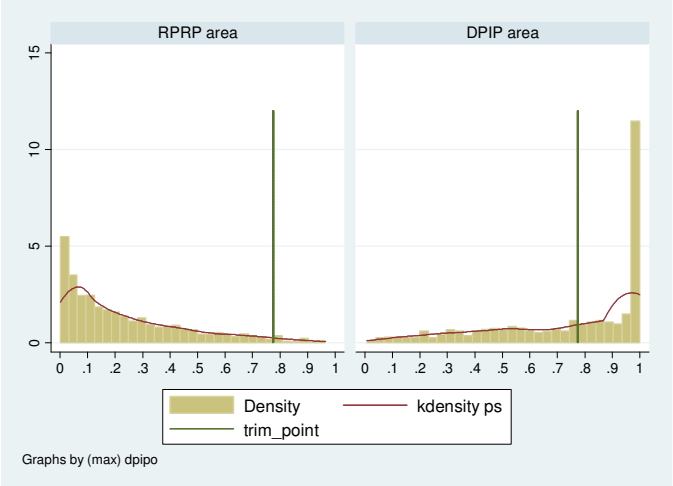
New participants



Converted participants



Non-participants



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Endnotes

[1] We thank CESS staff, in particular Prof. S. Galab, Prof. M. Dev, and Dr. P. Reddy, for their support and collaboration in making the data available and R. Mu, S. Jin, E. Galasso, and V. Rao for insightful comments and suggestions. Funding from the Norwegian ESSD Trust Fund (poverty window) and the Gender Action Plan is gratefully acknowledged. The views expressed in this paper are those of the authors and do not necessarily represent those of the World Bank, its Board of Executive Directors, or the countries they represent.

[2] 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.

[3] 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.

[4] VOs represent all the SHGs in one village, up to a total of about 15-20. In villages with more SHGs, two VOs are formed. VOs in turn become members of the MS at the *mandal* (equivalent to county) level. Recently federations have expanded to encompass the district (*zilla*) level.

[5] In 2006, about 50% of VOs in the sample had other committees with the most frequent ones relating to social action, loan recovery and bank linkages, and social audit and asset verification.

[6] 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.

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

[8] 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.

[9] 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.

[10] 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).

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

[12] 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.

[13] 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.

[14] The fact that income was lower than consumption may be due to drought conditions prevailing at the time of the survey that forced households to borrow or use savings to smooth consumption. Alternatively, it could be due to under-reporting of some income component.

[15] 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.

[16] 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.

[17] 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.

[18] We owe this point to Emanuela Galasso.

[19] 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.

[20] 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.

[21] Note that the slightly lower significance level (of 10% as compared to 5%) for new participants could be due to the smaller sample size.

[22] 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.

[23] 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.

[24] In our case, the income aggregate has to be constructed using 116 variables rather than only 84 as in the case of consumption.