

# Evaluating Program Impacts on Mature Self-help Groups in India

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Despite the popularity and the unique nature of women's self-help groups in India, evidence on the economic impact of these groups is scant. On the basis of two rounds of surveys of 2,517 households, we use a strategy of double differences and propensity score matching to assess the economic effects of a program that promoted and strengthened self-help groups in Andhra Pradesh in India. Our analysis finds that longer exposure to the program has a positive impact on consumption, nutritional intake, and asset accumulation. Our investigation into the heterogeneity of these effects suggests that even the poorest households are able to benefit from the program. JEL codes: I38, O12.

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India has long made efforts to expand credit availability to rural areas. Early programs, which often yielded less than satisfactory results, were gradually replaced by efforts focusing on the most disadvantaged women and organizing these women into self-help groups (SHGs) as a way to empower them socially and economically while facilitating the eventual establishment of access to bank credit. The use of SHGs for this purpose has recently undergone tremendous growth, and SHGs have emerged as one of the world's largest microfinance networks. In 2007, some 40 million households were organized into more than 2.8 million SHGs, which borrowed more than US\$ 1 billion of credit from banks in 2006–2007 (Reserve Bank of India 2008). India's cumulative credit to SHGs is estimated at US\$ 4.5 billion, representing approximately 10 percent of all rural credit (Garikipati 2008).

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With features such as “saver graduation” and an inherent tendency toward membership expansion (Ahlin and Jiang 2008), India’s SHGs have many desirable elements. Even within India, however, their outreach is concentrated in few states; 70 to 80 percent of SHGs are in the four southern states of Andhra Pradesh (AP), Karnataka, Tamil Nadu, and Kerala. Consequently, there is immense potential for expansion. Exploring the impact of the SHG model for microcredit on key household-level outcomes is useful to determine whether significant differences between SHG members and nonmembers (Dev, Kanbur, Galab and Alivelu 2012) can be interpreted as program effects. The results of this examination also have clear implications for the planned expansion of the SHG model under India’s National Rural Livelihoods Mission (Government of India 2011).

To provide empirical evidence, this paper evaluates an SHG-based microcredit intervention in Andhra Pradesh (AP) that includes efforts to (i) foster the formation of SHGs among the poor who had previously been excluded; (ii) establish second-tier institutions at higher administrative levels, both to address widespread market imperfections in India and to diversify credit risk, build capacity, and leverage local government efforts;<sup>1</sup> and (iii) provide a one-time injection of equity for the seed capital needed to jump start SHGs formed by the poor. Our primary evidence comes from a two-round survey of 2,517 households in early 2004 and late 2006 that covers “treatment” mandals, where the program began in early 2001, and a set of randomly chosen control mandals, where the program became available in late 2003.<sup>2</sup> To derive credible estimates of impact, we must address the challenges posed by the rapid expansion of the control mandals well before the time envisaged,<sup>3</sup> the endogeneity of households’ decisions to participate, the lack of a proper baseline, and a small sample size of randomization units (mandals). We address participants’ self-selection into the program by comparing outcomes between program SHG participants in treated and control areas. To eliminate any remaining systematic differences between the treatment and the control groups due to a flawed randomization design, we use double differences and propensity score matching to eliminate any time-invariant bias while addressing bias due to time-varying observables. The unavailability of a preprogram baseline and the contamination of the control group imply that what we estimate is only the impact of

1. The SHG federation is also featured in earlier programs promoted by NGOs, such as PRADAN (Baland, Somanatha, and Vandewalle 2008).

2. Mandals are administrative units above the village and below the district and are equivalent to counties in the US. In most other states, this unit is referred to as a block.

3. In Andhra Pradesh, a first phase targeted only six districts, but a follow-up program to expand coverage to the entire state was implemented less than three years after the launch of this intervention. This period may be too short to expect large economic impacts because the target group includes the poorest households, who require considerable training and capacity building before they are in a position to successfully use and repay loans.

exposure to an additional 2.5 years of the program on (mature) groups that are three years old.

Our results point to economic gains from the program through better nutrition, higher levels of consumption, and asset accumulation by the program's SHG participants. Differentiating by participants' poverty status suggests that the effects are most pronounced for poor participants, who were able to increase their levels of consumption, nutritional intake, and asset accumulation as a result of the program. Most of these effects accrue after more than one year in the program. Furthermore, there is some evidence that program SHGs perform better than nonprogram SHGs with respect to nutritional gains, presumably because of their ability to draw on a federated network to provide access to food grains in kind. Robustness checks and evidence from the literature suggest that this is a lower bound of total program effects. Tests for the heterogeneity of the effect by initial poverty status for SHG members, length of exposure, and the type of SHG (program versus nonprogram SHGs)<sup>4</sup> support our main results. Additionally, our results allow us to obtain a bound for the program's overall cost-benefit ratio.

The paper is structured as follows. Section 2 describes key program features, highlights the challenges faced in the evaluation, and presents the identification strategy for both the intention to treat estimate and the average treatment effect (ATE). Section 3 presents the data characteristics and sample design as well as descriptive evidence at the group and household levels. Section 4 presents ATEs for consumption, calorie, and protein intake; asset accumulation for households in program mandals; and the ATE on members of program SHGs, together with robustness checks and efforts to discern effects on different subgroups. Section 5 concludes, drawing implications for policy and future research.

## I. PROGRAM DESCRIPTION AND IDENTIFICATION STRATEGY

An innovative feature of this program is that it incorporates broad initiatives to cater to the specific needs of marginal groups in an effort to reach the poorer segments of the population, who may have been beyond the reach of pure microfinance initiatives because of a lack of opportunities. The goal is to empower the target groups socially and economically, thereby allowing them to become subjects of credit. Qualitative and descriptive accounts document the success of this approach in covering large shares of the rural population, but evidence on the extent to which target groups are reached by the program and the magnitude of the benefits they derive from it, especially in comparison

4. Nonprogram SHGs refer to SHGs formed by efforts prior to the program under study. We provide more information on earlier efforts to form SHGs in section 2.

with the program cost, remains scant. This section describes the program design, notes the challenges that it poses, discusses other evidence, and presents our identification strategy.

### *Program Design*

The formation of women's SHGs as a means to foster female empowerment, raise awareness, and facilitate access to independent savings and financial resources was introduced in the 1980s as part of a pilot scheme for the Development of Women and Children in Rural Areas to improve the gender component of India's Integrated Rural Development Project. A typical SHG comprises 10 to 20 women who meet regularly to collect members' savings (which are deposited in a joint bank account), discuss social issues, attempt to identify critical issues or skill gaps, and improve members' skills through specific training. Once savings have been accumulated, members can apply for internal loans, drawing on accumulated savings at an interest rate set by the group. Once the group has established a record of saving and repayment, it can gain access to commercial bank loans, generally in fixed proportion (commonly four to one) to its equity capital.

Efforts to promote SHGs in the state of Andhra Pradesh (AP) have been among the most proactive and successful in advancing this concept. To build on this success, the state government, with support from a US\$ 111 million World Bank loan, implemented the District Poverty Initiatives Project (DPIP) in the state's six poorest districts (Chittoor, Srikakulam, Adilabad, Vizianagaram, Mahabubnagar, and Anantapur). The goal was to expand coverage through the formation of new SHGs and to enhance the capacity of existing SHGs. To accomplish this goal, a three-pronged strategy was adopted.

First, efforts were undertaken to induce the formation of new SHGs among poor segments of the population who, in light of their limited attractiveness as subjects of credit, had failed to join earlier efforts. The tool to identify the target group was a statewide "participatory identification of the poor" that added vulnerability and social exclusion to quantitative indicators from the 2001 national census. Its main output was a set of lists, duly ratified by village assemblies, to determine the poverty status of all households in a village.<sup>5</sup> The program supported community organizers who explained the benefits of program participation to the poor and identified strategies that would make participation attractive to them.

Second, existing and newly formed SHGs were strengthened by creating and supporting a federated SHG structure at the village, mandal, and district (and,

5. Four categories were defined as follows. First, the *poorest of the poor* have food only when they obtain work, lack shelter, proper clothing, and respect and often cannot send children to school. The *poor* have no land, live on daily wages, and may need to send school-age children to work in times of crisis. The *not so poor* have some land and proper shelter, send children to public schools, and have access to bank credit and public services. The *nonpoor* own at least five acres of land, can hire laborers, send children to private schools, use private hospitals, lend money, and have high status.

eventually, state) levels through so-called “Village Organizations” and “mandal (later, “zilla”) samakhyas.” Village organizations can include 20 or more SHGs per village and are governed by an executive committee with two representatives from each member SHG. A similar pattern holds at the mandal and district levels. In addition to performing traditional functions of microfinance, such as obtaining loans from banks to on-lend resources to members, SHG federations assist with the implementation of government programs and aim to link membership to local government, possibly by forming specific committees. Other program interventions that may be implemented by group federations at the village or mandal level include agricultural marketing activities, insurance coverage, old age or disability benefits, and employment programs and job training.

An example of how such efforts can improve poverty targeting and the transparency of regular programs is the “rice credit line.” Under this scheme, mandal- and village-level SHG federations essentially take over the public distribution system by acquiring subsidized rice in bulk and making it available to their SHG members as an in-kind credit and in a more regular fashion than traditional delivery channels. The savings from bulk purchasing or better control of the supply chain can be passed on to members through lower prices. Anecdotal accounts suggest that this scheme allowed federations to circumvent some well-documented problems of the public distribution system (Kochhar 2005). In settings where such programs had remained out of reach, the prospect of gaining access to reliable rice supplies helped to attract new group members and establish discipline in terms of attendance at meetings, saving, and repayment.

Because weak capacity has been a key reason for the failure of previous credit and savings programs, these elements were complemented by an emphasis on capacity building. In each program community, community assistants were hired to provide technical support to entrepreneurial activities and increase credit demand. Trained “master bookkeepers” were deployed to educate villagers and to periodically assess SHG accounts to ensure proper management. A “community investment fund” was made available to jump-start lending, even among SHGs with low levels of accumulated member savings, by building sufficient group equity to provide collateral to borrow from banks and, at least in principle, to provide groups with a reliable source of income from interest payments.<sup>6</sup> The resources earned in this way were expected to provide revenue that could be reinvested in economic activities, such as marketing and processing.

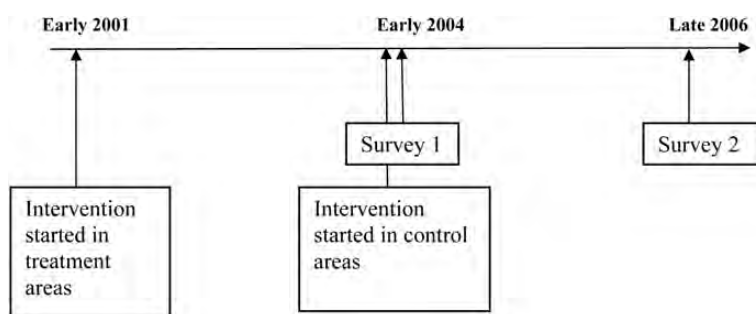
6. Community investment fund resources were initially made available to SHGs but later were offered to SHG federations at the village and mandal levels.

### *Previous Evidence and Evaluation Challenges*

A large amount of anecdotal evidence points to the effectiveness of SHGs in organizing poor women (Center for Economic and Social Studies 2003), providing access to financial services (Basu and Srivastava 2005, Shah, Rao, and Shankar 2007, Sinha 2006), and spreading innovative practices (Nair 2005). However, even in India, the reach of SHGs is highly variable across states, and there is a strong desire on the part of the government to replicate the model at a national scale. Evidence on the economic effects of SHGs is more limited. A qualitative survey in one of the districts where SHGs were first established finds that these groups helped to reduce vulnerability to drought, encouraged entrepreneurial behavior and the diversification of livelihoods, and improved social capital (Garikipati 2008). In Andhra Pradesh, after three years of program exposure, SHG participants had better nutritional and empowerment outcomes but did not perform better economically or accumulate more assets (Deininger and Liu 2009b). Positive effects of empowerment on SHG participants are also found in a larger five-state sample (Swain and Wallentin 2008). However, robust evidence on economic impacts is more difficult to find. In fact, one study concludes that although the project had a positive impact on coping with risk, empowerment, and nonfood expenditure, lasting economic impacts are unlikely (Lastarria-Cornhiel and Shimamura 2008).

To provide more detail on the effects of SHG exposure and participation, it is important to address issues of households' self-selection into program SHGs, which is based on unobservable characteristics as well as data constraints. We use a panel survey of approximately 2,500 households in 41 mandals (10 controls and 31 treatments) from 2004 and 2006, based on a randomized design, to evaluate the project. Although 10 mandals were intended to be kept as controls, the project's perceived success meant that controls were covered much sooner than anticipated. Additionally, because all of the staff's energy was devoted to implementation, no comprehensive preintervention baseline was conducted. Figure 1 shows that the surveys provide information for 2001

FIGURE 1. Timeline of Intervention and Survey



(retrospectively), when the program was initiated in treatment areas; for 2003, when it had just begun in control areas; and for 2006, when the treatment and control areas had been exposed to the program for 5 and 2.5 years, respectively.

### *Identification Strategy*

We are interested in two types of impact estimates: (i) the mean program impact on all households in the treatment areas, equivalent to the intention to treat and (ii) the ATE on members of program SHGs. To address the problem of a contaminated randomization design, we combine difference-in-difference (DD) estimates with propensity score (PS) matching throughout, as explained in greater detail below. We also explore the possible heterogeneity of impacts on members of program SHGs on the basis of their initial poverty status and the length of their participation. Furthermore, to the extent that sample sizes permit, we explore differences between program and nonprogram SHGs.

To evaluate the mean impact of the program, we use information from households located in treated areas compared with control areas. This selection is irrespective of households' participation in SHGs; rather, it is based on the program's availability to treatment areas in early 2001 and to control areas in late 2003. This method allows us to identify the "treatment effect" as the difference in impact between 5 years and 2.5 years of program exposure. Because households self-selected into program SHGs, credibly estimating the ATE on participants in program SHGs requires adjusting for differences in observable and unobservable characteristics. Instead of using nonparticipants in treatment mandals, we compare participants in the control mandals to participants in the treatment mandals. A combination of DD and PS matching allows us to circumvent the self-selection problem by drawing on the assumption that, conditional on observables, unobservables that affect self-selection into program SHGs and subsequent changes in outcomes are identical between participants in treatment and control areas. Because the control and treatment areas are subject to the same program (a condition that is satisfied here), this assumption is reasonable.

To assess the possible interaction of program impact with initial wealth, we use households' preprogram poverty rankings as a classification variable. On the basis of 2006 participation status, members of program SHGs from the corresponding wealth group in control mandals can serve as a control for those in treatment mandals, allowing us to estimate impacts on SHG member households by initial poverty category. Because control mandals entered the program in 2003, we estimate the impact of additional exposure to the program for approximately 2.5 years. Note that 2.5 to 3 years is close to the time required for groups to attain a level of maturity (Galab, Reddy, and Sreemaraju 2012). Our estimated impact can thus be interpreted as the impact of 2.5 years of program exposure for well-developed groups. Unless SHG members incur losses in the

first 2.5 years of their existence (e.g., because of high levels of investment in group capacity), our estimate will be a lower bound of true program impacts.<sup>7</sup>

To formally illustrate our approach, which combines DD and PS matching, let  $D = 1$  if a household is in a treated mandal and  $D = 0$  if it is in a control mandal. Let the outcome of being treated by the program and the counterfactual outcome at time be denoted by  $(Y_t^T, Y_t^C)$ . The gain from treatment is  $(Y_t^T - Y_t^C)$ , and we are interested in the average effect of treatment on the treated (ATT),  $E(Y_t^T - Y_t^C|D = 1)$ . The inability to observe the counterfactual outcome for treated households prevents us from directly estimating the ATT. Because we observe outcomes from 2003/04 and 2005/06, we use DD to control for household fixed effects. With  $t = 1$  denoting 2005/06 and  $t = 0$  denoting 2003/04, we can write the standard DD estimator as

$$\begin{aligned} DD &= E(Y_1^T - Y_0^T|D = 1) - E(Y_1^T - Y_0^C|D = 0) \\ &= E(Y_1^T - Y_1^C|D = 1) + B_1 - B_0 - M_1 - M_0 \end{aligned}$$

where  $B_t$  is the selection bias in period  $t$  and  $B_t = E(Y_t^C|D = 1) - E(Y_t^C|D = 0)$ .  $M_1 = E(Y_1^T - Y_1^C|D = 0)$ , and  $M_0 = E(Y_0^T - Y_0^C|D = 1)$ .  $M_1$  and  $M_0$  are the program impact of the first 2.5 years of exposure in control and treatment areas, respectively. As discussed earlier, the need to include them arises from the lack of a proper baseline survey and the contamination of the control group. The signs of  $M_1$  and  $M_0$  are important; if they are negative (positive), DD will overestimate (underestimate) the program impacts. Although their signs cannot be determined a priori, we expect  $M_1$  and  $M_0$  to be nonnegative on the basis of economic reasoning (it would be difficult to expect poor people, who are the program's primary target group, to make large up-front investments) and evidence in the literature suggesting the positive impact of short-term (three-year) exposure to the program (Deininger and Liu 2009b). If  $M_1$  and  $M_0$  are positive and selection bias is constant over time (i.e.,  $B_1 = B_0$ ), the DD estimator will yield a lower bound of program impact, which can be interpreted as the impact of exposure to the program for 5 years compared with 2.5 years' exposure, conditional on  $B_1 = B_0$ .

If the initial household characteristics that affect subsequent changes to the outcome variables are distributed differently between the treatment and the control groups, the condition  $B_1 = B_0$  or  $E(Y_1^C - Y_0^C|D = 1) = E(Y_1^C - Y_0^C|D = 0)$  will not hold. To allow for this situation, we use PS matching to balance these variables.<sup>8</sup> The assumption underlying PS matching is that, conditional on observables, changes in outcome variables, if untreated, are independent of actual treatment,  $[(Y_1^C - Y_0^C) \perp D|X]$ . This assumption has been shown to imply

7. As pointed out by a reviewer, if SHGs initially undertake significant investments, the initial program impact can be negative. In contrast, if 2.5 years is long enough for the members to derive some initial benefits, both will be positive.

8. Clustering at the village level was used throughout to control for village-level random effects.

$(Y_1^C - Y_0^C) \perp D | P(X)$ , where  $P(X)$  is the propensity score, defined as  $P(X) = \text{Prob}(D = 1 | X)$  (Rosenbaum and Rubin 1983). In the empirical implementation, we use a PS-weighted regression method (Hirano, Imbens, and Ridder 2003), which produces an estimate of the ATT as the parameter in a weighted least-square regression of the form

$$Y_{it} - Y_{i,t-1} = \alpha + \beta D_i + \varepsilon_i, \quad (1)$$

where  $i$  indexes households, and the weights equal one for treated observations and  $\hat{P}(X)/(1 - \hat{P}(X))$  for nontreated observations. See Chen, Mu, and Ravallion (2009) for empirical application of this method.

To obtain consistent and efficient estimates, we determine the common support region with

$$A_{10} = \{X | \hat{P}(X) \leq \lambda\} \quad (2)$$

where if

$$\sup_X \frac{1}{1 - \hat{P}(X)} \leq 2E \left[ \frac{1}{1 - \hat{P}(X)} | D = 1 \right] \quad (3)$$

and is a solution to

$$\frac{1}{1 - \lambda} = 2E \left[ \frac{1}{1 - \hat{P}(X)} | D = 1, \hat{P}(X) \leq \lambda \right] \quad (4)$$

otherwise. It has been shown that under homoscedasticity, this trimming method minimizes the variance of the estimated ATT (Crump, Hotz, Imbens, and Mitnik 2007). Our results are consistently based on trimmed PS-weighted DD, but we also report the results for the untrimmed simple DD for comparison. Exploring effects for different subsamples (e.g., by the length of program exposure) allows us to make inferences about the channels for effects to materialize, their heterogeneity, and the robustness of our results.

## II. DATA AND DESCRIPTIVE EVIDENCE

This section provides a more detailed description of the sample and the content and implementation of questionnaires beyond the standard for large household surveys of this nature. Descriptive statistics from a separate SHG survey illustrate the nature of the program and the extent to which it achieved its goals of expanding group coverage and providing SHGs with access to credit and skills. The group-level data provide initial evidence of the extent to which this

program may have led to broader social and economic impacts. The data also aid in the formulation of hypotheses, which can be tested using more rigorous methods.

### *Sampling Framework*

Our data are taken from two rounds of surveys, conducted at both SHG and household levels in early 2004 and late 2006. The sampling framework was designed at the beginning of the program by selecting three of the six program districts to represent the state's macroregions (Telangana, Coastal Andhra, and Rayalseema). Within these, a total of 41 mandals were selected, of which 10 were to serve as controls, where the program would not be available.<sup>9</sup> Households in each of the sample villages were to be selected randomly, with stratification aimed at oversampling the poorer groups targeted by the program.<sup>10</sup> The questionnaire consisted of male and female sections, intended to be answered separately by the main male and female members of the household.<sup>11</sup> In addition to the household sample, up to six randomly selected SHGs per village were selected to provide information on group-level activity through an SHG-level questionnaire.<sup>12</sup> The original sample comprised 2,639 households in 256 villages, 2,517 of which were also covered in the 2006 follow up. We illustrate the 2004 sample composition by district and by Mandal treatment status in table S1 in the supplemental appendix (available at <http://wber.oxfordjournals.org/>).<sup>13</sup> In table S2, we summarize the composition of the household sample for the treatment and control areas in 2004 (columns 1–3) and 2006 (columns 4–6) and those who dropped out (columns 7–9) by type of SHG participation. The contamination of the control is evident from the fact that in 2004, approximately 36 percent of control mandals (compared with 50 percent of treated ones) participated in program SHGs. With little change in the number of nonparticipants among the treatment and control groups between 2004 and 2006, much of the subsequent expansion appears to have involved the conversion of nonprogram SHGs into program SHGs. At 5 and 4 percent, respectively, attrition rates were similar in treated and control areas.

9. As noted earlier, this procedure was not followed; in late 2003, the program was made available to control mandals as well.

10. The goal was to choose four households from the poorest of the poor, three from the poor, two from the not so poor, and one from the nonpoor.

11. For example, information on health, consumption, and female empowerment, among others, was obtained from female respondents, whereas information on agricultural production was obtained from male respondents.

12. Unfortunately, SHGs were sampled independently from households, making it impossible to link the two groups. The original and follow-up SHG surveys covered 1,473 and 1,298 SHGs, respectively (the latter including 72 SHGs formed between 2003 and 2006). A total of 510 and 953 of the SHGs in the 2004 survey were program and nonprogram SHGs, respectively (see table S1).

13. A total of 175 SHGs and 122 households in the original sample could not be found in the 2006 survey.

### *A Descriptive Account of SHG-level Activities*

To illustrate the nature of the program activities and the extent of the implementation, in table 1, we report group-level variables for SHGs in the control and treatment areas in 2001 (which are based on retrospective information from the 2004 survey), 2003, and 2006 for all SHGs, irrespective of their type (i.e., program or nonprogram SHGs). We note clear improvements in the level of group activities and adherence to rules over time for SHGs, with some lag found in the control areas. In the treatment areas, the share of groups that met at least monthly rose from 48 percent in 2001 to 72 percent in 2003, where it remained relatively stable. By comparison, there is little change in meeting frequency for the control areas before 2003 (from 0.44 to 0.46), followed by a marked increase to 80 percent. Virtually all SHGs indicated that members contributed savings in meetings throughout the 2001 to 2006 period. The data also suggest that insurance, nutrition, marketing, and training actions by SHG federations at the village or mandal level were more pronounced in treatment mandals. Interventions to reduce vulnerability (i.e., in-kind credit, insurance, and disability programs), to provide a rice credit line, and to facilitate access to markets were implemented by 49, 40, and 10 percent of the SHGs in treated mandals, with increases to 71, 55, and, 23 percent, respectively, by 2006. Groups in the control areas lagged regarding the implementation of such activities in both 2003 and 2006, though approximately 18 percent (versus 22 percent in treated areas) had implemented job-training programs for SHG members by 2006.

Data regarding the lending portfolio indicate increased internal lending and access to loans from banks and the project-supported community investment fund. The share of groups in the treatment area practicing internal lending increased from 18 percent in 2001 to 53 percent in 2003 and to 87 percent in 2006, compared with rates of 20, 37, and 90 percent for the three respective years in the control area. Nonetheless, with exchange rates of Rs. 45 per US \$ in March 2004 and September 2006, loan sizes remained modest for both control and treatment groups, with a median size between Rs. 4,500 and 10,500. The median duration of internal loans was 12 months; half of these loans were used for consumption smoothing.

In 2001, only 3 percent of groups in the treatment area accessed program funds, a figure that increased to 25 percent in 2003 and to 62 percent in 2006. The SHG survey provides information on the purpose of each loan. At four to ten times the median internal loan size, the loans from project-supported funds were larger and more likely to be used for investment than internal lending. Although groups in the control area did not have access to program funds in 2001 or 2003, they eventually caught up with the treatment areas, with 55 percent gaining access to program funds by 2006.

In line with the program's goal of linking SHGs to commercial banks, access to bank loans increased for groups in both the control and treatment

TABLE 1. Summary of SHG Activities in 2001, 2003, and 2006

	2001			2003			2006		
	Control	Treated	Sig.	Control	Treated	Sig.	Control	Treated	Sig.
<b>SHG functioning</b>									
Meet at least monthly	0.44	0.48		0.46	0.72	***	0.80	0.74	**
Members make savings in meetings	0.97	0.88	***	1.00	1.00	*	0.92	0.88	**
<b>Non-microcredit activities</b>									
Activities to reduce vulnerability	0.13	0.07	***	0.24	0.49	***	0.48	0.71	***
Access to rice credit line	0.00	0.00		0.05	0.40	***	0.23	0.55	***
Marketing activities undertaken	0.02	0.03		0.05	0.10	***	0.14	0.23	***
Employment program/job training	–	–	–	–	–	–	0.18	0.22	*
<b>Microcredit activities</b>									
<i>Practice internal lending</i>	0.20	0.18		0.37	0.53	***	0.90	0.87	
if yes, median internal loan size	6950	4500	–	6450	5850	–	10005	10500	–
if yes, median internal loan length	–	–	–	12	10	–	12	12	–
share for consumption smoothing	0.46	0.47		0.51	0.48		0.45	0.48	
share for investment	0.38	0.45		0.41	0.44		0.47	0.44	
<i>Have access to program fund</i>	0.00	0.03	***		0.25	***	0.55	0.62	**
if yes, median size	–	36012	–	24600	34000	–	21000	40675	–
if yes, median length (months)	–	–	–	15	20	–	12	20	–
Main purpose: cons. smoothing		0.03	–	–	0.07	–	0.07	0.05	
Main purpose: investment	–	1.00	–	–	0.96	–	0.90	0.90	
<i>Access bank loans</i>	0.35	0.27	***	0.36	0.44	***	0.87	0.89	
if yes, median size	15000	15000		30000	24000		56500	50000	
if yes, median length (months)	–	–	–	12	12	–	20	20	–
Main purpose: cons. smoothing	0.50	0.36	***	0.37	0.39		0.30	0.07	***
Main purpose: investment	0.66	0.76	**	0.73	0.81	*	0.64	0.88	***

Source: Authors' analysis using data from 2004 and 2006 AP DPIP SHG surveys (2001 information collected retrospectively from the 2004 survey).

Significance levels of the difference between the control and treatment found by *t* test are indicated by stars: \* 10%; \*\* 5%; \*\*\* 1%.

SHGs include program and nonprogram SHGs, and empty cells indicate unavailable data or are not applicable.

areas from 2003 to 2006. The relatively slow increase in treatment areas from 2001 to 2003 illustrates startup problems in developing the program's implementation structure and suggests that access to bank loans is not immediate. From 2001 to 2006, the median size of bank loans (on-lend internally) increased from Rs. 15,000 to Rs. 50,000, highlighting groups' greater credit-worthiness, which can be at least partly attributed to the infusion of program funds.

This summary of SHG activities points to a number of hypotheses regarding program impacts. First, improved credit access, especially the ability to use loans from banks and program funds, should increase income by encouraging investment and asset accumulation. Second, participation in groups and associated access to internal lending may reduce vulnerability, improve nutritional status, and increase income. Third, increased asset endowments and income levels could lead to higher consumption and nutritional intake levels. Fourth, interventions such as the rice credit line or efforts to provide training in dairy production and to encourage the use of credit funds to acquire cows or buffaloes may have resulted in changes in nutritional intake (calories and protein) among SHG participants in program areas by reducing the price of food independently of income effects.

Because access to program funds, bank loans, and the rice credit line were all targeted at SHGs, we expect economic benefits to be limited to SHG members. With the possible exception of nutritional benefits gained from access to a rice credit line, spillovers to non-SHG members in treatment areas are unlikely. Moreover, to the extent that SHGs offer members a menu of options, the realization of these depends, at least partially, on wealth and the nature and magnitude of effects and may be affected by initial endowments, even for SHG participants. For example, a rice credit line and internal lending will be particularly attractive to the poor, whereas marketing activities provide larger benefits to the nonpoor. At the same time, the poor and the nonpoor may differ in their use of loan funds, with the poorest potentially being averse to taking out large loans because of the lack of complementary assets or skills or because of the fear of their inability to repay.

### *Descriptive Evidence at the Household Level*

The household survey contained male and female modules, administered separately to a key male or female person in the household, normally the household head and spouse. In table S3, we describe levels of participation in program SHGs by poverty status in three different periods: (i) 2001, the year the program was implemented in the treatment areas; (ii) 2003, the year the program began in the control areas; and (iii) 2006, the year of the final survey. Membership increased over time, both in treatment and control areas, with a lag in the latter. In 2006, approximately 59 and 47 percent of the treated and control areas, respectively, were members of program SHGs.

Outcome variables available in the survey include the value of consumption, nutritional intake, and levels of assets, which we express in per capita terms based on adult equivalents.<sup>14</sup> Consumption includes food and nonfood consumption over the past 30 days and lumpy items over the past year.<sup>15</sup> We compute the intake of calories and protein by multiplying quantities of more than 30 food items in the questionnaire's consumption section, with their caloric and protein content based on the main reference for Indian foods (Gopalan, Rama Shastri, and Balasubramanian 2004).<sup>16</sup> Nonfinancial assets include consumer durables, productive assets, and livestock assets.<sup>17</sup>

Table S4 illustrates outcome variables for control and treated mandals in the 2003/2004 and 2005/2006 periods. Comparing initial conditions between the households in the control and treated mandals in table S5 highlights some differences, such as much higher levels of scheduled tribes in treatment areas than in control areas (24 versus 6 percent), which could be due to the rather small sample of control mandals. As shown in table S6, compared with those who remained in the sample, households that dropped out were smaller in size, poorer, and more likely to be female-headed, landless, and illiterate. These findings are consistent with the notion that the probability of dropping out is higher among migrants or female-headed households.

In table S7, we present the regression results of early placement of a program for the overall sample (column 1), for the sample with participants of program SHGs only (column 2), and for the sample with nonparticipants only (column 3). In each case, the dependent variable is 1 if the household (in the subgroup of interest) is located in a treated mandal and 0 otherwise. Explanatory variables for demographics (location, caste, female headship, and literacy) and initial economic conditions (poverty status, land ownership, consumption, nutritional intake, and nonfinancial assets) are from the 2004 household survey. The low pseudo  $R^2$  is in line with the random selection of control mandals, although the results suggest that randomization failed to eliminate some systematic differences between treatment and control areas, possibly because of small sample sizes.

14. To obtain adult equivalent measures for caloric and protein consumption, we use nutritional requirements by sex and age as weights (Gopalan, Rama Shastri and Balasubramanian 2004). Additionally, to generate adult equivalent measures of income and total consumption, we weight those above 60 and below 14 by 0.78.

15. Although the survey instrument is less disaggregated than that used by the National Sample Survey, it follows the overall structure used there.

16. For fruits or vegetables where the survey includes only aggregate spending, we use the 55th round of the National Sample Survey to derive the price and caloric content of a representative basket of those consumed in Andhra Pradesh.

17. Financial assets were excluded because of concerns about misreporting. Asset values were measured as of December 2003 in the 2004 survey and as of June 2006 in the 2006 survey.

### III. ESTIMATION RESULTS

The methodological framework provides estimates of the program's overall effects and its specific effects for different groups. Positive and significant effects on SHG members' expenditure, nutritional intake, and asset accumulation appear to be driven mainly by the poorer participants. Longer exposure to the program yields a higher program impact, as expected. We do not find evidence of spillovers, either from participants in program SHGs to nonparticipants or from program SHGs to nonprogram SHGs, in the treatment areas.

#### *Intention-to-Treat Estimate*

To estimate the intention-to-treat estimator, we compare the households in the treatment mandals with those in the control mandals, irrespective of their SHG membership status. Because the above evidence points to significant differences in observables between treated and nontreated areas, we use estimated propensity scores (from the first-step logit regression, as reported in the first column of table S7) to balance variables that may influence outcomes. The first column in table S9 displays differences in the means of the matching variables between treatment and control areas for the PS-weighted and trimmed samples, illustrating that trimming and matching based on the estimated PS balanced all of the variables of interest.

Estimates for the ATEs on households in the treated mandals based on the trimmed sample are reported in the bottom panel of table 2. Simple DDs based on the total sample are shown in the top panel for comparison.<sup>18</sup> We find a significant impact of the intention to treat on investment in nonfinancial assets, which is estimated to be higher by Rs. 453, or 16 percent, compared with the counterfactual. This result suggests that once groups achieved a level of maturity, the objective of inducing higher levels of investment and capital formation was achieved. However, we fail to find a significant impact on total consumption or nutritional intake after trimming and reweighting. Because the estimated impact refers to the average of SHG members and nonmembers in treatment mandals, this result suggests the possibility of a program effect on members that cannot be detected in this setting. To explore this possibility, we turn to the impact on program SHG members alone.

#### *Average Treatment Effects on Program SHG Members*

To estimate program impacts on members of program SHGs, the treatment group is defined as households in the treatment areas that joined program SHGs before 2003. By comparison, the control group comprises households in

18. In tables 2 to 4, the number of observations eliminated through trimming can be obtained by comparing the number of observations between the total and trimmed samples in the relevant tables. For example, in table 2, for consumption per capita, trimming drops 181 (=1819-1638) observations in the treated group and 9 (=489-480) observations in the control group. Figure S1 in the appendix graphically illustrates the impact of trimming.

TABLE 2. Estimated Average Treatment Effects on Households Living in Treatment Areas

	Untrimmed sample, simple DD				
	Treated	Control	DD	(s.e.)	Sig.
Consumption p.c. (Rs/year)	1364	892	472	(215)	**
Food (Rs/year)	478	477	1	(109)	
Nonfood (Rs/year)	886	415	471	(155)	***
No. of obs.	1819	489			
Energy intake p.c. (Kcal/day)	156	213	-57	(60)	
Protein intake p.c. (g/day)	-0.90	0.37	-1.27	(1.30)	
No. of obs.	1917	518			
Nonfinancial assets p.c. (Rs)	750	411	338	(163)	**
No. of obs.	1926	519			
	Trimmed sample, PS weighted DD				
	Treated	Control	DD	(s.e.)	Sig.
Consumption p.c. (Rs/year)	1436	1103	333	(212)	
Food (Rs/year)	547	497	50	(118)	
Nonfood (Rs/year)	888	606	282	(152)	*
No. of obs.	1638	480			
Energy intake p.c. (Kcal/day)	214	179	36	(68)	
Protein intake p.c. (g/day)	1.05	-0.83	1.89	(1.52)	
No. of obs.	1751	507			
Nonfinancial assets p.c. (Rs)	754	301	453	(190)	**
No. of obs.	1758	508			

Source: Authors' analysis using data from 2004 and 2006 AP DPIP household surveys.

Note: "Treated" and "control" denote the change from 2004 to 2006 in an outcome for the treated group and the control group, respectively. DD denotes the double difference, and (s.e.) denotes standard errors. Significance of coefficients is shown as follows: \* at 10%; \*\* at 5%; \*\*\* at 1%.

control areas that were registered as program SHG members in 2006. We only keep members who joined before 2003 for the treatment mandals and eliminate 342 "late joiners" who joined a program SHG at a later date in the treatment areas. We also exclude 107 households who left SHGs after 2004, noting that these departures were the result of SHGs being dissolved or becoming dysfunctional rather than individuals joining different SHGs.<sup>19</sup> Therefore, our treatment group includes households that were exposed to the program for between 3.5 and 6 years, whereas the control group includes households with program exposure of less than 3 years.<sup>20</sup>

19. Project staff indicate that the main reason for individuals to exit groups is either expulsion, in most cases because of conflict or failure to honor repayment commitments, or the dissolution of the entire group, which is often due to conflict. Incentives for functioning groups to welcome such individuals are limited, implying that mobility across groups is virtually nonexistent.

20. We exclude 107 households that had been SHG members in 2004 but that were no longer members in 2006 because their SHG had become dysfunctional.

TABLE 3. Impact on Program SHG Participants and Nonparticipants

	Untrimmed sample, simple DD				
	Participants			Nonparticipants	
Consumption p.c. (Rs/year)	768	(272)	***	80	(338)
Food (Rs/year)	234	(135)	*	−210	(167)
Nonfood (Rs/year)	535	(212)	**	290	(247)
No. of obs.	510 + 239 = 749			917 + 221 = 1138	
Energy intake p.c. (Kcal/day)	85	(75)		−190	(88)
Protein intake p.c. (g/day)	1.96	(1.62)		−3.92	(1.84)
No. of obs.	535 + 246 = 781			970 + 241 = 1211	
Nonfinancial assets p.c. (Rs)	549	(243)	**	203	(243)
No. of obs.	539 + 243 = 782			977 + 245 = 1222	
	Trimmed sample, PS weighted DD				
	Participants			Nonparticipants	
Consumption p.c. (Rs/year)	552	(309)	*	−122	(341)
Food (Rs/year)	307	(168)	*	−257	(176)
Nonfood (Rs/year)	245	(223)		135	(238)
No. of obs.	414 + 228 = 642			839 + 218 = 1057	
Energy intake p.c. (Kcal/day)	161	(87)	*	−119	(92)
Protein intake p.c. (g/day)	4.47	(1.93)	**	−1.75	(2.00)
No. of obs.	456 + 237 = 693			887 + 235 = 1122	
Nonfinancial assets p.c. (Rs)	755	(276)	***	121	(262)
No. of obs.	438 + 234 = 672			892 + 241 = 1133	

Source: Authors' analysis using data from 2004 and 2006 AP DPIIP household surveys.

Note: Standard errors in parentheses. Significance of coefficients is shown as follows: \* at 10%; \*\* at 5%; \*\*\* at 1%.

The column "participants" compares participants with 3.5 to 6 years in program SHGs in treatment villages to program participants with less than three years of exposure in control villages. The "nonparticipant" column compares nonparticipants in treatment villages to those in control villages.

The first column of table 3 reports the ATE on program SHG members with DD and PS matching results based on the trimmed sample in the bottom panel and simple DDs in the top panel for comparison. In contrast to the lack of significance (except for nonfinancial assets) in the intention-to-treat estimation, we find significant impacts on the consumption, nutritional intake, and accumulation of nonfinancial assets by members of program SHGs. The magnitude of these impacts is large; the increment in per capita consumption compared with the counterfactual is estimated to amount to Rs. 552/year (approximately US\$ 11), or approximately 7 percent. Estimated increases in the per capita intake of energy and protein as well as investments are equivalent to increases

of 8 (161 calories/day), 10 (4.5 grams/day), and 24 (Rs. 755) percentage points, respectively. These are large effects, especially because a number of factors, such as cross-border spillovers and learning by the agencies responsible for implementation, are likely to bias estimates downward. In view of the finding of nonnegligible gains in nutritional intake, these effects may have occurred during the program's first three years (Deininger and Liu 2009b).

To assess our identification strategy, we run an identical regression comparing nonparticipants in treatment areas to nonparticipants in control mandals as a falsification test. If positive impacts for program SHG members were driven by factors that were not controlled for (for example, if the timing of program placement was based on unobservables that affected outcomes), this test should yield positive impacts for non-SHG members in treatment areas.<sup>21</sup> The results, as reported in column 2 of table 3, suggest that this is not the case; estimated impacts are small in magnitude, and none of the impacts is statistically significant at any conventional level. Because the standard errors are comparable to those obtained for SHG members, a positive impact is unlikely to be due to a lack of precision in the estimates, lending credence to the validity of our identification strategy.

### *Robustness Checks and Heterogeneity of Program Impacts*

Exploring effects for subsamples (e.g., by length of program exposure or poverty status) allows us to draw inferences about the channels in which effects materialize, the robustness of our results, and their heterogeneity.

First, we follow Behrman, Cheng, and Todd (2004) in exploring the effect of varying the length of exposure by comparing program impacts for subsamples of program SHG members who joined at different times. Estimates of exposure to the program for a specific length of time can be derived by comparing pairs of groups using DD and PS matching. The results, summarized in table 4, suggest that longer exposure yields higher impacts for all statistically significant results. Although different sample sizes must be considered when interpreting the results, estimated differences in impacts are greatest between the group that joined in 2001 and the group with the least exposure (i.e., the group that joined in 2004 or thereafter). There are few significant differences for one-year exposure, suggesting that this period is too short to yield clear effects. In contrast, significant differences emerge between the 2001 and 2003 groups and the 2002 and 2004 groups.

Second, it is interesting to examine whether the effects identified here are due to the “new” model of organizing SHGs supported by the program rather than to longer exposure to the traditional SHG model. To determine the source of this effect, we compare whether impacts differ between members of program SHGs and those of nonprogram SHGs (“old” SHGs). Because the program under study differs from “old” programs in its level of organization

21. The test is valid if program benefits accrue to SHG members only.

TABLE 4. Pairwise Difference in Program Impacts Among Program SHG Members Joining at Different Points in Time

	One-year exposure							
	2001 vs. 2002		2002 vs. 2003			2003 vs. 2004		
Cons p.c. (Rs/a)	−44	(338)	537	(251)	**	167	(265)	
Food (Rs/a)	−128	(181)	138	(143)		208	(140)	
Nonfood (Rs/a)	84	(258)		399	(184)	**	−41	(210)
No. of obs.	187 + 307 = 494		187 + 387 = 574			386 + 433 = 819		307 + 387 = 694
Energy p.c. (Kcal/d)	58	(95)	90	(78)		58	(81)	
Protein p.c. (g/day)	−0.33	(2.18)	2.42	(1.81)		1.36	(1.98)	
No. of obs.	192 + 323 = 515		192 + 402 = 594			399 + 452 = 851		323 + 402 = 725
Nonfinancial assets p.c. (Rs)	−216	(391)	516	(301)	*	37	(254)	
No. of obs.	194 + 324 = 518		193 + 403 = 596			394 + 453 = 847		324 + 403 = 727
	Multiyear exposure							
	2001 vs. 2003		2002 vs. 2004			2001 vs. 2004		
Cons p.c. (Rs/a)	579	(354)	547	(273)	**	821	(343)	**
Food (Rs/a)	64	(183)	308	(140)	**	375	(163)	**
Nonfood (Rs/a)	516	(257)	**	239	(226)	446	(277)	
No. of obs.	187 + 387 = 574		305 + 430 = 735			187 + 433 = 620		
Energy p.c. (Kcal/d)	168	(105)	109	(81)		240	(101)	**
Protein p.c. (g/day)	2.93	(2.27)	3.02	(1.87)		4.97	(2.30)	**
No. of observations	192 + 402 = 594		321 + 453 = 774			191 + 453 = 644		
Nonfinancial assets p.c. (Rs)	333	(330)	364	(332)		262	(370)	
No. of observations	193 + 403 = 596		322 + 455 = 777			192 + 456 = 648		

Source: Authors' analysis using data from 2004 and 2006 AP DPIIP household surveys.

Note: Trimmed sample with PS-weighted DD method is used throughout.

Significance of coefficients is shown as follows: \* at 10%; \*\* at 5%.

and outreach to the poor, we expect differences mainly in terms of access to public goods through village- or higher-level federations taking responsibility for the implementation of government programs. Unfortunately, the program's tendency to convert nonprogram SHGs into program SHGs leaves us few participants in "old" groups by 2006. Therefore, we rely on the SHG type in 2004 to define program and nonprogram SHG members and estimate the effects of the "new" model using DD and PS matching. The resulting estimates can be interpreted as the added effect of the "new" SHG model compared with the "old" SHG model. The results, as shown in table 5, point to significant effects on energy and protein intake but not on consumption or asset accumulations. This finding suggests that, in addition to the improved inclusion of the marginal groups, a key improvement of the "new" model is in non-microfinance activities, such as the rice credit line.

Third, we explore spillover effects from program to nonprogram SHGs in treatment areas. Such spillovers could arise from SHGs taking over the implementation of specific government programs (e.g., the public distribution system). We define the treatment group as the members of nonprogram SHGs in the treatment areas and the control group as the members of nonprogram SHGs in the control areas, and we use DD and PS matching. The results of this test are reported in table 6. We fail to find significant spillover effects for any of the outcome variables. However, this finding may be attributed to a small sample size (fewer than 250 observations).

Fourth, as noted earlier, the above estimates of an additional 2.5 years of exposure to the program among mature groups are only part of the total program effect. To the extent that our interest is in the latter, learning more about short-

TABLE 5. Estimated Average Treatment Effects on Program versus Nonprogram SHG Members

	Treated	Control	DD	s.e.	Sig.
Consumption p.c. (Rs/year)	1426	1308	118	389	
Food (Rs/year)	510	401	109	182	
Nonfood (Rs/year)	916	907	9	281	
No. of obs.	531	265	531	265	
Energy intake p.c. (Kcal/day)	251	49	202	94	**
Protein intake p.c. (g/day)	1.46	-2.89	4.35	2.39	*
No. of obs.	549	276	549	276	
Nonfinancial assets p.c. (Rs)	920	725	195	362	
No. of obs.	533	273	533	273	

Source: Authors' analysis using data from 2004 and 2006 AP DPIIP household surveys.

Note: Trimmed sample with PS-weighted DD method used throughout.

Figures in the "treated" and "control" columns refer to the difference in outcomes between 2006 and 2004 for program SHG members and nonprogram SHG members based on the 2004 status of SHG type. DD denotes the double difference, and s.e. denotes standard errors.

Significance of coefficients is shown as follows: \* at 10%; \*\* at 5%.

TABLE 6. Estimated Spillover Effects on Nonprogram SHG Members Residing in Program Areas

variable	Treated	Control	DD	s.e.	Sig.
Consumption p.c. (Rs/year)	1813	1154	659	774	
Food (Rs/year)	482	513	-31	362	
Nonfood (Rs/year)	1331	640	691	541	
No. of obs.	169	51	169	51	
Energy intake p.c. (Kcal/day)	133	106	27	147	
Protein intake p.c. (g/day)	-0.13	-1.66	1.53	4.25	
No. of obs.	175	52	175	52	
Nonfinancial assets p.c. (Rs)	1041	1124	-84	589	
No. of obs.	175	49	175	49	

Source: Authors' analysis using data from 2004 and 2006 AP DPIP household surveys.

Note: Trimmed sample with PS-weighted DD method used throughout. Figures in the "treated" and "control" columns refer to changes from 2004 to 2006 in outcomes for program SHG and nonprogram SHG members. DD denotes the double difference, and s.e. is the standard error. Significance of coefficients is shown as follows: \* at 10%; \*\* at 5%; \*\*\* at 1%.

term effects (i.e., the impact of exposure to the program for 2.5 years) is of interest. To do so, we use the entire sample to regress outcome variables of interest on the interaction of treatment location and membership in program SHGs (in 2004 for treatment areas and in 2006 for control areas) and other controls.<sup>22</sup> The results (in table S12) indicate nonsignificant effects for all of the outcome variables considered. These results increase our confidence that the main result will not be biased upward.

Finally, because the program explicitly targets the poor, we compare estimated impacts among groups with different initial poverty statuses.<sup>23</sup> The results, reported in table 7, suggest positive, significant, and relatively large impacts in terms of food consumption (Rs. 748 per capita) and nutritional intake (364 kcal/day and 8.93 g/day of protein), which increase by approximately 15 percent for initially poor households. The only significant impact for the poorest groups is an increase in nonfinancial assets. Although the effects on consumption and nutritional intake are positive, they are statistically nonsignificant. No significant impacts are detected for nonpoor members, which may have resulted from limited power due to the small number of observations in the subsample.

22. This procedure is essentially a DD estimation of  $\{(2004 \text{ outcomes of program-SHG members in treatment areas} - 2004 \text{ outcomes of nonmembers in treatment areas}) - (2004 \text{ outcomes of future members in control areas} - 2004 \text{ outcomes of future nonmembers in control areas})\}$ .

23. In table S4, we list means of outcome variables for our subgroups of the poorest of the poor, the poor, and the not-so-poor/nonpoor members separately. Not surprisingly, descriptive statistics suggest that richer households had higher consumption and assets than poorer ones in both periods. We observe the same trend for nutritional intake, although this finding is not as obvious as in the case of consumption and assets.

TABLE 7. Heterogeneous Impacts by Initial Poverty Status

	Untrimmed sample, simple DD						
	Poorest of the Poor		Poor			Not-So-Poor/Not Poor	
Consumption p.c. (Rs/year)	257	(330)	816	(465)	*	1357	(729)
Food (Rs/year)	−91	(202)	384	(224)	*	413	(332)
Nonfood (Rs/year)	348	(247)	432	(368)		945	(531)
No. of obs.	228 + 84 = 312		178 + 87 = 265			106 + 68 = 174	
Energy intake p.c. (Kcal/day)	76	(120)	156	(110)		30	(172)
Protein intake p.c. (g/day)	0.74	(2.78)	3.49	(2.46)		1.85	(3.66)
No. of obs.	239 + 86 = 325		187 + 89 = 276			111 + 71 = 182	
Nonfinancial assets p.c. (Rs)	439	(251)	452	(410)		931	(749)
No. of obs.	242 + 87 = 329		191 + 88 = 279			109 + 68 = 177	
	Untrimmed sample, simple DD						
	Poorest of the Poor		Poor			Not-So-Poor/Not Poor	
Consumption p.c. (Rs/year)	319	(385)	1394	(865)		876	(909)
Food (Rs/year)	239	(228)	748	(280)	***	185	(469)
Nonfood (Rs/year)	79	(281)	646	(689)		691	(599)
No. of obs.	176 + 81 = 257		144 + 84 = 228			79 + 65 = 144	
Energy intake p.c. (Kcal/day)	156	(135)	364	(121)	***	−28	(199)
Protein intake p.c. (g/day)	4.12	(3.40)	8.93	(2.96)	***	1.56	(5.23)
No. of obs.	209 + 84 = 293		156 + 88 = 244			89 + 69 = 158	
Nonfinancial assets p.c. (Rs)	589	(311)	644.78	(457)		502	(824)
No. of obs.	203 + 85 = 288		159 + 87 = 246			86 + 67 = 153	

Source: Authors' analysis using data from 2004 and 2006 AP DPIIP household surveys.

Standard errors in parentheses.

Significance of coefficients is shown as follows: \* at 10%; \*\* at 5%; \*\*\* at 1%.

### *A Simple Cost-Benefit Analysis*

To assess total program benefits, we note that when our follow-up survey was conducted in 2006, the program had reached 2.29 million households, with an average size of 4.79 adult equivalents. Approximately 47.4 percent of SHG members in our sample joined program SHGs before 2003. Multiplying the estimated ATT on per capita consumption (US\$ 11) with the number of participants enables us to establish a bound for project benefits. We do so by assuming two extreme scenarios. If future benefits are assumed to be maintained at current levels, applying a 0.9 discount factor puts the estimated net present value of project benefits at US\$ 567.1 million, yielding a benefit-cost ratio of 3.77:1.<sup>24</sup> Under the more conservative assumption that consumption benefits only occurred in the period covered by our survey, the estimated benefits would amount to 56.7 million. In this case, project-supported loans, which have a delinquency rate of 23 percent (Deininger and Liu 2009a), would reduce project costs to 88 million, and the associated benefit-cost ratio would be 0.64:1.<sup>25</sup> The actual benefit-cost ratio will be between these extremes. Given the positive impacts on asset accumulation found by our analysis, the extreme scenario of benefits below project cost may be unlikely. The extent to which groups established under the program are sustained is obviously a key determinant of the program's longer-term impact.

## IV. CONCLUSION AND POLICY IMPLICATIONS

This paper was motivated by the notion that, despite considerable interest in expanding SHG approaches to microfinance, rigorous evaluation of the impact of such interventions is scant. Even studies documenting clear social, empowerment, and nutritional impacts have been unable to ascertain economic effects. The household data collected 2.5 to 5 years after the start of the program allow us to assess the impact of longer program exposure, which, under plausible conditions, can proxy for the difference between a group that is fully functional and an immature one.

Using propensity score-weighted double differences on an appropriately trimmed sample and noting that our estimates are likely to constitute a lower bound of true effects, we find that SHG participation had significant economic impacts in the areas considered. If benefits are maintained at current levels, they significantly exceed program costs. The benefits are not confined to those who were already affluent; in fact, there is significant asset accumulation

24. The total project cost is US\$ 150.60 million, of which 111 million was provided by a World Bank loan.

25. Of the total project cost, project-supported loans offered through the CIF represented 81.4 million. Therefore, the cost is computed as follows:  $(150.6 - 81.4) + 81.4 \times 23\% = 88$ . Considering repayment rates is not necessary under the assumption that future benefits will be maintained forever because the CIF fund will remain in the group federations.

among the poorest of the poor, who (partly as a result of participation in SHGs and partly because gains in calorie and protein intake may have been realized earlier in the program) saw their consumption increase less than that of the poor. This finding suggests that if they participate in SHGs, the poorest individuals appear to benefit not only socially but also economically.

Our results suggest that a program that not only fosters group formation but also supports more mature groups through federation and access to credit can produce significant economic benefits in the long term. To assess the overall desirability and impact of such programs, a key question concerns the extent to which the benefits will be maintained once outside support is terminated. The answer to this question will at least partly depend on whether the SHGs established by the program continue to operate (possibly adjusting the services offered to the level of member development) and, related to this, whether beneficiary households will be able to use the one-time injection of credit and capacity to place them on a permanently higher trajectory of economic activity and asset accumulation. Answering this question is beyond the scope of this paper and will require additional information based on both group and individual activity after external support has ceased. Nonetheless, our finding of nonnegligible economic impacts in the case of Andhra Pradesh implies that further investigation of the determinants and implications of the sustainability of SHGs and the benefits that they provide to their membership could be of considerable interest for researchers and policy makers alike.

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