



## Review

# Failure vs. displacement: Why an innovative anti-poverty program showed no net impact in South India

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## ABSTRACT

We analyze a randomized trial of an innovative anti-poverty program in South India, part of a series of pilot programs that provide “ultra-poor” households with inputs to create new, sustainable livelihoods (often tending livestock). In contrast with results from other pilots, we find no lasting net impact on income or asset accumulation in South India. We explore concerns with program implementation, data errors, and the existence of compelling employment alternatives. The baseline consumption data contain systematic errors, and income and consumption contain large outliers. Steps to address the problems leave the central findings largely intact: Wages for unskilled labor rose sharply in the area while the study was implemented, blunting the net impact of the intervention and highlighting one way that treatment effects depend on factors external to the intervention itself, such as broader employment opportunities.

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## 1. Introduction

The poorest of the poor face broad challenges. A common policy response is to create safety nets with publicly-funded income transfers that ensure a basic standard of living. BRAC, a globally-recognized NGO based in Bangladesh, is building on the standard safety net idea by instead giving poor households a larger quantity of resources in a shorter period of time. With an eye on promoting economic advancement rather than just ensuring survival, BRAC couples financial transfers with transfers of assets and training to help recipients build new livelihoods as self-employed, small-scale entrepreneurs (Bandiera et al., 2013). The bet is on the possibility of escape from a life of extreme poverty into a life of economic self-sufficiency, an idea with roots in the economics of poverty traps (Bowles et al., 2006; Sachs, 2005).

BRAC created the “ultra-poor graduation” model in Bangladesh, and donors have supported its replication and evaluation in other sites. Karlan and Goldberg (2014) describe results from randomized trials in India (West Bengal), Pakistan, Ghana, Ethiopia, Peru, and Honduras, carried out under the umbrella of Innovations for Poverty Action. Bandiera et al. (2013) report on RCT results from Bangladesh.<sup>1</sup> While researchers

have been careful not to over-sell their results, the findings have mostly been very encouraging. In Bangladesh, for example, treatment households had 38% higher earnings than control households four years after the program started. In West Bengal, treatment households saw business income increased by 48% relative to control households, and consumption increased by 11% (impacts were measured three to 3.5 years after the program started; Duflo, 2012). Karlan and Goldberg (2014) report on a range of impacts two and three years after baseline surveys; beyond the successes in West Bengal, they show notable impacts on consumption in Ethiopia, promising results in Pakistan, and weaker results in Peru and Honduras.<sup>2</sup>

We report on a parallel study of a similar “ultra-poor graduation” program in the South Indian state of Andhra Pradesh, implemented by the NGO arm of SKS, a large commercial microfinance institution. Despite expectations that the intervention could be transformative (SKS 2011), three years after the program started in 2007 there were no measured, statistically significant net impacts on the key outcomes: average household income, consumption, asset accumulation, and use

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<sup>1</sup> Information on all sites (including the SKS NGO pilot evaluated here) is available at <http://graduation.cgap.org/>. BRAC's program is called Targeting Ultra Poor (TUP), and the approach is often simply tagged as “graduation” programs. Information on the Innovation for Poverty Action studies are available at <http://www.poverty-action.org/ultrapoor>.

<sup>2</sup> A February 14, 2014 blog post by Sue Fleming of the donor consortium, CGAP, reports: “Pressed on how upbeat he was over the findings, [Dean] Karlan said of the 500 or so randomized trials [Innovations for Poverty Action] had done along with MIT's Abdul Latif Jameel Poverty Action Lab over the years, he could count only five or six that had risen to the level where he was confident of advising policymakers to scale up the work. These included projects on deworming, chlorine dispensers, and remedial education. The Graduation Approach is now being added to that list.” Accessed online (5/19/14): <http://graduation.cgap.org/2014/02/21/researchers-highlight-graduation-research-say-time-is-right-to-scale-up/>.

of financial services. Was the program a failure? Was the study compromised? How do these results inform discussions about replicating the intervention elsewhere?

The SKS NGO pilot was funded alongside the other ultra-poor graduation pilots, but was implemented and evaluated independently. Like the other ultra-poor graduation pilots, the SKS program targeted the poorest households, focusing on those with few assets and chronic food insecurity. The SKS NGO intervention provided an asset and basic resources to start a sustainable livelihood. Households also received training, savings accounts, health consultations, and facilitation of access to government services. About 90% of households chose to raise livestock (most often buffaloes), although some households chose trades like tailoring or shop-keeping. After 18 months of weekly meetings and support, the program came to a close, with the hope that the households would then be equipped to “graduate” out of extreme poverty.

We consider four explanations for the lack of net impacts in Andhra Pradesh: (1) data problems in the empirical study, (2) design and implementation problems in the ultra-poor intervention, (3) low take-up and high drop-out rates, and (4) shifts away from wage employment which offset economic gains from program participation.

The data collected for the evaluation are imperfect, and we first describe the nature of errors and tests for robustness of results. The study involved three rounds of data collection: (a) a baseline survey before the intervention was implemented, (b) a midline survey at the end of the intervention, and (c) an endline survey a year after the intervention ended. There are two main data difficulties. First, baseline consumption data are implausibly large relative to income data, relative to data from later years, and relative to prevailing poverty lines. Second, outliers in the income and consumption data substantially diminish the correlations between key data series (consumption, income, and assets) in a given data wave and across time for a given variable. Both are serious issues and cannot be wholly fixed, but we take steps to limit their influence. The main results are robust to analyzing consumption using only the endline (i.e., without the problematic baseline consumption data), and to reducing the influence of outliers by Winsorizing the income and consumption data at the top 5% of observations (Appendix Tables 1 and 2). We also show that patterns of average household income are consistent with other data, including information on time use and the disaggregated composition of income by occupation.

Some observers have questioned the program's design and implementation. Post-intervention auditors, for example, have questioned the program's lack of customization for individual households, lack of consumption support for households, and lack of follow-up after the program ended at 18 months (Jawahar and Sengupta, 2012). These questions should be considered in future designs, but detailed monitoring data show that the intervention was largely implemented as designed, and budget data show that the intervention was comparable in cost to other ultra-poor graduation pilots in India.

The third possible reason for the lack of measured net impact is that program effectiveness was undermined by low take-up and high drop-out rates. The SKS NGO intervention required no fees from eligible participants and provided a substantial asset transfer, so it is not surprising that 70% of eligible households participated. This take-up rate is higher than that in the West Bengal replication where strong positive effects of an ultra-poor intervention were found (52% in the West Bengal study received and kept the assets; Banerjee et al., 2011, Fig. 1). The real worry is high drop-out. By the endline, among the households that participated and chose to raise an animal as their project, only 43% still owned an animal. This asset loss/sale figure is much larger than that found in West Bengal. We implemented a follow-up survey to verify and explore why households no longer owned their animals. Two-thirds reported selling their animals, and many used the proceeds to pay off debt. Compared to households that still had their animals, households that no longer owned animals in the endline survey were 19 percentage points less likely to have outstanding loans, had fewer loans outstanding, and had significantly lower average outstanding

loan amounts. Total income and consumption, however, increased more for households that held on to their animals than for those who chose to sell them. The pattern is consistent with a subsample of households experiencing relatively small gains from the intervention, selling their livestock, and returning to wage labor. The high “drop-out” rate (as captured by the high rate of asset sales/loss after completion of the program) may thus contribute to the lack of measured impact in a statistical sense and also reflect relative program ineffectiveness in a real sense (i.e., “drop-out” can be seen as a form of non-compliance that weakens statistical power, and the drop-out can also reflect low impacts on a subsample exposed to the intervention.).

This third concern is consistent with the fourth. The study took place in a time when the control group (along with members of the treatment group if they chose to) could take advantage of a tight labor market. Just under half of the treatment households still had their livestock at the end of the study. For them, the intervention may have yielded gains, but the gains for the treatment group as a whole did not exceed the gains that the control group received from wage employment.<sup>3</sup> While we do not find net impacts on overall household income, consumption, asset accumulation, and use of financial services, we see shifts in the composition of time use and the composition of income for the treatment group (the shifts are away from wage labor and toward livestock-rearing, consistent with program participation). Treatment households, as a group, experienced income gains, but simultaneously faced an offsetting opportunity cost when participating. In other words, the intervention partly displaced wage employment.

The gains experienced by the control group are consistent with evidence on wage growth in the state. Between August 2007 and December 2011 real agricultural wages in Andhra Pradesh rose by 57% (Venkatesh, 2013), placing Andhra Pradesh as the Indian state with the fastest wage increases.<sup>4</sup> The control group in our study could thus advance quickly through wage labor, while the treatment group diversified into livestock-rearing. The mechanism parallels the findings of Crépon et al. (2014) in evaluating a microcredit program in Morocco, for example. Participants there increased their self-employment income (activities associated with microcredit) but decreased income from wage labor equivalently, leading to no measured net gain in total income or consumption for those with access to microcredit.

In sum: The SKS NGO ultra-poor program was imperfect but implemented largely as designed. Data problems remain a concern, especially for consumption, but the main results are robust to steps to address measurement problems. One important context for the study is that the market for wage labor was strong during the study period, allowing both the treatment and control groups to experience a steady increase in income between the baseline and endline surveys. The strong labor market meant that villagers had competing strategies for economic advancement during this period in Andhra Pradesh. They could join the ultra-poor program and take advantage of its promise to enable self-employment or they could intensify participation in the increasingly attractive wage labor market. Many households tried to do both (and experienced some displacement of one form of income gains by the other),

<sup>3</sup> On average, total income per capita increased by 65% in the treatment group between the 2007 baseline survey and the 2010 endline survey, but control group income increased by a similar amount: 57% (Table 2). In data in which the top 5% of observations are removed to limit the role of outliers, we find that, on average, total income per capita increased by 78% in the treatment group between the 2007 baseline survey and the 2010 endline survey, but control group income also increased by 78%. For specific sources of income, households that participated in the ultra-poor program increased monthly per capita income from livestock by 53 Rupees more than control households, but control group households increased monthly per capita income from agricultural wage labor by 51 Rupees more than the treatment group.

<sup>4</sup> Economists actively debate the role of government programs in contributing to wage growth (e.g., Imbert and Papp, 2015). Our interest, though, is not in the source of wage growth but on how wage-earning opportunities affected relative outcomes for households in the ultra-poor program.

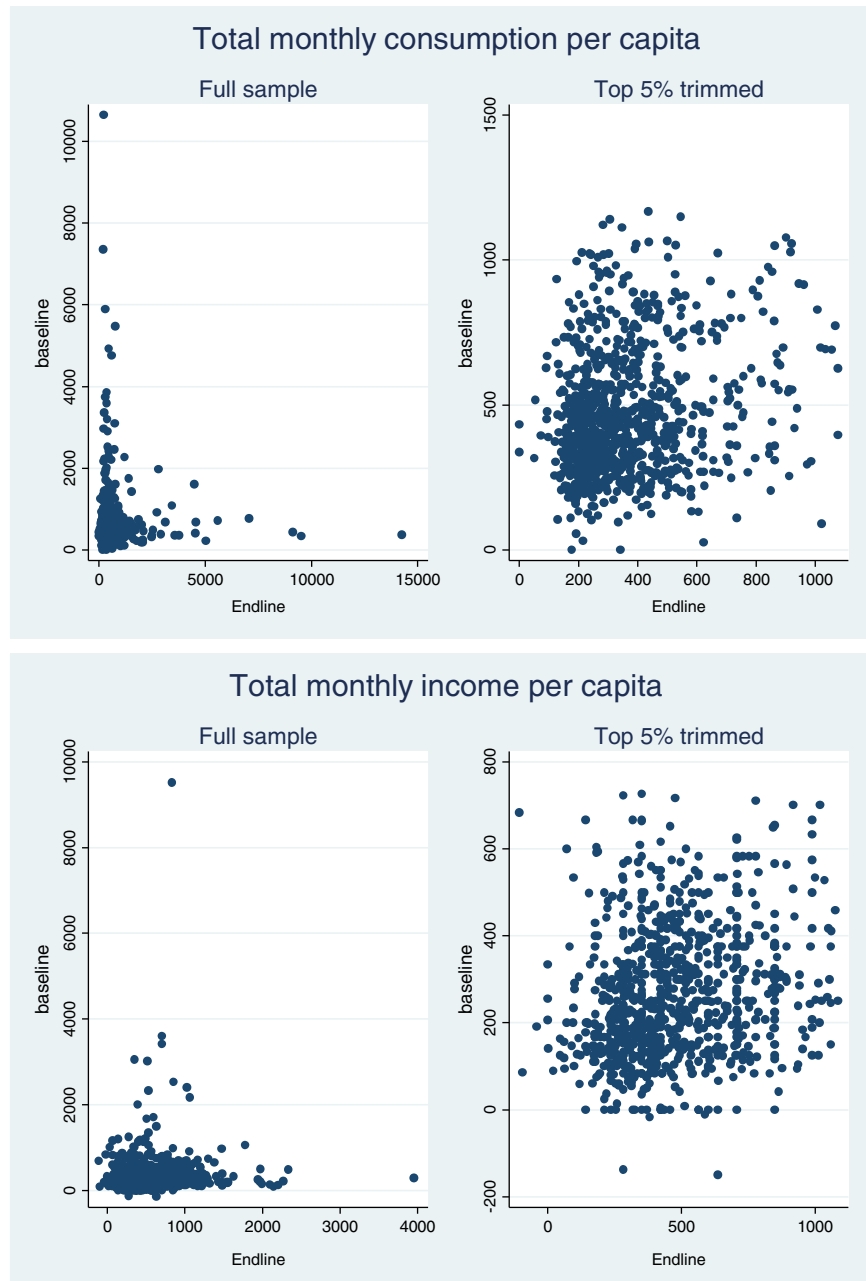


Fig. 1. Scatter plot of monthly per capita income and expenditures in the baseline survey versus the endline survey (full sample and sample with top 5% of values trimmed).

but the majority of treated households started with the ultra-poor program and later dropped out.

The results leave open the possibility that the same program could have led to positive net impacts in a different time or a different location with less possibility for displacement. In this way, the results complement the findings of [Karlan and Goldberg \(2014\)](#) and [Bandiera et al \(2013\)](#), and they show how the strength of alternative employment in the labor market can matter for the success and failure of this kind of employment-based intervention.

The remainder of the paper proceeds as follows. [Section 2](#) describes the intervention. [Section 3](#) describes the survey, sample, and data concerns. [Section 4](#) provides evidence that the intervention was implemented as designed. [Section 5](#) analyzes participation in the program and attrition. [Section 6](#) describes the evaluation methodology. [Section 7](#) gives results on asset accumulation and asset sales. [Section 8](#) turns to the main findings on income and consumption and the composition of earnings. [Section 9](#) gives evidence on saving, borrowing, and participation in

government programs. We conclude with thoughts on displacement and the context of impact results.

## 2. The intervention

The SKS Ultra Poor Program (UPP) aimed to establish micro-enterprises with regular cash flows, which would enable “ultra-poor” households to grow out of extreme poverty. In the early vision in Bangladesh, the hope was that households would then gain access to microfinance to keep expanding their self-employment activities ([Matin and Hulme, 2003](#)). In South India, though, most microlenders (apart from government-backed schemes) were pursuing better-off customers, so graduation into the broader microfinance sector was never a general aim. Instead, the focus was on reducing vulnerability and achieving greater economic independence for ultra-poor households. The formal “graduation” criteria included having children in school, being “food secure” for at least 30 days, creating an income-generating activity

beyond wage labor, and accumulating more than \$16 in savings (800 Rupees). Reflecting the program's holistic approach, households must also have gained knowledge about social and health issues, and become aware of available government programs.

The pilot program was implemented by Swayam Krishi Sangam (SKS NGO), an entity that grew out of the commercial lender, SKS Microfinance. The intervention was introduced in 198 villages of Medak District in the state of Andhra Pradesh, one of the 250 poorest districts in India. The program was later introduced in the state of Odisha.

The program as implemented by SKS was an 18-month intervention aimed at extremely poor households, identified through detailed participatory rural appraisals and village surveys. Households had to meet the five criteria to be eligible for the program: (i) not including a male working member, (ii) scoring less than a threshold number on a housing condition scorecard, (iii) owning less than one acre of land, (iv) not owning a productive asset, and (v) not receiving services from a microfinance institution. The housing condition scorecard took into account characteristics of the house such as its size, building material, and electricity and water access.

The program comprised four main components: 1) an economic package designed to provide self-employment and spur enterprise development, 2) essential health-care, 3) social development, and 4) financial literacy. The economic package for enterprise development involved a one-time asset transfer, enterprise-related training, cash stipend for large enterprise-related expenses, and the collection of minimum mandatory savings. It started with the selection of an income-generating activity by the household, from a menu of local activities such as animal rearing (mainly a buffalo or goats) or horticulture nursery. Non-farm activities, such as tea shops, tailoring, or telephone booths, were also available. Once the household had selected an activity, it underwent training sessions where one ultra-poor member, usually the woman head of household, was taught skills pertaining to the specific enterprise she had chosen and how to find additional help when needed (for example, veterinary care). After the training was completed, the specific asset or in-kind working capital was procured and transferred to the household. A mandatory weekly savings was required of all households, once the asset started to generate cash flow, such that households had to save at least \$16 by the end of the program in order to “graduate.”

A large majority of households in the SKS program chose to rear livestock as their enterprise: 55% of all households chose a buffalo, 31% chose goats, and 3% chose donkeys, pigs or sheep. The next most popular choice was non-farm business, an activity elected by 7% of households. Almost 3.5% of households used the program's grant to purchase land, earning an income from leasing it out for agricultural production. (The percentages are our calculations using the data described below.)

The second component of the program was the provision of essential primary health-care support. This was a combination of preventive training and techniques, and on-the-spot coverage. The health program was divided into: a) monthly visits by a field health assistant to each member, documenting the health status of the family and providing care or referrals as needed; b) a health screening and information awareness camp hosted with support from government doctors and health focused NGOs; c) monthly information sessions conducted by the health assistant on topics such as contraception, pre- and post-natal care, sanitation, immunization, tuberculosis and anemia; and d) training of one or two program members in each selected village on basic health services. These members were equipped with basic medicines (available free of cost from the government), taught to recommend a case to a doctor or hospital, and set-up as a health resource for other members.

The third component of the program was social development. It involved measures aimed at building social safety nets in the village, such as a solidarity group and a rice bank, and connecting participants

to existing public safety nets. Group solidarity was encouraged through weekly meetings where members discuss common concerns and solutions. A rice bank was created by members depositing a handful of rice every day, which could be drawn upon by member households at no interest.

The financial literacy component of this program involved basic training in budgeting exercise and setting financial goals. There was also an emphasis on accumulating savings and reducing reliance on moneylenders.

After 18 months, SKS stopped conducting the weekly meetings, collecting the weekly savings from members and organizing health camps in the treatment villages. The asset became the complete responsibility of the household with no enterprise-supporting stipend or advisory support from SKS. At the end of the program implementation, households were assessed on whether they met the formal criteria to “graduate” out of extreme poverty.

### 3. Surveys, sample and data quality

To analyze the intervention, we collected data from 3484 individuals, living in 1063 households across 198 villages in Medak District, in three waves of surveys between 2007 and 2010.

The baseline survey was conducted between August and October 2007. Data were collected on household monthly consumption expenditure, income and other financial transactions. Surveys also included questions on socio-demographic characteristics of the households (religion, caste, family type, size of household, age, marital status, disability, education, occupation, and migration details), living conditions (characteristics of the house, source of drinking water, sanitation and source of fuel), participation in government schemes (employment, pension, housing, training, credit and subsidized basic goods), asset ownership, use of time, women's social status and mobility, political awareness and access, social standing of the household within the community and future aspirations of the household members. Health information collected included data on physical health, hygiene habits and mental health conditions of household members.

Following the baseline survey, 102 villages were randomly assigned to the treatment group and 95 to the control group. The 102 treatment villages included 575 households (54% of the total sample) who were offered the treatment.<sup>5</sup>

A midline survey was conducted between April and September 2009, immediately at the end of SKS NGO's presence in the villages and about 18 months after treatment households received their asset. Since the enterprise training and subsequent asset transfer took almost six months to implement, the midline survey was conducted over a longer period than the other two survey waves. As a result, the effect of the seasonality of economic activities, particularly present in the agricultural communities where the program was implemented, influences the measurement of important outcomes in the midline survey. Because the impacts of interest are the program's long-term impacts, and to compare outcomes measured at similar periods of the year, we focus analyses on baseline and endline surveys.

The endline survey was conducted roughly three years after the baseline, in October and November 2010. In the endline wave, we were able to reach 1011 of the baseline households. The endline survey included the same questions as the baseline survey, with the addition of a section that collected detailed information on participation in the National Rural Employment Guarantee (NREG) scheme, including number of household members working in the scheme, number of days worked, and payment received for work in the scheme. Another additional section collected height and weight data for children under 10 years of age living in the household.

<sup>5</sup> With an average of just 5.6 households per village participating in the treatment (and village sizes generally between 500 and 5000 people), general equilibrium effects are unlikely although we cannot rule them out.



### 3.1. Data quality

Our data exhibit two main data issues: (1) low correlation across waves and across variables due to outliers, and (2) implausibly large consumption levels in the baseline survey. Table 1 presents a simple diagnostic of data quality.<sup>6</sup> The top-right cell shows that the correlation between income measured in the baseline and income in the endline is 9%. There is no clear benchmark for the “correct” correlation, and the correlation is likely reduced by the fast economic change experienced in the region, but the figure is too low to be taken at face value. The top-left panel of Fig. 1 presents the same data as a scatter plot. The figure points to the existence of large outliers, both in the endline and the baseline.

The second row of Table 1 shows that the correlations between consumption levels in different waves are even lower (2% correlation between baseline and endline), while the asset data is relatively highly correlated (38% correlation between baseline and endline). The bottom left panel of Fig. 1 also shows large outliers in the consumption data.

We investigated sources of the outliers, and while we could rule out problems with a particular field worker, data entry error, and decimal place mistakes, we were unable to pin down the exact source of the errors. Even without explaining the outliers, we can see their influence. Rows 4 through 6 of Table 1 show that when the largest 5% of observations are Winsorized, the income and consumption correlations increase substantially. The baseline-to-endline correlation from income increases from 9 to 17%. For consumption, it jumps from 2 to 18%, and for assets (where outliers were not notable) the correlation moves little, rising slightly from 38 to 40%. The right-hand-side figures in Fig. 1 show the scatter plots of the trimmed data, showing clearer correlations. To address the concern with outliers, the analyses of impacts of the program on income and consumption were completed for both the original data (in the text) and Winsorized data (in the online Appendix).

The lower half of Table 1 shows correlations between variables in a given round of the survey. In the baseline, the correlation between income and consumption is 14%, but rises to 28% after Winsorizing the largest 5% of observations. The increased correlation in the endline is from 16% before Winsorizing to 24% afterward. The correlations with assets are generally low, consistent with there being an overall low asset-base before the program and asset depletion after the program. (Appendix Table 3 gives parallel results when the data are trimmed at the top 5% of observations or trimmed at both the top 5% and bottom 5%.)

The main interest when evaluating the program is in changes between baseline conditions and endline conditions. For consumption, however, we also give results for the cross-sectional consumption data in the endline. This is a response to evidence of systematic measurement error in the baseline consumption data. The summary statistics in Table 2 document the reason for concern. First, baseline monthly household consumption per capita is implausibly larger than baseline income data. The control group earned an average of 331 Rupees per person per month but is measured as having spent 587 Rupees per person per month; the treatment group earned on average 312 Rupees per person per month but is measured as having spent 542 Rupees per person per month. In contrast, the income and consumption data are within 10% of each other in the endline survey. Second, the average monthly per capita consumption expenditure (Rs. 587 per person per month, or about US\$1.18 per day in PPP conversion) is implausibly higher in the baseline sample than the rural poverty line of Rs. 448 per person per month (Tendulkar et al., 2009). The endline consumption data, in contrast, are consistent with the poverty line for the district: By the time of the endline (2009–10), the local poverty line is 512 Rupees, and measured consumption in the treatment

**Table 1**

Correlations between income, consumption, and assets (percent).

	Baseline to midline	Midline to endline	Baseline to endline
<i>Full sample</i>			
Income	12	18	9
Consumption	1	0	2
Assets	40	43	38
<i>5% Winsorized</i>			
Income	23	22	17
Consumption	8	9	18
Assets	38	38	40
	Income–consumption	Income–assets	Consumption–assets
<i>Full sample</i>			
Baseline	14	10	4
Midline	17	16	5
Endline	16	–4	8
<i>5% Winsorized</i>			
Baseline	28	13	3
Midline	26	16	2
Endline	24	–5	9

Notes: The top six rows give the correlation between data on the variable in one wave of the survey with the same variable in a later wave. The bottom six rows give the correlation between different variables within the same wave. The (full) sample was obtained after establishing that households were properly matched by household name, ID, and demographic data. The “5% Winsorized” sample was obtained after Winsorizing at the top 5%.

group is 496 Rupees per person per month. Third, measured average food expenditures drop by half between the baseline and endline surveys (Table 2), which is not consistent with household reports of improvements in food security as measured by whether any household member skipped meals, whether adults ever go entire days without eating, or whether all household members had enough food all day, every day (Appendix Table 4). Fourth, the measured consumption decline is not consistent with rising income as seen in Table 2 (and seen in the region generally).

For completeness, we present baseline/endline analyses of the impact of the program on consumption expenditures despite the evidence of measurement error in the baseline (Table 10). Results are similar, though, whether we include the baseline in the analysis or not, and whether we include binary variables to control for month of year in which the survey was conducted (Appendix Table 5).<sup>7</sup>

### 4. Implementation

An analysis of overall program costs suggests that the program was as input-intensive as two similar ultra-poor graduation pilots in India. On average, the SKS direct program costs were \$343.53 for each participant (Table 3), the largest part of which was the costs of the asset and a stipend given to help households meet enterprise-related expenses (\$195.61 per participant).<sup>8</sup> Direct costs also included a share of staff salaries, training, travel, office costs, and other materials and equipment. The total cost of the program (\$571.11 per participant) included other uses of staff salaries, travel, and head-office management costs; these non-direct expenses cover the start-up phase through to the implementation of the program.

<sup>7</sup> We attempted to detect the source of the measurement error, but the source remains unclear. The same survey firm completed all waves of the survey using the same survey instrument but with different survey teams. Data quality issues are not attributable to specific surveyors. The survey firm had no role in implementing the intervention itself.

<sup>8</sup> The cost data were assembled by M-CRIL, an independent microfinance research organization based in Gurgaon, India, based on self-reports from the organizations. Salaries and other expenses were allocated by activity. Start-up expenses include branch setup and initial survey, staff salaries, staff training, and other expenses. Direct expenses are detailed in Table 1. Auxiliary expenses include staff salaries, staff training, travel, material expenses, stationery and course materials, and other expenses. Indirect expenses include head office management costs (salaries and travel), and other expenses. Data are from Table 1, Sinha and Roy (2013).

<sup>6</sup> We thank Dean Karlan and Josh Dean for bringing the data problems in Table 2 to our attention and for providing comparative data with the other ultra-poor graduation pilots.

**Table 2**  
Summary statistics for the control and treatment households.

	Baseline			Endline			Percent change baseline–endline	
	N	Control	Treatment	N	Control	Treatment	Control	Treatment
Total income	1063	331	312	1030	518	515	57	65
Income from livestock	1055	2.4	3.6	1017	7.6	62.1	221	1644
Income from agriculture labor	1059	174	176	1025	316	266	82	51
Income from non-agriculture labor	1047	60	55	983	105	103	75	86
Total income, top 5% Winsorized	1063	290	283	1030	500	493	72	74
Income from livestock, top 5% Winsorized	1055	−0.2	−0.2	1017	2.2	29.9	–	–
Income from agriculture labor, top 5% Winsorized	1059	163	159	1025	297	258	83	62
Income from non-agriculture labor, top 5% Winsorized	1047	53	48	983	97	96	84	98
Total expenditures	1063	587	542	1030	496	470	−15	−13
Food expenditures	1063	277	277	1030	142	139	−49	−50
Non-food expenditures	1063	310	265	1030	355	332	15	25
Total expenditures, top 5% Winsorized	1063	495	499	1030	412	387	−17	−22
Food expenditures, top 5% Winsorized	1063	271	269	1030	135	131	−50	−51
Non-food expenditures, top 5% Winsorized	1063	215	221	1030	271	249	26	13
Household has savings (%)	1063	51	59	1030	60	65	18	9
Per capita savings balance	697	110	140	714	292	294	165	110
Household saves in SHG (%)	1062	47	57	1025	58	55	22	−4
Household has outstanding loan (%)	1063	68	74	1030	47	49	−32	−34
Per capita outstanding loan balance	1063	2479	2921	1030	1447	1534	−42	−47
Household borrows from moneylender (%)	1062	28	31	1024	8	9	−72	−71
Household borrows from SHG (%)	1062	30	40	1024	30	33	1	−16
Household sought/received government assets (%)	1063	3.3	4.3	1029	9.9	9.4	203	115
Household sought/received government training (%)	1063	0.4	0.5	1028	7.6	6.5	1761	1141
Household received goods with PDS card (%)	1062	93	93	1030	98	98	5	6
Household has a BPL card (%)	1051	91	92	1016	96	98	5	6
Household sought/received NREG work (%)	1061	31	37	1028	82	80	167	115
Number of days household worked in NREG		n/a	n/a	57	32	35	n/a	n/a
Monthly per capita income from NREG		n/a	n/a	366	72	75	n/a	n/a
Household owns any animal(s) (%)	1055	7	13	1006	6	32	−22	149

Notes: All data are averages, except in the last two columns. All amounts are in Rupees of 2007. The percentage change displayed in the last two columns may be different from the percentage change calculated from the data displayed in the table because of rounding. “N” is the number of observations. Income and expenditures are monthly per capita values. Income is negative when the costs of that occupation are larger than its revenues. Savings in and borrowing from specific institutions are not conditional on the household having savings/borrowings. PDS and BPL cards entitle holders to rationed goods through government schemes providing basic goods at subsidized prices to poor households. The number of days worked in NREG and income from NREG are conditional on participating in the NREG scheme, and include many missing values.

Table 3 gives a comparison of the costs incurred by SKS NGO alongside the costs of the two other ultra-poor graduation programs in India, both in West Bengal. One was implemented by Bandhan (its impacts are the focus of Banerjee et al., 2011 and Duflo, 2012) and the other by Trickle Up. The side-by-side comparison shows that the direct inputs were most expensive at SKS NGO (\$344 versus \$221 at Bandhan and \$332 at Trickle Up). This difference aligns with

differences in asset costs (\$183 at SKS versus \$101 at Bandhan and \$170 at Trickle Up).

In contrast, the direct stipend payment to households was lowest at SKS NGO (\$12 versus \$65 at Bandhan and \$50 at Trickle Up). At SKS NGO, the stipend was designed to cover enterprise-related costs and was delivered “as needed” over the 18 month period of the intervention. (The stipend was designed to be as large as \$18, but \$12 was delivered

**Table 3**  
Expenses per client (US\$).  
Pilot programs of three ultra-poor graduation programs in India.  
Notes: Data are from Table 1, Sinha and Roy (2013).

	SKS (NGO)	Bandhan	Trickle-UP
Location and state	Narankhed (Medak District), Andhra Pradesh	Murshidabad, West Bengal	South 24 Parganas, West Bengal
Number of clients	426	300	300
Number of field workers	9	6	6
Start-up expenses	46.10	27.15	22.35
Direct expenses	343.53	220.54	331.75
Auxiliary expenses	143.65	46.09	38.84
Indirect expenses	37.83	27.25	280.82
Total expenses	571.11	331.03	673.77
<i>Direct expenses (detail)</i>			
Asset cost	183.19	101.30	169.84
Food subsidy/stipend	12.42	64.89	49.70
Veterinary services/sector specialist	–	–	20.12
Staff salaries	77.64	28.77	43.19
Staff training	14.72	–	3.06
Beneficiaries' training	18.99	4.59	–
Travel	11.65	2.79	9.20
Office rent and utilities	24.78	17.95	33.33
Material expenses	0.23	0.25	1.15
Other equipment	–	–	2.17
Total direct expenses	343.53	220.54	331.75

**Table 4**  
Tests of randomization balance.

	Control group		Treatment group		p-Value	Regression
	Mean	N	Mean	N		
Household size	3.2	488	3.3	575	0.156	0.016 (0.012)
Average age of household members	28.9	488	30.2	575	0.107	0.003** (0.001)
Household owns home	72.4	486	70.0	573	0.382	−0.039 (0.039)
Household includes any migrant worker	16.7	460	15.0	528	0.445	−0.034 (0.053)
Land ownership (acres)	0.39	476	0.43	558	0.548	−0.010 (0.018)
Monthly household income per capita	331	488	312	575	0.474	−0.000 (0.000)
Monthly household expenditures per capita	587	488	542	575	0.241	−0.000 (0.000)
Household has any outstanding loan	68.4	488	73.6	575	0.066*	0.027 (0.041)
Household has any savings	51.0	488	59.3	575	0.007***	0.030 (0.040)
Household works in an employment-generating scheme	30.5	488	37.3	573	0.020**	0.057 (0.043)
Household receives a pension	60.5	488	68.1	574	0.009***	0.046 (0.043)
Household owns animal(s)	7.2	486	12.8	569	0.003***	0.169*** (0.062)
Constant						0.244* (0.147)
Observations						951
R-squared						0.104
F-test of all variables in the regression						2.25
p-Value of F-test						0.011

The sample includes all baseline households; the number of observations is lower than 1063 because of missing values. The p-values are from t-tests of the differences in means between the treatment and control groups. Regression estimates are from a linear probability model where the dependent variable is 1 if the household was assigned to the treatment group (defined as intent-to-treat) and 0 if the household was assigned to the control group. Standard errors in parentheses are clustered at the village level. Stratification variables used in the randomization (total number of households in the village, distance from village to the nearest metallic road, and percentage of poor households in the village) are included in the regression but not shown.

\*\*\* p < 0.01.

\*\* p < 0.05.

\* p < 0.1.

**Table 5**  
Correlates of participation in the program.

Household size	0.035* (0.019)
# adults (age 14 +)	−0.057** (0.027)
1 if anyone in hh migrates for work	−0.110* (0.060)
1 if owns house	−0.014 (0.048)
# acres land owned by hh	0.011 (0.025)
1 if hh owns livestock	−0.173** (0.082)
1 if hh owns poultry	−0.015 (0.093)
1 if hh owns plow	0.116 (0.176)
Constant	0.749*** (0.055)
Observations	507
R-squared	0.031
Mean and (std. dev.) of dep. var. at baseline	320 (420)

Notes: Standard errors are clustered at the village level. The dependent variable is a binary variable equal to 1 if the household participated in the project, 0 if the household refused the (randomized) offer to participate, and missing if the household was assigned to the control group.

\*\*\* p < 0.01.

\*\* p < 0.05.

\* p < 0.1.

on average.) At the other programs, the stipend was intended to cover food expenses for up to 10 months (Bandhan) or 6 months (Trickle Up). The ultra-poor graduation pilots outside of India described by [Karlan and Goldberg \(2014\)](#) also feature a consumption stipend for 6–12 months.

The ultra-poor graduation pilots are thus similar in design and aspiration, but not identical (with the lack of a consumption stipend at SKS being the most notable design difference). While we cannot rule out that differences in program impacts in the SKS and Bandhan studies are due to the lack of a consumption stipend, we note that the data overall show that the SKS program was generous at a scale as great, or greater, than the other programs.

Was the SKS intervention implemented as designed? SKS NGO implemented a Client Monitoring System to track the progress of program participants throughout the 18 months of the program. (No data were collected on households in villages assigned to the control group in the randomized experiment.) The system was developed by BRAC Development Institute, a research arm of the NGO BRAC in Bangladesh, and it drew on BRAC's own ultra-poor graduation program. Three rounds of data were collected during the implementation of the program (September 2008, January 2009 and June 2009), and an additional round was collected six months after the end of implementation, in January 2010. The Client Monitoring System relied on SKS NGO program officers electronically collecting data on the participants that they managed, and covered a wide range of indicators such as asset ownership, savings behavior, amount and use of stipends, other sources of income, illnesses, and food security.

**Table 6**  
Impact of the ultra-poor program on asset ownership.

	Household owns its house?	Acres of land owned	Non-ag. assets index	Ag. assets index	Household owns livestock?	Household owns poultry?	Household owns plow?
Treatment	−0.02 (0.02)	−0.14 (0.08)	0.04 (0.11)	0.35*** (0.11)	0.26*** (0.03)	0.02* (0.01)	−0.00 (0.01)
Baseline dep. var.	0.32** (0.03)	0.26*** (0.07)	0.33*** (0.05)	0.05 (0.05)	0.02 (0.06)	0.12** (0.06)	−0.01** (0.00)
Constant	0.59*** (0.05)	0.24* (0.14)	0.02 (0.27)	−0.13 (0.19)	0.09 (0.06)	0.01 (0.04)	0.02* (0.01)
Observations	1004	963	996	978	996	980	1000
R-squared	0.17	0.05	0.15	0.04	0.13	0.03	0.00
Mean and (std. dev.) of dep. var. at baseline	0.711 (.453)	0.413 (.969)	<0.001 (1.691)	0.007 (1.328)	0.069 (.254)	0.050 (.218)	0.012 (.110)

Notes: Standard errors are clustered at the village level. Regressions in which the dependent variable is a binary variable are run as linear probability models. The treatment is the offer to participate in the program (intent-to-treat estimate). Stratification variables used in the randomization (total number of households in the village, distance from village to the nearest metallic road, and percentage of poor households in the village) are included in the regressions but not shown. The non-ag. assets index is the principal components index of non-agricultural household durable goods owned by the household (e.g. television, table, jewelry). The agricultural assets index is the principal components index of household agricultural durable goods and animals owned by the household (e.g. plow, tractor, pump, livestock).

\*\*\* p < 0.01.

\*\* p < 0.05.

\* p < 0.1.

**Table 7**  
Characteristics of treatment households, by animal ownership status in endline survey.

	Did not own animal in endline	Owned animal(s) in endline	p-Value
<i>Panel A. Baseline characteristics</i>			
Household size	3.2	3.6	0.009
Average age of household members	30.4	29.6	0.527
Acres of land owned	0.38	0.56	0.044
Total monthly income per capita (Rs)	330	297	0.295
Owned any animal (percent)	12	16	0.273
Household has any loan outstanding (percent)	73	77	0.222
Number of loans outstanding	1.0	1.1	0.311
Amount of loans outstanding (Rs)	8574	9204	0.627
<i>Panel B. Endline characteristics</i>			
Household sold animal in last 12 months (percent)	1.3	16.2	<0.001
Monthly income from sales of animals (Rs)	3.6	34.7	<0.001
Total monthly income per capita (Rs)	487	578	0.004
Monthly agriculture labor income per capita (Rs)	272	253	0.357
Monthly livestock income per capita (Rs)	20	160	<0.001
Household had unexpected event in last year (percent)	7	18	<0.001
If event: total cost of event(s) (Rs)	31,351	41,099	0.497
Household has any loan outstanding (percent)	42	61	<0.001
Number of loans outstanding	0.48	0.79	<0.001
Amount of loans outstanding (Rs)	2807	5473	<0.001

Notes: Sample is constituted of treatment households only. Data are averages. The p-values are from t-tests of the difference between the means. All amounts are in Rupees of 2007.

The Client Monitoring System reports confirm that participating households received the assets and services as promised, started new livelihoods and generated income from it, and proceeded toward meeting the goal of “graduation.” According to the Client Monitoring System, 97% of participants reached that goal.

## 5. Participation and attrition

Of the 575 household assigned to the treatment group, 70% (404 households) participated. The other 30% (171 households) is counted as part of the treatment group in the analysis (yielding intent-to-treat estimates). The most common reasons for not participating in the program were “not interested in taking asset” (52%), migration (33%) and having access to microfinance loans (11%).<sup>9</sup> “Microfinance” loans do not include loans from self-help groups; almost

50% of households which reported having outstanding loans in the baseline had one or more loans from self-help groups. Non-participants also include 19 households (3%) which were deemed ineligible by SKS post-targeting because they had existing access to microfinance products.

The non-participation by 30% of the initially targeted treatment households is in line with the evidence from the study of the Bandhan ultra-poor program in West Bengal. There, 35.6% of those offered decided not to join the program, and an additional 12.5% were found ineligible after they were randomly assigned to the treatment group (Banerjee et al., 2011, p. 8).

In other ways, the population served by the SKS NGO intervention differs substantially from that served in West Bengal. Both interventions aimed to foster non-agricultural self-employment, but that had been only a small factor for poor households in the SKS study area. Before the intervention, only 4% of the total household income in the sample was generated from non-agricultural self-employment, and 1% was generated from livestock. Most earning instead came from wage labor: at baseline, 56% of the total income of the treatment group came from agricultural wage labor and 18% from non-agricultural wage labor. For the control group, the percentages are similar: 53 and

<sup>9</sup> Subsequent interviews with some of the households that refused to take part in the program revealed that “not interested” could imply a lack of entrepreneurial ability or self-confidence, or simply having access to higher wages as construction workers in the nearby township. Seasonal migration for work is a common feature of the labor market in Medak district.



**Table 8**  
Impact of the ultra-poor program on income.

	Total	Ag. self-empl.	Ag. labor	Non-ag. labor	Salaried empl.	Livestock	Non-ag. self-empl.	Other sources
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	−0.06* (22.60)	5.03 (5.19)	−50.06** (19.62)	0.39 (15.05)	−6.63 (6.49)	56.24*** (13.76)	1.26 (0.83)	−3.94 (5.70)
Baseline dep. var.	0.08*** (0.02)	0.06 (0.06)	0.15** (0.07)	0.16** (0.06)	0.09 (0.07)	0.11 (0.18)	0.00 (0.00)	0.01 (0.01)
Constant	495.14*** (38.90)	12.01 (8.40)	274.39*** (29.77)	111.55*** (24.71)	28.43** (11.75)	2.40 (17.33)	1.32 (0.99)	70.70*** (17.43)
Observations	1009	1009	1000	946	994	988	976	792
R-squared	0.01	0.01	0.03	0.01	0.01	0.02	0.00	0.00
Mean and (std. dev.) of dep. var. at baseline	321 (420)	13 (59)	175 (181)	57 (106)	7 (49)	3 (24)	37 (327)	38 (164)

Notes: Standard errors are clustered at the village level. The treatment is the offer to participate in the program (intent-to-treat estimate). Stratification variables used in the randomization (total number of households in the village, distance from village to the nearest metallic road, and percentage of poor households in the village) are included in the regressions but not shown. The monthly per capita income from each source is in 2007 Rupees; 1 USD ≈ 40 Rs. Livestock income includes income from irregular sales of animals. Other sources of income include land sales, rental, government assistance, remittances, pensions and other unclassified sources.

\*\*\* p < 0.01.

\*\* p < 0.05.

\* p < 0.1.

18%, respectively. Winsorized summary statistics in Table 2 yield very similar percentages.

These percentages contrast with baseline conditions for the ultra-poor intervention in West Bengal evaluated by Banerjee et al. (2011). Panel B of their Table 4 shows that non-agricultural self-employment was already the main income source for 32% of West Bengal treatment households, and wage labor was the main source for 51% in the West Bengal sample (versus 83% in our Andhra Pradesh sample). In short, in West Bengal, non-agricultural self-employment was already an integral part of the economic environment, but in Andhra Pradesh it was virtually non-existent and the greatest focus was on wage labor.

The role of labor market opportunities can be seen in a regression analysis. Table 5 gives the results of a linear probability model where the dependent variable is an indicator for participation, estimated on a sample restricted to eligible households. The results show that households with more working-age adults (ages 14 and above) are 6 percentage points less likely to participate, and households with migrants are 11 percentage points less likely. The results suggest that these households opted out from the start if they were better-positioned for (or more committed to) the labor market. Already owning livestock reduces participation by a further 17 percentage points.

Who is in the treatment group? Table 2 reports the mean of key indicators in baseline and endline survey waves, by treatment assignment. Households were ineligible for the program if they owned

goats, buffaloes or a large flock of chicken, but households could own a few small animals and still be eligible. As a result, about 7% of control households and 13% of treatment households owned an animal. The difference is statistically significant.

In keeping with the focus on the very poor, the average monthly per capita income in the baseline survey, including the value of household-produced consumption items, was slightly above 300 Rupees, equivalent to about 0.60 US dollars per day in purchasing power parity (PPP) terms. In comparison, the Tendulkar Committee Report of the Government of India estimates a rural poverty line at Rs. 448 per person per month or about US\$0.90 per day in PPP conversion (Tendulkar et al., 2009).

Participation in government safety nets was heterogeneous in the baseline survey, and remained so throughout the years in which we collected data. On one hand, government programs distributing subsidized foods and basic necessities were used by more than 90% of all households. On the other hand, fewer than 5% of households in the baseline survey reported seeking or receiving assets, vocational training or subsidized loans from the government. Participation in the National Rural Employment Guarantee scheme was relatively low at the time of the baseline (34% of all households participated), but increased sharply from 2007 to 2010. By the endline, 80% or more of both treatment and control households worked in the scheme.

The baseline survey indicates that many women had an active, mostly informal, financial life. At baseline, before receiving any service from

**Table 9**  
Impact of the ultra-poor program on time use of adults and children.

	Productive time	Leisure time	Time doing chores	Selected sub-categories of productive time		
				Agricultural labor	Tending animals	Tending animals, if owns animals
Treatment	−10.59 (10.77)	0.19 (2.80)	9.54 (10.42)	−41.88*** (15.82)	17.69*** (3.43)	24.34 (18.71)
Baseline dep. var.	0.02* (0.02)	0.02 (0.03)	0.08** (0.03)	0.09*** (0.03)	0.09 (0.08)	0.16 (0.13)
Constant	373.00*** (20.06)	15.85*** (5.68)	169.32*** (16.65)	277.49*** (28.08)	2.35 (6.23)	−2.27 (25.76)
Observations	1009	1009	1009	984	1002	194
R-squared	0.01	0.01	0.01	0.03	0.03	0.03
Mean and (std. dev.) of dep. var. at baseline	323 (239)	27 (67)	226 (114)	266 (230)	7 (49)	52 (126)

Notes: Standard errors are clustered at the village level. The treatment is the offer to participate in the program (intent-to-treat estimate). Stratification variables used in the randomization (total number of households in the village, distance from village to the nearest metallic road, and percentage of poor households in the village) are included in the regressions but not shown. Number of households owning animals: baseline = 108, endline = 199. Time is measured in minutes in the last 24 h. Productive time includes working in the field, tending animals, working in business, agricultural labor, working in someone else's house, non-agricultural labor and doing other work. Leisure time includes shopping, watching TV/listening to radio and doing political activities. Time doing chores includes gathering water and fuel, cooking, cleaning home and clothes and caring for children/elderly.

\*\*\* p < 0.01.

\*\* p < 0.05.

\* p < 0.1.

**Table 10**  
Impact of the ultra-poor program on expenditures.

	Total	Food	Non-food	Non-food details				
				Energy	Tobacco/alcohol	Medical	Education	Other
Treatment	−34.57 (53.03)	−3.04 (6.69)	−32.23 (52.06)	−2.60 (3.83)	−1.26 (1.57)	−3.92 (6.84)	−5.83 (4.31)	−19.51 (49.95)
Baseline dep. var.	0.03* (0.03)	0.10*** (0.02)	0.01 (0.02)	0.00 (0.00)	0.00 (0.00)	0.02 (0.02)	0.07 (0.05)	−0.00 (0.03)
Constant	300.94** (130.58)	116.80*** (11.20)	171.77 (132.89)	24.93*** (5.61)	8.41** (3.41)	38.24*** (9.03)	15.67*** (3.89)	85.99 (132.95)
Observations	1009	1009	1009	1009	1009	1009	1009	1009
R-squared	0.01	0.02	0.01	0.00	0.01	0.01	0.01	0.01
Mean and (std. dev.) of dep. var. at baseline	563 (626)	277 (151)	286 (583)	24 (263)	19 (135)	54 (170)	13 (39)	176 (470)

Notes: Standard errors are clustered at the village level. The treatment is the offer to participate in the program (intent-to-treat estimate). Stratification variables used in the randomization (total number of households in the village, distance from village to the nearest metallic road, and percentage of poor households in the village) are included in the regressions but not shown. The monthly per capita expenditures are in 2007 Rupees; 1 USD  $\approx$  40 Rs. Energy expenditures includes expenditures on electricity, other forms of energy (e.g., kerosene for lamps), and own vehicle fuel. Other expenditures include general household expenditures (household products, personal care products, clothing, phone, rent, utilities), transportation, entertainment, ceremonial expenditures, and unspecified expenditures.

\*\*\*  $p < 0.01$ .

\*\*  $p < 0.05$ .

\*  $p < 0.1$ .

SKS, more than 50% of all households saved and almost three quarters of them had outstanding loans. Average total outstanding loan balances represented eight to 10 times the average per capita monthly income.<sup>10</sup>

Overall, these baseline descriptive statistics highlight that households eligible for the ultra-poor program were very poor by income measures. They were reliant on income from day labor working for local farmers and on government-subsidized basic goods markets. Despite some animal ownership, these households did not own other productive assets. They were active in informal financial markets, and had a relatively high level of debt at the baseline.

### 5.1. Attrition

The rate of attrition between baseline and endline surveys was 5%. We compare in Appendix Table 6 the means of characteristics of households that were successfully reached in the endline survey versus those that were not. The households that were not included in the endline survey have an older and more literate head, but there are no significant differences in family size, income, expenditure, asset ownership, use of financial services, or participation in government schemes.

We tested whether attrition was different for the treatment and control groups by regressing an indicator variable equal to one if the household was an attriter and zero otherwise on a treatment indicator, a series of household characteristics, and the interaction of the treatment dummy with each of the characteristics (Appendix Table 7). An F-test of the joint significance of the interactions shows that being assigned to the treatment group does not significantly predict long-run attrition ( $F = 0.71$ ,  $p$ -value = 0.744).

## 6. Experimental design and empirical strategy

The evaluation of the program is conducted through a randomized controlled experiment, where the level of randomization is the village.

<sup>10</sup> This is notable in the context of the microfinance crisis in Andhra Pradesh: these households did not participate in formal microfinance (other than self-help groups), yet were already over-indebted. Jawahar and Sengupta (2012) use interviews and focus groups to assess decisions to participate in the intervention. They highlight fear of credit as a reason that some eligible women refused to participate. They report on confusion between the for-profit microfinance institution (SKS Microfinance) and the NGO arm, causing women to falsely assume that the intervention focused on providing loans. One senior staff member of the NGO remarked that “Those who were willing to join the program were generally better off than those who did not. I would say around 75% of the members that refused to join were the worst off, who refused simply out of fear.” (Jawahar and Sengupta, p. 19). On the other hand, they note that some women refused to join because they felt they had sufficient income or family support.

The assignment was stratified by village population, number of ultra-poor households as a proportion to total village population, and distance from the nearest metallic road.

We randomized at the village level due to (i) ease of program implementation and group intervention on the part of SKS, (ii) ease in ensuring that villages were treated according to the initial random assignment (relative to monitoring the treatment of individual households), and (iii) minimization of spillovers from treatment to control households.

The experimental design took into account the possibility that the error term may not be independent across individuals. Since treatment status across individuals within a group is identical and outcomes may be correlated, a larger sample size (relative to the individual-level randomization) was required to tease out the impact of the program. Power calculations assumed a relatively high level of intra-village correlation ( $\rho = 0.30$ ).

The analytical strategy draws on a series of reduced-form regressions. The difference in the means of the treatment and control groups is the OLS coefficient  $\beta$  in the following equation:

$$Y_{ijt} = \alpha + \beta T_{ij} + \delta Y_{ijt-1} + \gamma X_{ijt-1} + \varepsilon_{ijt} \quad (1)$$

where  $i$  indexes households and  $j$  indexes villages. The subscript  $t$  indicates the endline and  $t - 1$  is the baseline.  $Y$  is the outcome of interest (consumption, income, etc.).  $T$  is an indicator variable that equals 1 if a household lives in a treatment village and 0 otherwise, and  $\beta$  is the impact of the treatment.  $\varepsilon_{ij}$  is the unexplained variance at the household level. Since the treatment was random across villages,  $\varepsilon_{ij}$  is uncorrelated with  $T$ . The coefficient of interest  $\beta$  is the intent-to-treat estimate (the expected change in the outcome for a household that was offered the treatment).  $X_{ijt-1}$  includes the variables used to stratify the randomization.

While randomizing participants into the treatment and control groups produces similar groups in expectation, this outcome is not guaranteed in practice and was not achieved in our evaluation. The unit of randomization was the village, and household-level data show some statistically significant baseline differences between households in the treatment and control villages. In Table 4 we consider 12 key variables, and find two dimensions for which treatment and control households differ significantly at baseline: the average age of household members (28.7 years in the control group and 30 years in the treatment group) and the ownership of animal(s) (7% of control households, versus 13% of treatment household owning one or more heads of livestock or poultry).

To test robustness, we also run regressions that include in  $X_{ijt-1}$  the two baseline variables that systematically differ between the treatment and control households. These regressions are included in Appendix Tables 8 to 13; the results are similar to those of the main specification.

## 7. Asset accumulation, asset sales, and program dropout

We start with impacts on asset accumulation, the most immediate expected impact of the program. The ultra-poor program was designed to help households accumulate assets in at least two ways. First, the program had a direct impact on agricultural or enterprise asset ownership by transferring an animal or by providing working capital for a non-farm microenterprise. Second, the program helped indirectly by improving financial tools and income.

The first four columns of Table 6 analyze the impact of the program on the ownership of housing, land, consumer durables, livestock, and other assets. The non-agricultural assets index is the principal components index of household durable goods owned by the household (such as a television, tables, or jewelry). The agricultural assets index is the principal components index of household agricultural durable goods (such as a plow, tractor, or pump) and animals owned by the household.

We find an increase in animal ownership among treatment households. Column 5 of Table 6 provides regression estimates of these changes: being assigned to participate in the program led to a 26-percentage point increase in the likelihood of owning livestock, which includes animals such as buffaloes and goats that were provided by the program. In line with this, Table 2 shows that the percentage of households reporting owning an animal increased between baseline and endline surveys for treatment households, but not for control households. As a further check, we note that ownership of poultry did not increase (column 6 of Table 6), which is consistent with the fact that chicken and ducks were not available as grants from the program. Nor is there an impact of the program on the ownership of consumer durables, land, or housing, as seen in the non-agricultural asset index in column 4. The final column also shows no impact on farm equipment. The only effect is on livestock holdings, which is a direct effect of the program.<sup>11</sup>

The increase in animal ownership seen in Table 6 is strong, but not as large as expected. By the time of the endline, many households had sold or lost their livestock.<sup>12</sup> Of the 404 households who actually participated in the program, 89% chose animals as the asset that they wish to receive from the program. In the endline, only 43% of the households who chose livestock as their program asset still owned any animal. In the study of the ultra-poor intervention of Bandhan in West Bengal, by comparison, the endline included 429 households which were initially selected to participate in the program. Among those, 58.5% had assets at the endline (Banerjee et al., 2011, p. 8). This percentage is consistent with the relatively low initial take-up rate in the West Bengal study: just 52% of the 512 households initially offered the program took it up. The two pieces of data suggest that there was minimal asset sale/loss in the Bandhan replication, unlike in the replication by SKS NGO, which contributes to explaining the different measures of impact of these two programs.

Table 7 describes characteristics of treatment households based on their animal ownership at endline. At baseline, households that will later keep the animal given by the program were overall similar to those who eventually sell their animal, with the exception of the household size and the amount of land owned, which were both slightly larger for those who will own an animal at endline.

Panel B of Table 7 shows that households who did not own any animal at endline were more likely to report having sold animals in the last 12 months, as well as to report higher income from selling animals than those who still owned animals. The evidence suggests under-reporting of livestock sales, however. Table 7, Panel B, indicates that fewer than 20% of households who participated in the program and did not own animals in the endline reported having sold their animal.

To pursue the possibility that sales are under-reported, we worked with SKS to implement a follow-up survey of 57 treatment households which chose buffaloes or goats as their activity in the program but reported not owning an animal in the endline survey. In this follow-up survey, 75% of the respondents indicated that the animal they received from SKS was lost or had been sold, and 15% indicated still owning and caring for the animal (the remaining households answered “other”).

Data on household indebtedness reinforce the argument that households that did not hold on to their animal actually sold it. Panel B of Table 7 indicates that, compared to households that held on to their animal, households that did not own animals at the time of the endline survey were 19 percentage points less likely to have outstanding loans, had a smaller number of loans outstanding, and had significantly lower average outstanding loan amounts.

Some households may thus have made a choice to stop pursuing their livestock-related activity and used the proceeds from selling their animal(s) for other purposes. At the same time, households that held on to their animals did better than others by the endline. Total per capita income and expenditures increased more for households that held on to their animals than for those who chose to sell them. The difference is statistically significant (not shown). We cannot causally interpret these differences since holding on to animals is an endogenous choice, but the pattern is consistent with heterogeneity in treatment effects, followed by re-optimization toward wage labor by those who experienced weaker impacts from program participation.

In sum: Few households dropped out of the intervention during its 18 month lifetime, but many sold their assets afterward. Selling assets was the right of the households, though it countered the intent of the program. Asset sales are strongly correlated with debt reduction. From the perspective of the program design, asset sales are a form of non-compliance (i.e., drop-out) that helps to explain the lack of net impacts on income and consumption by the time of the endline survey. The pattern underscores the importance of context: ultra-poor households in our sample had relatively high levels of debt at the baseline, and (as the next section shows) increasingly attractive opportunities in the wage labor market.

## 8. Income, income composition, consumption, and time-use

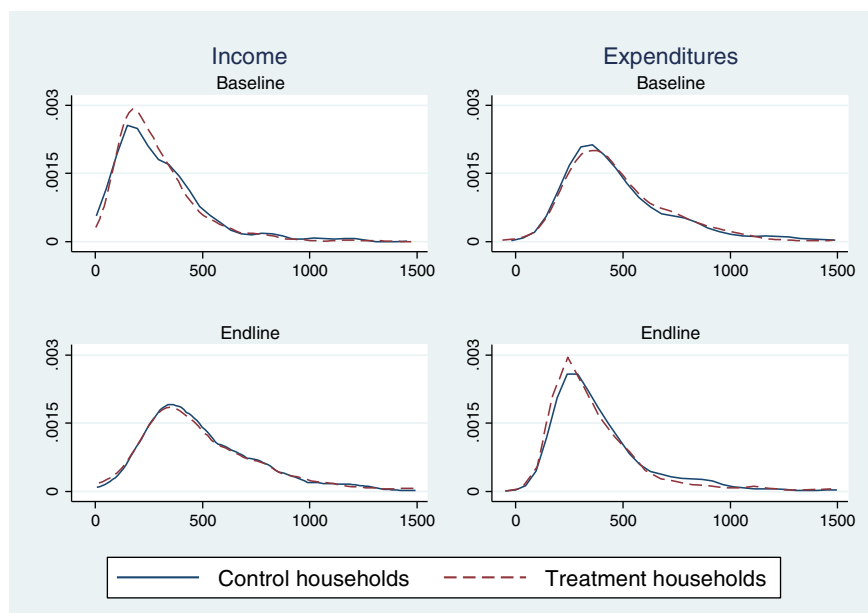
The study period was one of rapid economic growth in the state. In line with that, average monthly per capita total income in our sample increased from Rs. 321 (US\$19.4 in PPP conversion) in the baseline to Rs. 516 (US\$31.2 in PPP conversion) in the endline, a 61% increase. Fig. 2 shows that the distribution of monthly income per capita shifted to the right and flattened between the baseline and endline surveys. It also highlights that these changes happened similarly for the treatment and control households.

The similar income increase in the treatment and control villages yields the finding of no net impact on income. Column 1 of Table 8 reports the coefficients from a regression using the base specification with per capita monthly income as a dependent variable. The coefficient is near zero (−0.06) with a large standard error (22.6). The effect size is estimated as zero (calculated with Stata using the partial  $\omega^2$  estimate).

The other columns of Table 8 analyze the components of household income. Columns 3 and 6 show that the program was successful in raising income from livestock and a smaller increase in agricultural labor income. The regressions show that by the endline the treatment households' increase in livestock income was Rs. 56 larger than that of the control households', while the former's increase in income from agricultural labor was Rs. 50 lower than the latter's. The Rs. 56 increase is 18% of baseline income for the treatment group (Rs. 312) and 27.5% of the total income gain that the treatment households experienced between the baseline and endline (from Table 2: Rs. 515 − 312 = Rs. 203). These increases are important and relatively large; nevertheless, most of the income increases between baseline and endline came

<sup>11</sup> The results are largely in line with qualitative evidence on asset accumulation from Jawahar and Sengupta (2012).

<sup>12</sup> We note that there is no indication that households joined the program with the intent of eventually selling the asset.



**Fig. 2.** Density of monthly per capita income and expenditures. The graphs show distribution of per capita monthly total income and expenditures, truncated at Rs. 1500. Horizontal axes show amounts that are in Rupees of 2007.

from labor income, even for the treatment households. The changes are in the same range as those from the summary statistics in Table 2.

We can rule out that asset sales explain the treatment effect on income. In Appendix Table 14, we re-run the key specifications from the income regressions in Table 8. We find essentially the same results after removing revenue from animal sales from the definitions of total income and livestock income. The treatment effect on livestock income, for example, falls modestly from 56 (Table 8) to 52 (Appendix Table 14), remaining statistically significant.

Changes in the households' use of time are consistent with the observed changes in income. Measures of time use presented in Table 9 include both adults and children to take into account the fact that the latter often help with tending animals and with household chores. The table shows that aggregate measures of time spent in productive activities, in leisure, and doing chores did not change differently for the treatment and control households. The intent-to-treat measures of time use over the past 24 h, however, show that treatment households spent more time tending animals than control households (18 min per day on average), and less time doing agriculture labor (42 fewer minutes per day on average).<sup>13</sup>

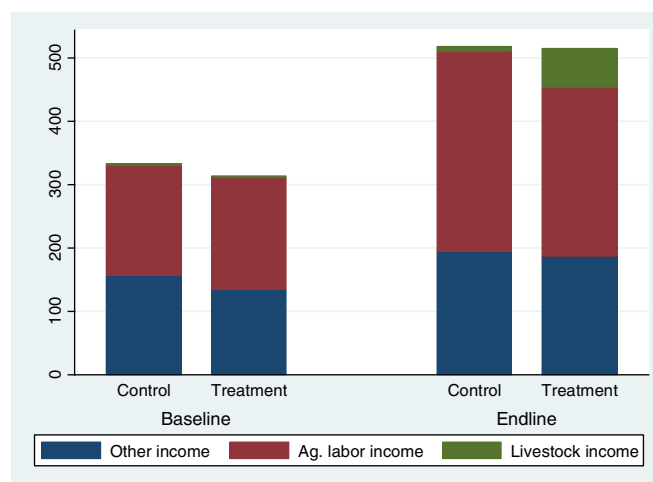
In sum: on average, both treatment and control households experienced large increases in total income per capita. On net, though, treatment households did not experience larger income increases than the control group. Fig. 3 provides a visual summary of the finding on the composition of income. While neither the levels of income nor the change in total income were statistically different in the treatment and control groups, the change in the composition of income was. Treatment households obtained a larger share of their income from livestock than control households, while the latter obtained a larger share of their income from agriculture labor than the former. Still, for both treatment and control groups, increases in total household income were mainly driven by labor income.

### 8.1. Consumption, food security, and health outcomes

As described in Section 3, baseline consumption data suffer from systematic measurement errors. We describe the impact of the program on

household consumption nonetheless since it is an important outcome. Fig. 2 shows the density of total monthly per capita consumption for treatment and control households, and Fig. 4 details consumption into food and non-food consumption. As the graphs indicate, the distribution of total and food expenditures shifted toward the left side, indicating a decrease over time consistent with a substantial measurement error in the baseline.

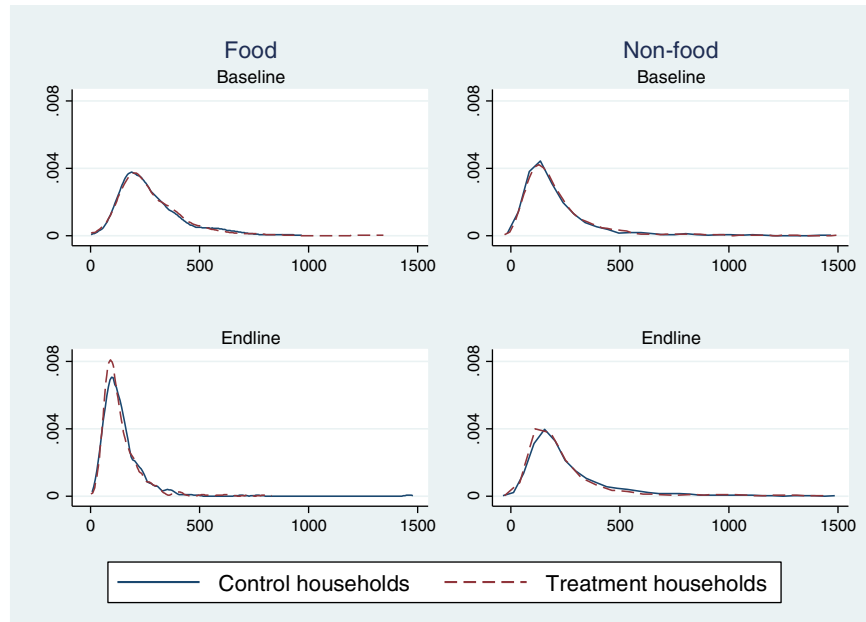
In Table 10 we report the results from the base regression with various measures of monthly per capita expenditures as dependent variables. The differences in consumption between the treatment and control households were small and not statistically significant. The effect size is 0.0005 (calculated with Stata using the partial  $\omega^2$  estimate). To limit the influence of measurement error in the baseline, Appendix Table 15 presents coefficients from a cross-sectional regression on endline data only; again, the treatment effect is small and not statistically significant.



**Fig. 3.** Average household monthly per capita income, by source of income, survey wave and treatment assignment. Other sources of income include non-agriculture labor, agriculture and non-agriculture self-employment, salaried employment, and other unclassified sources.

<sup>13</sup> Appendix Table 18 provides time-use regressions for the full range of possible uses of time.





**Fig. 4.** Density of monthly per capita food and non-food expenditures. The graphs show distribution of per capita monthly food and non-food expenditures, truncated at Rs. 1500. Horizontal axes show amounts in Rupees of 2007.

As a further check, we ran a series of regressions on consumption-related questions. Only one shows a significant impact. Appendix Table 4 gives results on 5 indicators: Did adults cut the size of meals or skip them? Did adults not eat for a whole day? Did children under 16 cut the size of meals or skip them? Do all households have enough food every day, all year? And does everyone in the household eat at least two meals per day? Only one of the 5 treatment effects is sizeable and statistically significant: the intent-to-treat effect on children under 16 reducing or skipping meals is  $-0.07$  (significant at 10%). The outcome is not prevalent in the baseline, though; only 4% of households in the baseline indicated that this was an issue.

Appendix Table 16 gives results from regressions on health-related questions. We analyze indicators of whether physical health improved in the last year, the number of days that household members were unable to work due to illness, and whether any member went to the doctor or hospital in the last year. In line with the lack of results on income and consumption, none of these treatment effects is large and none is statistically significant.

## 9. Saving, borrowing, and participation in government programs

In this section, we explore impacts on the households' financial lives. One important motivation for the program was to help ultra-poor households establish a microenterprise with a regular income flow that will help them "graduate" into microfinance or other sustained source of support. The program required the treatment households to save every week such that at the end of 18 months they had accumulated at least Rs. 800 to "graduate." Data collected immediately at the end of the program indeed show that treatment households reported being more likely to save and had higher savings balances than control households (data not shown).

These effects did not persist in the long run, however (as seen in the top row of the final two columns of Table 11). On average, in the long run all households reduced their borrowing and were more likely to save than they were in the baseline, but not differently so for treatment and control households. The small-sample qualitative interviews found that, two and a half years after the program ended, almost all participants had withdrawn their savings and closed the post office account that had been opened for them during the program (Jawahar and Sengupta, 2012). Some households prefer to keep cash at home, but

the lump sum created while in the program was commonly used to repay outstanding debts.

Table 11 shows the impact of the program on access to credit, measured as (i) the likelihood of having outstanding loans, (ii) the number of outstanding loans, and (iii) the total amount of loans outstanding. Over the long run, uses of loans were not significantly different for treatment households than for control households. The drop in debt among treatment households that sold their animal between midline and endline surveys is not large enough to be reflected in the overall treatment-versus-control comparison. Appendix Table 17 looks at the sources of loans and finds a small but statistically significant increase in the use of loans from shopkeepers for treatment households, compared to control households, but no other significant difference, particularly in the use of loans from microfinance institutions or self-help groups.

### 9.1. Use of government safety nets

The expected net impact of the ultra-poor program on the use of government safety nets is ambiguous. On one hand, part of the training provided to ultra-poor households was meant to empower them to connect with existing support in their community, including government social services. Jawahar and Sengupta (2012) note that this effort was augmented by "political competition" in Medak District during the period of the ultra-poor graduation intervention. Such competition increased awareness of, and participation in, government safety nets for all households. On the other hand, a long term goal was to create independent livelihoods and reduce reliance on public safety nets.

Table 12 shows no overall evidence of a substitution of the ultra-poor program with government safety net programs. Ultra-poor households were 9 percentage points more likely to participate in government housing programs than control households, a statistically significant difference, but not statistically significantly more or less likely to participate in any of the other programs relative to control households.

The table shows how widespread some government programs already were at the baseline. The final row in the final two columns of Table 12 show that at baseline over 90% of the sample had access to resources through a Below Poverty Line (BPL) card or Ration (PDS) card. The BPL card is a "white card" which serves three main purposes: (1) establishes the identity of the individual and the family, (2) entitles

**Table 11**  
Impact of the ultra-poor program on loans and savings.

	Household has outstanding loans?	Number of loans outstanding	Value of loans outstanding	Household saves?	Total savings balance
Treatment	0.02 (0.05)**	0.01 (0.06)	5.35 (306.31)	0.03 (0.04)	−65.00 (62.07)
Baseline dep. var.	0.13** (0.04)	0.10** (0.03)	−0.01 (0.02)	0.20*** (0.03)	0.06 (0.06)
Constant	0.43*** (0.08)	0.52*** (0.11)	529.42 (577.39)	0.43*** (0.06)	183.05* (105.33)
Observations	1009	1009	1009	1009	471
R-squared	0.03	0.03	0.01	0.05	0.02
Mean and (std. dev.) of dep. var. at baseline	0.712 (.453)	0.968 (.791)	2718 (5086)	0.555 (.497)	126 (558)

Notes: Standard errors are clustered at the village level. Regressions in which the dependent variable is a binary variable are run as linear probability models. The treatment is the offer to participate in the program (intent-to-treat estimate). Stratification variables used in the randomization (total number of households in the village, distance from village to the nearest metallic road, and percentage of poor households in the village) are included in the regressions but not shown. The amounts of loan outstanding and savings balance are in 2007 Rupees; 1 USD ≈ 40 Rs.

\*\*\*  $p < 0.01$ .

\*\*  $p < 0.05$ .

\*  $p < 0.1$ .

the family to, and is used for delivery of *all* government services such as housing, educational fee waiver, health insurance and pension, and (3) entitles the family to subsidized rations through the public distribution system (PDS). A Ration (PDS) card also entitles a family to subsidized food (grains) and non-food items (sugar, fuel, etc.) through the public distribution system.

The National Rural Employment Guarantee scheme is of particular interest. The NREG scheme is the largest public safety net scheme in the world. In its fiscal year 2010–2011, it provided employment to 53 million households in India, including six million in Andhra Pradesh (Ministry of Rural Development of the Government of India, 2011). The NREG scheme provides up to 100 days of unskilled wage employment per household, for a daily wage that averaged Rs. 115 in March 2011.<sup>14</sup> Although a minority of households actually worked for 100 days in fiscal year 2010–2011, the potential income from NREG represents a substantial proportion of an ultra-poor's total yearly income and could contribute to dampening the measured impact of the ultra-poor program. Our data, however, do not support this hypothesis. Even though participation in NREG increased sharply in our sample between the baseline and endline surveys (from about 34% to about 81%), the rate of increase was not statistically significantly different for the treatment and control households (Table 12, column 1) and the amount earned from working in the scheme was similar for the treatment and control households in the endline survey (Table 2).<sup>15</sup>

## 10. Conclusion

We report on an asset transfer program aimed at ultra-poor households in rural India, part of a set of pilot programs designed to test efficacy in different sites and conditions.

The program aims to permanently shift ultra-poor households' living conditions by providing resources (including training, an asset, and other support) intensively but for a limited time, rather than simply providing an ongoing safety net. The basic idea of the program is for households to establish a microenterprise with a regular cash flow such that they can move out of extreme poverty. Over the 18 months of the program, households received support in the form of intensive

training and monitoring, and a stipend to meet enterprise-related expenses (but not to support household consumption).

In contrast to findings from evaluations of similar programs in other regions, we find no significant long term net impacts of the program on income and asset accumulation of ultra-poor households. (Nor do we find impacts on total consumption, though we highlight measurement error in those data.) Overall, the data are imperfect, but the results are robust to a series of checks and alternative specifications (in Appendix tables).

As with many evaluations, it is possible that a better designed and/or implemented program would have had a stronger impact. The program, though, was implemented largely as designed, and the source of weakness is not implementation failure. A qualitative study by Jawahar and Sengupta (2012) echoes the conclusion that the program was largely implemented as designed. Could the intervention have been improved? They raise the additional question of whether the program could (or should) have been designed differently:

Our observations beg the question: “Did the way program inputs were designed get in the way of sustained behavioural change?” SKS emphasized peer-to-peer learning over sustained one-on-one interactions between UPP members and field officers. Household visits consisted of a summary discussion with the member outside her homestead to inquire about the status of her assets, the health of family members, and whether her children were attending school. This format did not allow any room for customized and creative support from the [Field Officer], on whom SKS discouraged any sort of emotional dependence (p. 19).

Jawahar and Sengupta (2012) also note a consequence of SKS NGO ending engagement after 18 months of the intervention. While veterinary services were provided during the intervention, awareness of the importance (and availability) of further vaccination was incomplete at the end of the intervention. They note with regard to women who received goats: “Without past experience in their chosen livelihoods, and without support networks to turn to, many had lost their assets due to illness or death (or been forced to sell them to cover expenses) and were effectively indistinguishable from women who had not participated in the program.” (p. 13) Goats, though, only accounted for a third of livestock choices, and buffalo, the most popular choice, fared much better.

Another concern raised by Jawahar and Sengupta (2012) is independent of the intervention, but bears on the period after its completion. Medak district had been an area richly served by microfinance, including the for-profit SKS Microfinance. This is one reason that SKS NGO chose to work in Medak. Women in the SKS ultra-poor graduation project were not involved with microfinance, but the most successful and

<sup>14</sup> These numbers are more indicative than definitive, as official NREG wages do not perfectly reflect the amounts received by households (Niehaus and Sukhtankar, 2013). Implementation has been uneven in India, but Imbert and Papp (2015) highlight Andhra Pradesh as one of five “star states” with the best and most intensive implementation.

<sup>15</sup> The lack of displacement of NREG participation arises in part because the work is close to the village (and sometimes within it), making it possible to simultaneously care for livestock. Working as an agricultural laborer, in contrast, usually requires travel and being away from home for extended stints.

**Table 12**  
Impact of the ultra-poor program on the use of government safety nets.

	Household sought or received the following:						Received goods with PDS card	Has BPL card
	work from EGS	pension	govt. housing	govt. assets	govt. training	subsidized loans		
Treatment	−0.01 (0.03)	−0.03 (0.05)	0.09** (0.04)	0.00 (0.03)	−0.01 (0.03)	−0.02* (0.01)	0.00 (0.01)	0.01 (0.01)
Baseline dep. var.	0.05** (0.03)	0.10*** (0.03)	0.08** (0.04)	−0.06** (0.03)	−0.05** (0.02)	−0.04*** (0.01)	0.13*** (0.04)	0.12*** (0.04)
Constant	0.82*** (0.06)	0.62*** (0.10)	0.10 (0.08)	0.11*** (0.04)	0.08** (0.04)	0.06** (0.03)	0.83*** (0.04)	0.84*** (0.05)
Observations	1006	1006	1006	1008	1007	1006	1008	986
R-squared	0.01	0.02	0.03	0.01	0.01	0.01	0.06	0.05
Mean and (std. dev.) of dep. var. at baseline	0.342 (.475)	0.646 (.478)	0.167 (.372)	0.039 (.193)	0.005 (.068)	0.024 (.152)	0.927 (.259)	0.920 (.271)

Notes: Standard errors are clustered at the village level. Regressions in which the dependent variable is a binary variable are run as linear probability models. The treatment is the offer to participate in the program (intent-to-treat estimate). Stratification variables used in the randomization (total number of households in the village, distance from village to the nearest metallic road, and percentage of poor households in the village) are included in the regressions but not shown. EGS include all government “employment-generating schemes,” the largest of which is the National Rural Employment Guarantee scheme created by the Mahatma Gandhi National Rural Employment Guarantee Act of 2005.

\*\*\*  $p < 0.01$ .

\*\*  $p < 0.05$ .

\*  $p < 0.1$ .

ambitious “graduates” may possibly have had a chance at engagement with microlenders. A major regulatory action in December 2010, though, ended that chance. In responding to consumer protection concerns, authorities enacted microfinance regulations that effectively stopped microfinance expansion and greatly reduced its scale in the state. The self-help groups, a form of microfinance in which the village women were involved, continued at scale, however.

In summarizing implementation issues: the intervention was clearly ambitious and input-intensive, but questions have been raised about the intensity of engagement of participants and field officers, and the sharp withdrawal of the program at the end of 18 months. The state-wide microfinance regulation enacted in December 2010 may also have affected post-intervention possibilities for some participants. Our analysis, though, can only investigate the intervention as designed, one that was launched with much hope and in parallel with other international pilots.

Taken as a whole, the study shows that the program helped some households create new livelihoods as intended. The study also shows that the treatment group engaged less in the labor market, creating an offsetting impact on income and time use. The region had depended most heavily on wage labor before the intervention, and during the study period wages in agricultural labor markets were rising steadily. This created the context for displacement. We found that all households with available labor (whether in the treatment or control groups) were able to substantially improve their economic conditions. Households in the treatment group, though, had the choice to either participate in the ultra-poor program or work more intensively in the labor market. While 70% of eligible households opted to participate in the program and 89% of these were given livestock, a large number of them later dropped out of the program (as reflected by the fact that only 43% of households who had been given livestock still owned an animal by the endline survey). The strong labor market created incentives for households in the treatment group to sell their assets, pay off debt, and turn to wage labor.

The roles of displacement and drop-out are similar to those described in the context of job-training programs in the United States by Heckman et al. (2000). In this spirit, we speculate that in another economic setting (with a weaker market for wage labor), the exact same intervention targeted to an identical population might have generated positive net impacts — as suggested by the range of positive results reported by Karlan and Goldberg (2014). To the extent that it is so, the study illustrates how the external validity of impact estimates — the ability to generalize from one setting to another — depends not just on the similarity of populations but also on the nature of the alternative economic activities that are available in the region.

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## Appendix A. Supplementary data

Supplementary data to this article can be found online at <http://dx.doi.org/10.1016/j.jdeveco.2015.03.005>.

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