Substitution Bias and External Validity:

Why an innovative anti-poverty program showed no net impact

Jonathan Morduch, New York University*

Shamika Ravi, Indian School of Business

Jonathan Bauchet, Purdue University

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Abstract

The net impact of development interventions can depend on the availability of close substitutes to the intervention. We analyze a randomized trial of an innovative anti-poverty program in South India which provides "ultra-poor" households with inputs to create a new, sustainable livelihood. We find no statistically significant evidence of lasting net impact on consumption, income or asset accumulation. Instead, income from the new livelihood substituted for earnings from wage labor. A very similar intervention made a large difference elsewhere in South Asia, however, where wage labor alternatives were less compelling. The analysis highlights the roles of substitution bias and dropout bias in shaping evaluation results and delimiting external validity.

JEL codes: O1, J2, C1, I3

* Corresponding author. NYU Wagner Graduate School of Public Service, 295 Lafayette Street, 2nd Floor, New York, NY 10012, USA. Phone: (212) 998-7515; Fax: (212) 998-4162; Email: jonathan.morduch@nyu.edu.

SUBSTITUTION BIAS AND EXTERNAL VALIDITY:

WHY AN INNOVATIVE ANTI-POVERTY PROGRAM SHOWED NO NET IMPACT

Jonathan Morduch, Shamika Ravi, Jonathan Bauchet

1. Introduction

The poorest of the poor face broad challenges. The traditional policy response is to create safety nets, with publicly-funded income transfers that provide a basic standard of living. The transfers are designed for survival, not economic advancement. BRAC, a globally-recognized NGO based in Bangladesh, sought to improve on the standard safety net idea by instead giving poor households a larger quantity of resources in a shorter period of time. BRAC coupled financial transfers with training and assets to help recipients build a new livelihood as a self-employed, small-scale entrepreneur (Matin and Hulme 2003). The bet is on the possibility of "graduation" 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. 2011, Sachs 2005). BRAC created the model in Bangladesh, and donors have supported its replication and evaluation in India, Pakistan, Ghana, Ethiopia, Yemen, Haiti, Peru, and Honduras.[1]

We design and implement an RCT to analyze the replication of a similar 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), a year after the intervention ended there were no statistically significant net impacts on average household income, consumption, asset accumulation, nor use of financial services. We

1 Information on all sites is available at http://graduation.cgap.org/. The evaluation of the replication in West Bengal has followed on a similar timeline to this one.

show that the program was implemented as designed, but it caused substitution away from paid wage employment, erasing the net economic and social impacts on the treatment group.

The substitution mechanism is a version of “substitution bias” (Heckman et al. 2000). Concerns with external validity tend to take two forms. First, difficulties when generalizing in the face of population heterogeneity (e.g., Alcott and Mullainathan 2012, Heckman and Vytlacil 2007, Eldridge et al. 2008), and, second, difficulties when there are varied complementary inputs – including differences in infrastructure, transportation, government programs, and economic conditions (Cartwright 2010).

Substitution bias is a third, less appreciated class of problems for external validity. It receives no mention on the extensive list of biases described in a well-cited toolkit on RCTs in developing countries (Duflo et al. 2008); nor in essays that cover problems of extrapolation from RCTs (Deaton 2010). Yet, optimization across alternative economic mechanisms, both formal and informal, is a mainstay of development theory (Bardhan and Udry 1999).

One reason that substitution bias may be under-recognized is that the formalization was formulated in a particular way for a particular problem. In parallel to the present context, Heckman et al. (2000) seek to explain why a promising social experiment did not deliver the expected positive net impacts. The Job Training Partnership Act (JTPA) was a large federal program in the U.S. that provided job skills training services and employment referral services to disadvantaged adults and youth. It was evaluated as a randomized controlled trial, with treatment groups given exclusive access to the JTPA. But Heckman et al. (2000, Table 1, p. 654) show that many people in the control group received training from other programs, getting training with similar quality and duration. In addition, some members of the treatment group dropped out of the federal training program. The substitution and drop out combined to create a situation in which the gap in actual training received between treatment and control groups was much smaller than 100 percent (the scenario in which the control group would not receive any training): it fell as low as 19 percent for some groups. The lower gap reduced the measured net effect of training on earnings and employment.

As a result, the JTPA social experiment could accurately measure the net effect of the particular *program*, but not (without strong assumptions) the effect of *training*. Policymakers, however, benefit from having answers to both questions when extrapolating lessons to other settings – and the latter can sometimes be more important than the former. Heckman et al. (2000) show that the private net return to training turns out to be large, even though the program itself delivered mixed results. In this line, they conclude that:

> Our evidence suggests that experimental evaluations cannot be treated as if they automatically produce easily interpreted and valid answers to questions about the effectiveness of social programs. Reporting the experimental estimates by themselves without placing them in the context in which treatments and controls operate invites misinterpretation. (p. 689)

To extend their analysis, it's helpful to generalize in two directions. Heckman et al. (2000) describe substitution bias in a way that follows from the actual JTPA experience: the treatment group received a useful program, and members of the control group found an alternative way to get similar services. In drawing the parallel to the experiment in India, it helps to re-formulate the JTPA substitution mechanism: both the treatment group and control groups have ways to get training services, but the treatment group was offered the JTPA training program as well (and many substituted into it). The outcomes in both formulations are similar, as is the implication for how RCT results are interpreted, but the controls act in the first case and the treatments makes the switch in the second.

One parallel is evaluations of microfinance, in which most poor households in developing countries already have access to some forms of finance, even if they are mostly informal (e.g., moneylenders, community-based savings groups, and loans from relatives; Collins et al, 2009). The introduction of a formalized microfinance program will induce some people in the treatment group to substitute away from these financial arrangements. Because of substitution bias, an impact evaluation would thus show the net benefit of access to the microfinance program, but will not provide answers to other relevant questions like the size of the private net benefit of access to finance in general. Das et al (2013) provide a budget-driven example; they document how households given educations grants re-optimize their spending to fully offset the grants, such that anticipated increases in school funding fail to yield significant improvements in students' test scores.

The second way to generalize the substitution bias mechanism is to apply the idea to substitution between any alternative activities that can be used to achieve similar ends. In the case of Heckman et al. (2000), the issue was that nearly identical training opportunities were available to the treatment and control group members. In the South Indian case, the options are less similar, but the basic mechanism remains. The issue in our study period was that the option to work as a wage laborer was increasingly compelling as wages increased rapidly in South India (Clément and Papp2012), and members of the control group benefited considerably. Members of the treatment group had to forego much of those gains if they participated fully in the anti-poverty program and got on a path to self-employment. Both wage labor and self-employment are alternative job strategies to obtain stable livelihoods, and any one person has difficulty doing both simultaneously.

The evidence shows that the SKS anti-poverty intervention directly created income gains by promoting livelihoods in the livestock sector (almost 90 percent of participating households chose livestock rearing as their enterprise). On average, income increased by 65 percent in the treatment group between the baseline survey and the endline survey.

But control group income increased by a similar amount (67 percent). Two developments can explain why the treatment and control groups had similar outcomes, yielding no net impact. First, gains from participation in the treatment group were offset by foregone wages from agricultural labor. Time constraints made it hard to both work fully as a wage laborer on other people's farms and to take care of one's own livestock as part of the SKS program. On average, households that participated in the anti-poverty program increased monthly per capita income from livestock by 53 Rupees more than control households (about US$3.20 in PPP conversion, or 17 percent of the average baseline monthly per capita income), but the control group increased monthly per capita income from agricultural wage labor by 51 Rupees more than the treatment group (calculations from Table 3). The relative gain was undone by the relative loss.[2]

Second, about 40 percent of households who elected to receive an animal from the program did not own any animal at the time of the endline survey. The evidence suggests that these households chose to sell their animal(s), pay down outstanding debt, and take advantage of opportunities in the labor market.[3] This mechanism corresponds to "dropout bias", a

2 The market exchange rate at the baseline (October 2007) was 39 rupees per US$1. At the endline (October 2010), it was 44 rupees per US$1.

3 On average, treatment households who did not own an animal had a lower total income per capita than treatment households who held on to their animal. The endogenous nature of the decision to keep or sell animals prevents us from interpreting this difference causally, but we note that households who sold their animal – likely those who were not doing as well as they hoped with livestock rearing – had higher income from wage labor than those who held on to their animal.

phenomenon related to substitution bias, in which households with compelling alternative opportunities drop out of the program to pursue those alternatives (Heckman et al. 2000). Dropout bias differs from attrition bias, since households fail to follow through on the programs' expectations, but they stay in the sample.

These possibilities for substitution between programs and alternatives are growing in India. India's recent economic growth has brought overlapping programs rolled out by banks, NGOs and the government. Of particular note is the ambitious National Rural Employment Guarantee scheme (NREG), which swept through our study region, guaranteeing (on paper) 100 days of employment per year per household, paid 115 Rupees per day on average (Ministry of Rural Development of the Government of India 2011). At the time of the baseline, 34 percent of all households in our sample (across treatment and control groups) participated in the NREG scheme; by the endline, 81 percent did.

The most important substitution that we find is not with NREG participation directly but with participation in the agricultural labor market broadly. At a national level, the National Sample Survey Organization (NSSO) data reveal a 27 percent increase in real wages for casual labor in rural India, between 2004 and 2010. The wage increase aligns with a broader shift out of self-employment and into paid labor. The NSSO calculated a drop in self-employment from 56 percent of the labor force to 51 percent between 2004 and 2010, while casual labor rose from 28 percent to 33 percent and wage labor rose from 15 percent to 17 percent. The SKS ultra-poor program, which was designed to promote self-employment in a population dominated by wage labor, can be seen as fighting against these trends.

All else the same, the net impact would have likely been greater in another region, with a less tight labor market or where wage labor is less prevalent. The version of BRAC's program implemented in West Bengal showed large positive net benefits to livestock income and entrepreneurial activities, with limited evidence of the substitution that marked the SKS program. One main factor, we suspect, is that in our site over 90 percent of the households cited wage labor as a main income source before the program started, versus only about half in West Bengal (Banerjee et al. 2011, Table 4). Similarly, a new round of BRAC's program evaluated with an RCT in Bangladesh shows that the program led to a large increase on average income. In BRAC's program, about half of ultra-poor households were involved in any wage employment, and only 28 percent were exclusively working in wage employment (Bandiera et al., 2012).

These programs followed a similar design and were instituted and evaluated through coordinated (but independent) studies. We cannot rule out, however, that some of the differences in net impact are due to elements of program design that were adapted locally. Most important, while the overall level of household support in the SKS replication was comparable to that in the other programs, the composition differed. In the SKS replication, households did not receive a consumption stipend, unlike in other locations; instead, a greater share of funds went to pay for the asset and its upkeep.

Recognition of substitution bias re-frames conclusions about what the anti-poverty program achieved and what it might contribute elsewhere. Even as efforts proceed to make evaluations more central in development policy, attention to external validity is mixed and incomplete, and there's no consensus about what should be considered a generalizable "proven impact." The findings here affirm the importance of rigorous evaluations while highlighting the conditional nature of impact results.

2. Background and Data

The Ultra Poor Program (UPP) in South India aims to establish microenterprises with regular cash flows, which would enable ultra-poor households to grow out of extreme poverty, and eventually gain access to microfinance in order to maintain and expand their economic activity. The pilot program was implemented by Swayam Krishi Sangam (SKS)[4]in198 villages of Medak district in the state of Andhra Pradesh, one of the poorest districts in India. The program we evaluate has now been introduced in the state of Orissa.

The program targets the poorest households which have few assets and are chronically food insecure. It combines support for immediate needs with investments in training, financial services, and business development. Funds to partially defray the costs of livestock rearing are transferred in the SKS version, but, unlike other program designs, no direct consumption support is provided. The overall cost of the program, though, is in line with other pilots. The aim is that within two years ultra-poor households are equipped to help themselves "graduate" out of extreme poverty. The approach is thus sometimes called a "graduation program."

The replications were inspired by the success in Bangladesh of BRAC's "Challenging the Frontiers of Poverty Reduction - Targeting the Ultra Poor" (CFPR-TUP) program, which reaches about 300,000 households in Bangladesh. BRAC estimates that over 75 percent of the beneficiaries in Bangladesh are currently food secure and managing sustainable economic activities. The program there has been studied extensively using non-experimental techniques(Emran et al. 2009, Krishna et al. 2012, Mallick 2009, Matin and Hulme 2003), with most studies finding positive impacts on income, consumption and asset accumulation of poor households. A randomized controlled trial evaluation of BRAC's program is also being

4 The program was implemented by SKS NGO, an entity distinct from SKS Microfinance.

conducted in Bangladesh, and we compare our findings with preliminary findings from that study (Bandiera et al. 2012).

The idea of expanding this type of interventions gained ground through concern that ultra-poor households remain outside most programs aimed at poverty reduction. Even within the context of microfinance, it has been noted that poorer households do not gain significantly from access to credit (Morduch 1999). Many government schemes that target “below the poverty line” households have failed to do so due to mistargeting (Drèze and Khera 2010, Jalan and Murgai2007, Ministry of Statistics and Programme Implementation of the Government of India 2005). Banerjee et al. (2007)find that the poorest are not any more likely to be reached by government programs than their better off neighbors.

SKS’s Ultra Poor Program

The program as implemented by SKS is an 18-month intervention aimed at extremely poor households, identified through detailed participatory rural appraisals and village surveys. Households have to meet 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 takes into account characteristics of the house such as its size, building material, and electricity and water access.

The program comprises 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 involves a one-time asset transfer, enterprise-related training, cash stipend for large enterprise-related expenses, and the collection of minimum mandatory savings. It starts 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, are also available. Once the household has selected an activity, it undergoes training sessions where one ultra-poor member, usually the woman head of household, is taught skills pertaining to the specific enterprise she has chosen and how to find additional help when needed (for example, veterinary care). After the training is completed, the specific asset or in-kind working capital is procured and transferred to the household. A mandatory weekly savings is required of all households, once the asset begins to generate cash flow, such that households save at least $16 by the end of the program in order to “graduate.”

On average, the program cost US$357 for each participant (Table 1). The costs of the asset and stipend given to help households meet enterprise-related expenses represent 42 percent of the total program cost. Capacity building (training) and implementation are the next two biggest costs (30 percent and 26 percent, respectively). The remaining costs were incurred at the targeting phase.

A large majority of households in the program chose to rear livestock as their enterprise: 55 percent of all households chose a buffalo, 31 percent chose goats, and three percent chose donkeys, pigs or sheep. The next most popular choice was non-farm business, an activity elected by seven percent of households. Finally, almost 3.5 percent of households used the program’s grant to purchase land, earning an income from leasing it out for agricultural production. All analyses are performed with the entire sample of households, because the sample of households which chose non-farm businesses and land lease is too small.

The second component of the program is the provision of essential primary health-care support. This is a combination of preventive training and techniques, and on-the-spot coverage. The health program is divided into the following: 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) health screening and information awareness camp hosted with support from government doctors and health focused NGOs; c) monthly information session conducted by the health assistant on topics such as contraception, pre- and post-natal care, sanitation, immunization, tuberculosis and anemia; and d) one or two program member in each selected village is trained by a doctor on basic health services. This member is equipped with basic medicines (available free of cost from the government) and a knowledge of when to recommend a case to a doctor or hospital, and serves as the touch-point for other members.

The third component of the program is social development. It involves 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 is encouraged through weekly meetings where members discuss common concerns and solutions. A rice bank is created by members depositing a handful of rice every day, which can be drawn upon by member households at no interest.

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

After18 months, SKS stops conducting the weekly meetings, collecting the weekly savings from members and organizing health camps in the treatment villages. The asset becomes a complete responsibility of the household with no enterprise-supporting stipend or advisory support from SKS. By the end of the program implementation, households are supposed to "graduate" out of extreme poverty. The 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, household must also have gained knowledge about social and health issues, and become aware of any available government programs.

Our findings on the net impact contrast with broadly positive impacts found in parallel studies in West Bengal, India (Banerjee et al. 2011) and the original BRAC program in Bangladesh (Bandiera et al. 2012). Why do the results differ? The most immediate possibility is program failure (a failure to effectively implement the program). Taken on its own terms, however, the program was not a failure. SKS implemented a Client Monitoring System to track the progress of program participants throughout the 18 months of the program. (No data was 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 involved, among other things, in the evaluation of BRAC's own TUP 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 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.

The Client Monitoring System shows that the average cost of the program reached US$357 for each beneficiary, covering an asset with which to start a small enterprise, a stipend covering enterprise-related costs, and 18 months of peer-to-peer skills training, basic healthcare and saving promotion. As evidenced by detailed results described below, 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 percent of participants reached that goal.

Data

Most of our analyses rely on detailed quantitative data collected from3,485 individuals, living in 1,064 households across 198 villages in Medak district, in three waves of surveying between 2007 and 2010.

The baseline survey was conducted between August and October 2007. Detailed information was collected on socio-demographic characteristics of the households, which included religion, caste, family type, size of household, age, marital status, disability, education, occupation, and migration details. Information was also collected on the household's living conditions, including 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) was recorded. The baseline survey also included measures of asset ownership, use of time, women's social status and mobility, and political awareness and access. Health information collected included data on physical health, hygiene habits and mental health conditions of household members. In addition, we have gathered details of household monthly consumption expenditure, income and other financial transactions of the household. We also collected details on social standing of the household within the community and future aspirations of the household members.

Following the baseline survey, we randomly assigned 103 villages to the treatment group and 95 to the control group. The 103 treatment villages included 576 households (54 percent of the total sample) who were offered the treatment.[5] Of these, 426 households participated in the program and 150 households declined to participate. In all our analyses, these 150 households are counted as part of the treatment group (to measure the intention to treat estimates). The most common reasons for not participating in the program were "not interested in taking asset" (52 percent), migration (33 percent) and having access to microfinance loans (11 percent).[6] "Microfinance" loans do not include loans from self-help groups; almost 50 percent of households which reported having outstanding loans in the baseline had one or more loans from self-help groups. SKS realized post-targeting that 19 households initially deemed eligible for the program had existing access to microfinance products. Since the design of UPP aims to "graduate" people into microfinance, households that already enjoy access are deliberately left out of the program.

A midline survey was conducted for the entire sample between April and September 2009, immediately at the end of SKS'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 effects 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

5. Note that with 5.6 households per village participating in the treatment, general equilibrium effects are unlikely.

6. 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 this part of rural India.

the impacts of interest are the program's long-term impacts, and to compare outcomes measured at similar periods of the year, we focus our analyses on baseline and endline surveys.

The endline survey was conducted for the entire sample of households almost exactly three years after the baseline, in October and November 2010. In the endline wave, we were able to reach 1,011 of the baseline households. The endline survey included the same questions as the baseline survey, with the addition of two new sections that collected detailed information on participation in the NREG scheme, including number of household members working in the scheme, number of days worked, and payment received for work in the scheme. The other additional section collected height and weight data for children under 10 years of age living in the household.

The rate of attrition between baseline and endline surveys was five percent. We compare in Appendix Table 1 the means of various household characteristics between households that we successfully reached in the endline survey and those that we could not. The households that we were not able to follow up 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. Appendix Table 1 shows that the difference in attrition rates between treatment and control groups is not statistically significant. We tested whether attrition was different for 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, the five control variables, as described in the Analysis Strategy section below, and the interaction of the treatment dummy and each of the control variables. An F-test of the joint significance of the treatment dummy and the five interactions confirms that being assigned to the treatment group does not significantly predict long-run attrition ($F = 0.51$, p-value = 0.802).

Most of the analyses compare the baseline data to the endline using a difference-in-difference strategy. For consumption, however, our main focus is on the endline only. This is a response to evidence of systematic measurement error in the baseline consumption data. The summary statistics in Table 3 document the reasons for concern. First, baseline monthly household consumption per capita is implausibly larger than baseline income data. The control group earned an average of 312 rupees per person per month but is measured as having spent 587 rupees; the treatment group earned on average 313 rupees per person per month but is measured as having spent 543 rupees. In contrast, the income and consumption data are within 10 percent 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 (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, Radhakrishna and Sengupta 2009.) The endline consumption data, however, is 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 group is 496 rupees per person per month. Third, average food expenditures drop by half between the baseline and endline surveys (Table 3), which is not consistent with households reports of improvements in food security as measured by whether any household member skipped meals, whether adults ever go an entire days without eating, or whether all household members had enough food all day, every day (Appendix Table 2). Fourth, the consumption decline is not consistent with rising income as seen in Table 3 (and seen in the region generally).

For completeness, we present difference-in-difference analyses of the impact of the program on consumption expenditures even though the results may be biased by measurement error. Our focus, though, is on results for consumption using the endline data only. The endline-only results are consistent with the broader analyses.[7]

The SKS intervention was also assessed in an independent qualitative study conducted 2.5 years after completion of the program (Jawahar and Sengupta 2012). The qualitative study was conducted using seven focus group discussions and 32 individual interviews with program participants and control group households, as well as interviews with program staff. These data are not meant to measure the program's impact, but they provide insight into how the program worked and conditions in treatment and control villages. Overall, the qualitative findings line up with findings from the RCT.

Who were the ultra-poor?

Table 3reports 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 10 percent of households reported in the baseline survey owning one or more animal(s). Animal ownership differed across treatment status in the baseline survey: seven percent of control households and 13 percent of treatment households owned an animal. The difference is statistically significant.

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

7 We tried 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. The survey firm had no role in implementing the intervention itself.

0.60 US dollars per day in purchasing power parity (PPP) terms. Even though 65 percent of ultra-poor households in the area had more than one source of income, they were very heavily dependent on agricultural labor as a primary source of income: at baseline, more than half of their per capita income came from agriculture labor. Average livestock income was very small, and more than 90 percent of all households did not have income from livestock (not shown).[8]

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 percent of all households. On the other hand, fewer than five percent of households reported in the baseline survey 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 percent of all households participated), but increased sharply from 2007 to 2010. By the endline, 80 percent or more of both treatment and control households worked in the scheme.

Even though sample households were among the poorest households in a poor district of India and participation in microfinance excluded them from being eligible for the program, our baseline survey indicates that they had an active, mostly informal, financial life. At baseline, before receiving any service from SKS, more than 50 percent 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.[9]

[8] As indicated above, average per capita monthly consumption appears to be measured with substantial positive error. Table 3 reports the impacts of the program on consumption, which should be taken with caution.

[9] 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.

Overall, these baseline descriptive statistics highlight that households eligible for the ultra-poor program and included in our sample 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. The population thus fits squarely within the targets set by the ultra-poor program.

3. Experimental Design and Empirical Strategy

Design

The impact assessment of the program is conducted through a randomized controlled experiment, where the level of randomization is the village. The assignment was stratified by village population, number of ultra-poor households as a proportion to total village population, distance from nearest metallic road, and distance from nearest mandal headquarter.[10]

We randomized at the village level due to (i) ease of program implementation and group interventions 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 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 individual-level randomization) was required to tease out the impact of the program. Power calculations assumed a relatively high level of intra-village correlation ($\rho = 0.30$).

10 A mandal is an administrative unit lower than the district but including several villages.

Analysis strategy

Before turning to the analytical strategy, we describe a frame for interpreting the estimated parameters. We focus on the role of substitution between the ultra-poor program and wage labor. The effect can be seen by considering two different interventions, T and x, that affect income y such that $y = \beta_0 + \beta_1 T + \beta_2 x + \beta_3 (T \cdot x) + \varepsilon$ where $E(\varepsilon|T, x) = 0$. With x = 1 everywhere, the common measure of impact, which is the treatment-control difference, is thus $\Delta = E(y|T = 1, x) - E(y|T = 0, x) = \beta_1 T + \beta_3 (T \cdot x)$. In our context, T is eligibility for the ultra-poor program and x is access to the agricultural labor market. In our case, even though access to T is limited to the treatment group, everyone in the treatment or control group has access to x. Thus the concern is not that the control group is contaminated. Instead, the concern arises from shifts in households' portfolios of economic activities (re-optimization) from x to T. The two opportunities may interact positively ($\beta_3 > 0$) if re-optimization brings out ways that they reinforce each other, or negatively ($\beta_3 < 0$) if there is substitution.

With x = 1 everywhere, families in treatment areas opt to split their energies between the two available options T and x, while families in control areas fully participate in their single option x. The treatment-control difference $\Delta = \beta_1 T + \beta_3 (T \cdot x)$ is thus smaller than $\beta_1 T$ when $\beta_3 < 0$. Where there is full displacement, β_3 could be large enough in absolute value to explain the finding that$\Delta = 0$.[11] The logic for $\beta_3 < 0$ in our case hinges on the hypothesis that if a person engages in the ultra-poor program, she lacks the time, energy or freedom to simultaneously participate fully in agricultural labor.

11 At the same time, the result could be consistent with there being a potential positive impact when the alternative intervention is not available (x = 0 everywhere) in which case the impact would be$\Delta = E(y|T = 1, x = 0) - E(y|T = 0, x = 0) = \beta_1 T$.

This scenario highlights that families in the treatment group would have been in roughly the same place had the ultra-poor program not existed (assuming they re-optimized and took greater advantage of other labor opportunities). But it is simultaneously true that inputs from the ultra-poor program translated into meaningful outcomes for those it served. The distinction from the finding that $\beta_1 = 0$(that is, program failure) matters when extrapolating from the result that $\Delta = 0$ and for understanding what was actually estimated.

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 β in the following reduced-form regression

$$Y_{ij} = \alpha + \beta T_i + v_j + \varepsilon_{ij} \quad (1)$$

Where i indexes households and j indexes villages. Y is the outcome of interest (consumption, income, etc.). T is an indicator variable that equals 1 if household lives in a treatment village and 0 otherwise, and β is the impact of the treatment. The variables v_j and ε_{ij} are the unexplained variance at the village and the household level. In theory, since the treatment was random across villages, ε_{ij} is uncorrelated with T. The coefficient of interest β is the intent-to-treat estimate which measures the expected change in the outcome for a household that was offered the treatment. This is different from the impact of actually participating in the program ("treatment on the treated" estimates) because of partial compliance. That is, not every household that was offered the treatment participated in the program; as detailed above, almost 30 percent of households invited to participate declined the offer. The treatment on the treated estimate is the parameter of interest when we want to capture the cost-effectiveness of the program, but it is biased by the self-selection of households into actually participating in the program or not. The intent-to-treat estimate indicates the causal impact of being assigned to participate in the program, and it is the focus of our analysis.

The intent-to-treat analysis is complemented by treatment-on-the-treated estimates obtained by estimating the impact of the program with an instrumental variable specification, instrumenting actual participation in the program with the random assignment. Table 2 reports these results for select outcomes. The signs and statistical significance of the coefficients are similar to those of coefficients obtained by regressing each outcome on the treatment indicator following specification (2) below (our main results, displayed in Table 6 through Table 11). Coefficients obtained by an instrumental variable specification, however, tend to be of a larger magnitude, confirming that the program had a strong effect on households which participated than the intent-to-treat measures indicate.

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 differences between households in treatment and control villages. We therefore adapt our regression specification to include variables controlling for the characteristics according to which treatment and control households differ at baseline, and to exploit the panel nature of our data:

$$Y_{ij} = \alpha + \beta(T_{ij} * P_{it}) + \delta P_{it} + \gamma X_{ij} + v_j + \varepsilon_{ijt} \qquad (2)$$

Where the subscript t indexes the waves of data (baseline, endline), P_{it} is a binary variable equal to 0if the data come from the baseline surveys and 1 if the data come from the endline survey, X_{ij} includes the baseline values of five control variables described in the next paragraph, and all other quantities are as in equation (1).We focus our analysis on long-term impacts, measured with baseline and endline waves. Typical impact evaluations focus on coefficient β, which shows the impact of the program above and beyond changes that happened to the control group (indicated by δ). In this analysis, for most outcomes of the program, β does not reach conventional levels of statistical significance but many δ coefficients are large and statistically significant, showing that, on average, both treatment and control households in the study area experienced important changes in their economic situation.

The specification in (2) also allows the assessment of interactions with other markets and interventions. To get at possibilities for substitution, we define Y as participation in competing programs or as income from alternative sources. We then quantify how the availability of the ultra-poor program affected other economic activities such as participation in the agricultural labor market.

Appendix Table 3 shows the average baseline values of characteristics of the treatment and control groups. At baseline, treatment and control households were similar on most demographic, consumption, income, health, occupation and housing characteristics. But despite the random assignment of villages into treatment and control groups, households living in treatment villages appear better off than control households along some dimensions. In Appendix Table 3 we consider 38 key variables, and find five dimensions for which treatment and control households differ significantly at baseline. These include the percentage of households that report holding some form of savings (51 percent of control households and nearly 60 percent of treatment households), participate in the NREG employment scheme (31 percent of control group households and 37.5 percent of treatment households), have outstanding loans (69 percent of control households against 74 percent of treatment households), have outstanding loans from self-help groups (47 percent of control households but 58 percent of treatment households), and own any animal (seven percent control households, versus 13 percent of treatment household own one or more heads of livestock or poultry). We control for the baseline value of these five characteristics in all analyses.

4. Results

This section describes impacts on the core outcomes in Table 6 through Table 11. The impact of the program on additional outcomes is reported in Appendix Tables.

Asset accumulation

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.

We find a relative increase in animal ownership among treatment households, but no impact of the program on the ownership of other assets. The first four columns of Table 5 analyze the impact of the program on the ownership of assets such as housing, land, livestock, and household and agricultural assets. The assets index is the principal components index of household durable goods owned by the household (such as television, table, or jewelry). The agricultural assets index is the principal components index of household agricultural durable goods (such as plough, tractor, or pump) and animals owned by the household. Ownership of household and agricultural assets did not significantly change between baseline and endline surveys, neither for control nor for treatment households. The finding of no impacts on ownership of assets is corroborated by qualitative insights suggesting that households were largely unable to diversify their asset base, even when asset holdings increased (Jawahar and Sengupta 2012).

The lack of impacts on asset ownership could be a sign that the program failed to even transfer a productive asset to participating households. Patterns of animal ownership, however, reflect the implementation of the program and confirm that this was not the case. Table 3 shows that the percentage of households reporting owning an animal increased between baseline and endline surveys for treatment households, but not for control households. Column 5 of Table 5 provides regression estimates of these changes: being assigned to participate in the program led to a 24-percentage point increase in the likelihood to own livestock, which includes animals such as buffaloes and goats that were provided by the program. As a check, we note that ownership of poultry did not increase, which is consistent with the fact that chicken and ducks were not available as grants from the program.

Animal ownership

Increasing animal ownership was a primary means for the program to support ultra-poor households. We should therefore see a clear impact of the program on the likelihood of owning animals in the endline survey. Instead, we see substantial drop out. While the coefficient showing the impact of the program on livestock ownership is statistically significant, the magnitude of the increase in the rate of livestock ownership is relatively low for a program based on the premise that animal rearing is economically profitable and generally desirable for ultra-poor households in the area.[12] Of the 405 households who actually participated in the program (576 lived in a village assigned to the treatment group), nearly 90 percent chose animals as the

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

asset they wish to receive from the program. In the endline, only 43 percent of the 362 households who chose livestock as their program asset still owned any animal. Consistent with the existence of dropout bias, the data suggest that some households in the treatment group sold the animal they received from the program (once the program implementation period ended and SKS stopped monitoring participants), used the revenue to pay off debt, and returned to wage labor.

Table 4 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 amount of land owned, which was larger for those who will own an animal at endline.

Panel B of Table 4 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 4, Panel B, indicates that fewer than 20 percent 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 this is under-reported, we worked with SKS to implement a follow-up survey of treatment households which chose buffalos or goats as their activity in the program but reported not owning an animal at the endline survey. In this follow-up survey, two-thirds of the valid responses indicate that the animal was sold, and eight percent indicated still owning and caring for the animal (the remaining households either lost their animals to illness or were leasing them out.)

Data on household indebtedness reinforce the argument that households that did not hold on to their animal actually sold it. Panel B of Table 4 indicates that, compared to households that held on to their animal, households that did not own animals in the endline wave were 19 percentage points less likely to have outstanding loans, reduced their number of loans outstanding, and had significantly lower average outstanding loan amounts.

This suggests that, given the lack of net positive impact of the program, some households may 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 onto 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.

Income and its composition

One of the basic changes that we observe is in the income of ultra-poor households. The average monthly per capita total income increased from Rs.312 (US$18.9 in PPP conversion) in the baseline to Rs.518 (US$31.3 in PPP conversion) in the endline, a 66 percent increase. Figure 1 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 in a similar fashion for treatment and control households.

This main finding holds when controlling for unbalanced characteristics of the households at baseline and village fixed effects. Table 6 reports the coefficients from a panel regression using the specification detailed in equation (2) above and the log of per capita monthly income. On average, both treatment and control households experienced a large and statistically significant increase in total income per capita. Over the 3 years between baseline and endline surveys, average household income per capita increased by 62 percent for households in the treatment group (Panel B) and 74 percent for households in the control group (Panel A).

The ultra-poor program itself, however, failed to raise households' total income per capita beyond income increases for households in the control group. Panel C analyzes the households in a cross-section at the endline. There, the average household in treatment villages had an income almost identical to that of the average household in control villages. This lack of net average impact does not mean that the program failed to create any impact. Figure 2 provides a visual summary of our argument. While the levels of and change in total income were not statistically different in 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.

We document with more precision the interaction of the ultra-poor program with other opportunities by defining the variable on the left-hand side of equation (2) as various components of household income.[13] Columns 3 and 6 of Table 6 confirm that the program was successful in raising income from livestock, but simultaneously caused a stagnation of agricultural labor income. In the long run, treatment households experienced a 97 percent increase in livestock income, as well as a nine percent decrease in income from agricultural labor (the coefficient is not statistically significantly different from zero).[14] The change in income from treatment households' re-optimizing away from agriculture labor to livestock rearing is most

[13] We also tested a seemingly unrelated regression specification to analyze the different sources of income. Results are qualitatively similar and are not reported here.

[14] We attribute the large change in other income for all households, reported in column 8, to measurement errors rather than an economically meaningful phenomenon.

visible in Panel C of Table 6: at endline, on average, the income from livestock of households in treatment village was 111 percent higher than that of households in control village, and the former's income from agriculture labor was 35 percentage points lower than the latter's.

Changes in the household's use of time corroborate the observed changes in income. Measures of time use presented in Table 7 include both adults and children to take into account the fact that the latter often help with tending animals and with household chores. The tableshows that aggregate measures of time spent in productive activities, in leisure, and doing chores did not change differently for treatment and control households. Detailed measures of time use over the past 24 hours, however, show that treatment households spent more time tending animals than control households, and less time doing agriculture labor. On average, between baseline and endline surveys, households participating in the program reduced the time they spent doing agricultural labor by 15 minutes while control households increased the time they devote to this activity by 44 minutes, leading to a net difference of 59 minutes per day.

Consumption

As described above, measures of food consumption likely suffer from measurement error. We describe the impact of the program on household consumption nonetheless since it is an important outcome. Figure 1 shows the density of total monthly per capita consumption for treatment and control households, and Figure 3 details consumption into food and non-food consumption. As the graphs indicate, the distribution of total and food expenditures shifted towards the left side, indicating a decrease over time consistent with substantial measurement error in the baseline. The decrease in total and food expenditures did not affect treatment and control households differently, but medical expenditures decreased significantly more for treatment households, making a marginal impact on non-food expenditures.

In Table 8 we report the results from estimating equation (2), with various measure of monthly per capita expenditures as dependent variables. The regression results corroborate that average total expenditures decreased between baseline and endline survey for all households, driven by measurement error causing a large decrease in food expenditures. The difference between the treatment and control households, however, was not statistically significant.

To limit the influence of measurement error, Panel C of Table 8 presents coefficients from a cross-sectional regression on endline data only. The coefficients on the binary variable indicating assignment to the ultra-poor program are all small and not statistically significant, showing the lack of average impact of the program on per-capita household expenditures.

Unlike other measures of expenditures, the data in Panel A of Table 8 suggest that medical expenditures declined sharply due to the program. This might in fact be a good sign. Assuming that treatment households were not more likely to feel in better health, to be too sick to work, nor to have consulted a doctor or gone to a hospital in the last year (Appendix Table 4), we cautiously interpret the decrease in medical expenditures as positive outcome consistent with the program's training of a local basic health responder in the village responsible for the basic diagnoses, referrals, and the provision of common medicines. The result, however, disappears in Panel C which relies on the endline cross-section only.

Saving and Borrowing

An important motivation for the program was to help ultra-poor households establish a microenterprise with a regular income flow that would help them later "graduate" into microfinance or other sustained source of support. In this section, we explore the impact of the program on the financial lives of the poor households.

Table 9 reports that the program had a strong impact on savings in the short run, as it required treatment households to save every week such that at the end of 18 months they had accumulated at least Rs. 800 to "graduate." As a result, immediately at the end of the program treatment households reported being more likely to save than control households, and reported savings balances 1.3 times that of control households, on average (data not shown).

These effects did not persist in the long run, however. 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. Qualitative insights confirmed 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 program was commonly used to repay outstanding debts.

The debt reduction is visible is our quantitative data for both treatment and control households, measured as (i) the likelihood to have outstanding loans, (ii) the number of outstanding loans, and (iii) the total amount of loans outstanding. 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 5 looks at the impact of the program on access to credit. It shows that, over the long run, sources of loans were not significantly different for treatment households than for control households. The program also did not significantly increase poor households' use of formal credit.

Households strongly reduced their use of moneylender loans – treatment households significantly more so than control households. The percentage of control households which had outstanding loans from moneylenders fell by 10 percentage points between the baseline and endline surveys, a large effect which represents about 20 percent of the baseline percentage of all households' borrowing from moneylenders. Treatment households were an additional 15 percentage points less likely to borrow from moneylenders, for a total effect representing one-third of the baseline percentage of households borrowing from moneylenders.

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. On the other hand, a long term goal was to create independent livelihoods and reduce reliance on public safety nets.

Table 10 shows no direct evidence of a substitution of the ultra-poor program with specific government safety net programs. While participation in most safety net schemes increased for all households between the baseline and endline surveys, ultra-poor households were not statistically significantly more or less likely to participate in any of them relative to control households. In the qualitative study, Jawahar and Sengupta (2012) make a similar note that "political competition" led to an increased awareness of, and participation in, government safety nets for all households in Andhra Pradesh. For this outcome, as for other outcomes of the ultra-poor program, context mattered greatly.

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). As noted in the introduction, 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. 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 percent to about 81 percent), the rate of increase was not statistically significantly different for treatment and control households (Table 10, column 1) and the amount earned from working in the scheme was similar for treatment and control households in the endline survey (Table 3).[15]

Heterogeneity in impacts

To assess heterogeneous impacts of the program, we divided the sample into subsamples of households based on land ownership, house ownership and livestock ownership at baseline. Table 11 shows the impact of the program on total monthly per capita income for each of these subgroups.

The results suggest that poorer households, as characterized by not owning livestock, land or a house prior to the program, tended to do worse in the program. Poorer households witnessed a larger decline in average income by the end of the study relative to their counterparts who owned assets at the start. While the statistical significance of these differences does not provide a compelling argument on its own, Jawahar and Sengupta's (2012) qualitative study also

15 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.

concludes that the impact of the program depended to a significant extent on the amount of experience with the livelihood activity chosen and the availability of support networks.

5. Conclusion

We report on an innovative asset transfer program aimed at ultra-poor households in rural India. 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).

The results are surprising: 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 in analysis of the endline survey, a preferred analysis given evidence of substantial measurement error in the baseline consumption data.)

We argue that the results are explained in large part by substitution with other economic activities. This is manifested as both substitution bias and dropout bias (Heckman et al. 2000). During the study period, wages in agricultural labor were rising steadily in the region, so that households in the control group were able to improve their economic conditions in parallel with households in the treatment group. It is left open whether the composition of support could have made a difference for households – especially the very poorest– which struggled to maintain their microenterprises, or whether there might have been greater impacts had the implementing organization maintained a presence in the villages after the program ended.

Taken as a whole, the study shows that the program helped households create new livelihoods as intended. At the same time, the study highlights the need to interpret evaluations in the context of the economic opportunities faced by families and their ability to re-optimize their livelihood strategies. Because of the substitution of economic activities, even a relatively well-implemented intervention delivered resources as intended but yielded no net average impact. In another economic setting, however, the exact same intervention targeted to an identical population might have generated very different levels of net impact.

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Table 1. Average Costs of the Program

	Cost in Rupees	Cost in US Dollars
Livelihoods asset	7,000	140
Capacity building	5,350	107
Implementation costs	4,700	94
Targeting costs	260	5
Stipend (working capital allowance)	550	11
Total cost per program participant	17,860	357

Notes: SKS NGO calculations, 2009. 50 Indian rupees = US$1.

Table 2. Impact of the Ultra-Poor Program, Instrumental Variable Specification

	Income			Time in agr. labor	Time tending animals	Total expend.	HH has loans?	HH saves?
	total	agr. labor	livestock					
Post*Treatment	-0.19	-0.50*	1.44***	-80**	18***	-0.07	-0.04	-0.05
	(0.13)	(0.26)	(0.23)	(34)	(5)	(0.08)	(0.08)	(0.07)
Post (0 if baseline, 1 if endline)	0.74***	0.21	-0.04	50***	-4**	-0.21***	-0.22***	0.09**
	(0.07)	(0.15)	(0.03)	(17)	(2)	(0.04)	(0.04)	(0.04)
Observations	1,976	1,991	1,909	1,973	1,992	2,000	2,000	2,000
R-squared	0.150	0.020	0.158	0.009	0.013	0.041	0.154	0.323
Mean of dep. var. at baseline	318	178	3.6	264	3.6	568	.714	.557

Notes: *** p<0.01, ** p<0.05, * p<0.1. Regressions in this table report coefficients from an instrumental variable specification, where actual participation in the program is instrumented by the random assignment to participate. All regressions include village-level fixed effects. Standard errors are clustered at the village level. Variables controlling for unbalanced characteristics of the sample (baseline values of whether the household saves, participates in EGS, receives a pension, has outstanding loan(s) from self-help groups, and own an animal) are included in the regressions but not shown. Income and consumption measures are the log of monthly per capita income or consumption (log of 1 + amount in 2007 Rupees; 1 USD ≈ 40 Rs). Time in agricultural labor and tending animal are measured in minutes in the last 24 hours. The means of the dependent variables at baseline are in level form. Livestock income includes income from irregular sales of animals.

Table 3. Summary Statistics for Control and Treatment Households

	Baseline		Endline		Percent change baseline-endline	
	C	T	C	T	C	T
Total income	312	313	520	516	67	65
Income from livestock	2.4	3.6	7.6	62.0	221	1,644
Income from agriculture labor	174	176	316	267	82	51
Income from non-agriculture labor	60	56	105	103	75	85
Total expenditures	587	543	496	471	-15	-13
Food expenditures	277	278	142	139	-49	-50
Non-food expenditures	310	265	355	333	15	25
Household has savings (percent)	51	59	60	65	18	9
Per capita savings balance	110	140	292	295	165	111
Household saves in SHG (percent)	47	58	58	55	22	-4
Household has outstanding loan (percent)	68	74	47	49	-32	-34
Per capita outstanding loan balance	2,479	3,041	1,447	1,531	-42	-50
Household borrows from moneylender (percent)	28	31	8	9	-72	-71
Household borrows from SHG (percent)	30	40	30	33	1	-16
Household sought/received government assets (percent)	3.3	4.3	9.9	9.3	203	115
Household sought/received government training (percent)	0	1	8	6	1,761	1,141
Household received goods from PDS (percent)	93	93	98	98	5	6
Household received BPL rationing (percent)	91	93	96	98	5	6
Household sought/received NREG work (percent)	31	37	82	80	167	116
Number of days household worked in NREG	n/a	n/a	32	35	n/a	n/a
Monthly per capita income from NREG	n/a	n/a	72	76	n/a	n/a
Household owns any animal(s) (percent)	7	13	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 data displayed in the table because of rounding. "C" indicates control households. "T" indicates treatment households. Income and expenditures are monthly per capita values. Savings in and borrowing from specific institutions is not conditional on the household having savings/borrowings. PDS and BPL rationing are government schemes providing basic goods at a subsidized price to poor households. The number of days worked in NREG and income from NREG are conditional on participating in the NREG scheme.

Table 4. Characteristics of Treatment Households, by Animal Ownership Status in Endline Survey

	Did not own animal in endline	Owned animal(s) in endline	p-value
Panel A. Baseline characteristics			
Household size	3.2	3.6	0.008
Average age of household members	30.4	29.6	0.512
Acres of land owned	0.38	0.56	0.042
Total monthly income per capita (Rs)	331	297	0.273
Owned any animal (percent)	12	16	0.267
Panel B. Endline characteristics			
Household sold animal in last 12 months (percent)	1	16	<0.001
Monthly income from sales of animals (Rs)	4	35	<0.001
Total monthly income per capita (Rs)	489	576	0.007
Monthly agriculture labor income per capita (Rs)	273	253	0.342
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)	30,417	41,099	0.449
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)	2,800	5,473	<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.

Table 5. Impact of the Ultra-Poor Program on Asset Ownership

	Household owns its house?	Acres of land owned	Assets index	Agr. assets index	Household owns livestock?	Household owns poultry?	Household owns plough?
Post*Treatment	-0.003	-0.172*	-0.059	0.210	0.242***	-0.002	-0.007
	(0.032)	(0.101)	(0.125)	(0.134)	(0.040)	(0.018)	(0.009)
Post (0 if baseline, 1 if endline)	0.139***	0.108	0.028	-0.131	-0.015	-0.015	-0.002
	(0.023)	(0.090)	(0.086)	(0.089)	(0.014)	(0.010)	(0.007)
Constant	0.653***	0.388***	-0.372***	-0.112**	0.037**	0.028***	0.009**
	(0.026)	(0.044)	(0.078)	(0.049)	(0.014)	(0.008)	(0.004)
Observations	1,995	1,956	1,989	1,977	1,992	1,978	1,994
R-squared	0.040	0.015	0.053	0.145	0.179	0.142	0.040
Mean of dep. var. at baseline	0.711	0.414	-0.007	0.016	0.069	0.050	0.013

Notes: *** p<0.01, ** p<0.05, * p<0.1. All regressions include village-level fixed effects. Standard errors are clustered at the village level. Regressions in which the dependent variable is a binary variable are run as linear probability models. Variables controlling for unbalanced characteristics of the sample (baseline values of whether the household saves, participates in EGS, receives a pension, has outstanding loan(s) from self-help groups, and own an animal) are included in the regressions but not shown. The assets index is the principal components index of 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. plough, tractor, pump, livestock).

Table 6. Impact of the Ultra-Poor Program on Income

	Total	Ag. self-empl.	Ag. labor	Non-ag. labor	Salaried empl.	Live-stock	Non-ag. self-empl.	Other sources
Panel A. Difference-in-difference								
Post*Treatment	-0.14	-0.05	-0.36*	0.30	-0.03	1.01***	0.03	-0.34*
	(0.09)	(0.16)	(0.19)	(0.29)	(0.09)	(0.17)	(0.10)	(0.20)
Post (0 if baseline, 1 if endline)	0.74***	-0.12	0.21	-0.08	0.10	-0.04	-0.27***	2.75***
	(0.07)	(0.12)	(0.15)	(0.21)	(0.07)	(0.03)	(0.07)	(0.14)
Constant	5.30***	0.56***	4.44***	1.85***	0.01	0.15**	0.38***	0.75***
	(0.05)	(0.08)	(0.11)	(0.14)	(0.05)	(0.07)	(0.06)	(0.09)
Observations	1,976	1,928	1,991	1,938	1,987	1,910	1,967	1,777
R-squared	0.152	0.012	0.016	0.010	0.012	0.129	0.025	0.382
Mean of dep. var. at baseline	318	15	178	57	7	4	37	38
Panel B. First difference, Treatment group only								
Post (0 if baseline, 1 if endline)	0.62***	-0.17	-0.09	0.19	0.06	0.97***	-0.25***	2.42***
	(0.05)	(0.10)	(0.13)	(0.19)	(0.06)	(0.16)	(0.07)	(0.14)
Constant	5.31***	0.42***	4.42***	1.55***	-0.06	0.21*	0.42***	0.80***
	(0.07)	(0.12)	(0.18)	(0.20)	(0.08)	(0.12)	(0.09)	(0.12)
Observations	1,090	1,064	1,100	1,075	1,100	1,031	1,091	965
R-squared	0.138	0.031	0.007	0.010	0.019	0.139	0.023	0.334
Mean of dep. var. at baseline	318	15	178	57	7	4	37	38
Panel C. Cross-section with endline data								
Treatment (1 if T group, 0 if C group)	-0.03	0.00	-0.35**	0.04	-0.10	1.11***	0.07*	-0.06
	(0.05)	(0.12)	(0.18)	(0.22)	(0.07)	(0.16)	(0.04)	(0.11)
Constant	6.13***	0.39***	4.73***	2.12***	0.16**	0.13	0.06*	3.50***
	(0.05)	(0.12)	(0.18)	(0.23)	(0.07)	(0.10)	(0.04)	(0.13)
Observations	985	968	995	953	995	941	998	940
R-squared	0.007	0.007	0.010	0.010	0.007	0.114	0.013	0.021
Mean of dep. var. at baseline (full sample)	313	13	175	58	7	3	37	38

Notes: *** p<0.01, ** p<0.05, * p<0.1. All regressions include village-level fixed effects. Standard errors are clustered at the village level. Variables controlling for unbalanced characteristics of the sample (baseline values of whether the household saves, participates in EGS, receives a pension, has outstanding loan(s) from self-help groups, and own an animal) are included in the regressions but not shown. The dependent variables are the log of the monthly per capita income from each source (log of 1 + amount in 2007 Rupees; 1 USD ≈ 40 Rs). The means of the dependent variables at baseline are in level form. 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.

Table 7. Impact of the Ultra-Poor Program on Time Use of Adults and Children

	Productive time	Leisure time	Time doing chores	Agr. Labor	Tending animals	Tending animals, if owns animals
Post*Treatment	-21	8	12	-59**	16***	50
	(25)	(5)	(13)	(24)	(4)	(43)
Post (0 if baseline, 1 if endline)	71***	-13***	-50***	44**	-6**	-72*
	(19)	(4)	(8)	(17)	(2)	(40)
Constant	309***	23***	226***	254***	7***	106***
	(12)	(3)	(7)	(13)	(2)	(29)
Observations	2,000	2,000	2,000	1,981	1,994	298
R-squared	0.032	0.014	0.053	0.017	0.028	0.076
Mean of dep. var. at baseline	326	26	226	272	7	53

Notes: *** p<0.01, ** p<0.05, * p<0.1. All regressions include village-level fixed effects. Standard errors are clustered at the village level. Variables controlling for unbalanced characteristics of the sample (baseline values of whether the household saves, participates in EGS, receives a pension, has outstanding loan(s) from self-help groups, and own an animal) are included in the regressions but not shown. Number of households owning animals: baseline = 73, endline = 186.Time is measured in minutes in the last 24 hours. 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. Animal ownership is measured in each wave.

Table 8. Impact of the Ultra-Poor Program on Expenditures

	Total	Food	Non-food	Non-food details: Energy	Tobacco/ Alcohol	Medical	Education	Other
Panel A. Difference-in-difference								
Post*Treatment	-0.05	0.01	-0.10	0.12	-0.10	-0.36***	-0.13	-0.04
	(0.06)	(0.05)	(0.08)	(0.09)	(0.15)	(0.12)	(0.11)	(0.09)
Post (0 if baseline, 1 if endline)	-0.21***	-0.71***	0.24***	0.68***	-0.95***	0.07	0.27***	0.33***
	(0.04)	(0.03)	(0.06)	(0.08)	(0.11)	(0.09)	(0.08)	(0.06)
Constant	6.05***	5.46***	5.12***	2.26***	1.13***	3.27***	1.00***	4.44***
	(0.04)	(0.03)	(0.04)	(0.05)	(0.07)	(0.07)	(0.09)	(0.05)
Observations	2,000	2,000	2,000	2,000	2,000	2,000	2,000	2,000
R-squared	0.041	0.286	0.024	0.189	0.148	0.015	0.021	0.039
Mean of dep. var. at baseline	568	279	290	25	19	55	13	179
Panel B First difference, Treatment group only								
Post (0 if baseline, 1 if endline)	-0.26***	-0.70***	0.14***	0.81***	-1.06***	-0.27***	0.15*	0.30***
	(0.04)	(0.04)	(0.05)	(0.05)	(0.10)	(0.08)	(0.08)	(0.06)
Constant	6.07***	5.43***	5.16***	2.29***	1.05***	3.41***	0.92***	4.46***
	(0.05)	(0.05)	(0.06)	(0.06)	(0.10)	(0.10)	(0.12)	(0.07)
Observations	1,105	1,105	1,105	1,105	1,105	1,105	1,105	1,105
R-squared	0.044	0.256	0.014	0.237	0.167	0.024	0.029	0.034
Mean of dep. var. at baseline	542	277	266	12	15	61	14	164
Panel C. Cross-section with endline data								
Treatment (1 if T group, 0 if C group)	-0.06	-0.02	-0.08	0.11	-0.08	-0.12	-0.13	-0.07
	(0.06)	(0.05)	(0.07)	(0.07)	(0.05)	(0.12)	(0.10)	(0.07)
Constant	5.84***	4.78***	5.35***	2.96***	0.32***	3.27***	1.33***	4.80***
	(0.05)	(0.04)	(0.06)	(0.08)	(0.08)	(0.11)	(0.10)	(0.06)
Observations	1,000	1,000	1,000	1,000	1,000	1,000	1,000	1,000
R-squared	-0.06	-0.02	-0.08	0.11	-0.08	-0.12	-0.13	-0.07
Mean of dep. var. at baseline (full sample)	563	277	286	24	19	54	13	176

Notes: *** p<0.01, ** p<0.05, * p<0.1. All regressions include village-level fixed effects. Standard errors are clustered at the village level. Variables controlling for unbalanced characteristics of the sample (baseline values of whether the household saves, participates in EGS, receives a pension, has outstanding loan(s) from self-help groups, and own an animal) are included in the regressions but not shown. The dependent variables are the log of the monthly per capita expenditures in each category (log of 1 + amount in 2007 Rupees; 1 USD ≈ 40 Rs). The means of the dependent variables at baseline are in level form. 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.

Table 9. Impact of the Ultra-Poor Program on Loans and Savings

	Household has outstanding loans?	Number of loans outstanding	Log (Amount of loan outstanding)	Household saves?	Log (Total savings balance)
Post*Treatment	-0.030	-0.09	-0.13	-0.039	-0.37
	(0.059)	(0.09)	(0.45)	(0.051)	(0.43)
Post (0 if baseline, 1 if endline)	-0.223***	-0.33***	-1.92***	0.090**	0.90***
	(0.044)	(0.07)	(0.34)	(0.038)	(0.34)
Constant	0.568***	0.69***	4.23***	0.227***	0.52***
	(0.025)	(0.04)	(0.19)	(0.020)	(0.14)
Observations	2,000	2,018	2,018	2,018	1,344
R-squared	0.155	0.134	0.132	0.322	0.219
Mean of dep. var. at baseline	0.714	1.0	2,825	0.557	119

Notes: *** p<0.01, ** p<0.05, * p<0.1. All regressions include village-level fixed effects. Standard errors are clustered at the village level. Regressions in which the dependent variable is a binary variable are run as linear probability models. Variables controlling for unbalanced characteristics of the sample (baseline values of whether the household saves, participates in EGS, receives a pension, has outstanding loan(s) from self-help groups, and own an animal) are included in the regressions but not shown. The amounts of loan outstanding and savings balance are in log form (log of 1 + amount in 2007 Rupees; 1 USD ≈ 40 Rs). The means of these dependent variables at baseline are in level form.

Table 10. Impact of the Ultra-Poor Program on the Use of Government Safety Nets

	Household sought or received the following:						Received goods from PDS	Received goods from BPL
	work from EGS	pension	govt. housing	govt. assets	govt. training	subsidized loans		
Post*Treatment	-0.080	-0.085	0.045	-0.011	-0.010	-0.010	-0.000	0.002
	(0.052)	(0.061)	(0.048)	(0.036)	(0.034)	(0.014)	(0.017)	(0.021)
Post (0 if baseline, 1 if endline)	0.510***	0.062	0.011	0.063**	0.070***	0.020*	0.054***	0.053***
	(0.035)	(0.043)	(0.033)	(0.026)	(0.025)	(0.011)	(0.013)	(0.017)
Constant	0.147***	0.292***	0.130***	0.032***	0.012	0.030***	0.878***	0.866***
	(0.019)	(0.019)	(0.018)	(0.012)	(0.009)	(0.008)	(0.013)	(0.015)
Observations	1,998	1,998	1,997	1,999	1,998	1,997	1,999	1,977
R-squared	0.456	0.261	0.008	0.020	0.044	0.006	0.038	0.036
Mean of dep. var. at baseline	0.344	0.643	0.168	0.039	0.005	0.023	0.926	0.918

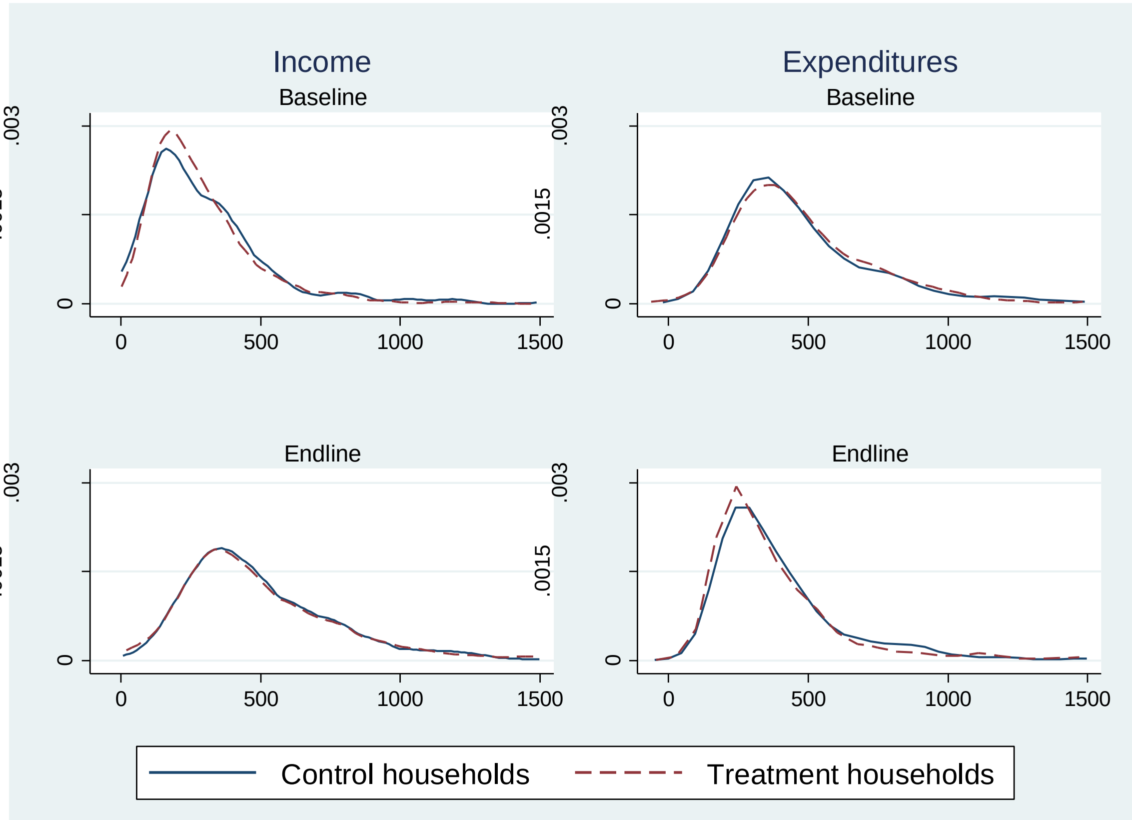
Notes: *** p<0.01, ** p<0.05, * p<0.1. All regressions include village-level fixed effects. Standard errors are clustered at the village level. Regressions in which the dependent variable is a binary variable are run as linear probability models. Variables controlling for unbalanced characteristics of the sample (baseline values of whether the household saves, participates in EGS, receives a pension, has outstanding loan(s) from self-help groups, and own an animal) 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.

Table 11. Impact of the Ultra-Poor Program on Total Monthly Per Capita Income, by Subgroups

Owned animals at baseline?	**No animals**	**Owned animals**
Post*Treatment	-0.15	0.19
	(0.09)	(0.23)
Post (0 if baseline; 1 if endline)	0.78***	0.28
	(0.07)	(0.20)
Constant	5.27***	5.32***
	(0.05)	(0.23)
Observations	1,772	204
R-squared	0.162	0.142
Mean of dep. var. at baseline	313	358
Owned land at baseline?	**No land**	**Owned land**
Post*Treatment	-0.21*	-0.08
	(0.12)	(0.10)
Post (0 if baseline; 1 if endline)	0.84***	0.59***
	(0.09)	(0.07)
Constant	5.18***	5.59***
	(0.07)	(0.08)
Observations	1,217	713
R-squared	0.168	0.176
Mean of dep. var. at baseline	311	323
Owned house at baseline?	**No house**	**Owned house**
Post*Treatment	-0.32**	-0.06
	(0.16)	(0.11)
Post (0 if baseline; 1 if endline)	0.85***	0.70***
	(0.12)	(0.09)
Constant	5.16***	5.34***
	(0.12)	(0.07)
Observations	571	1,397
R-squared	0.185	0.163
Mean of dep. var. at baseline	313	318

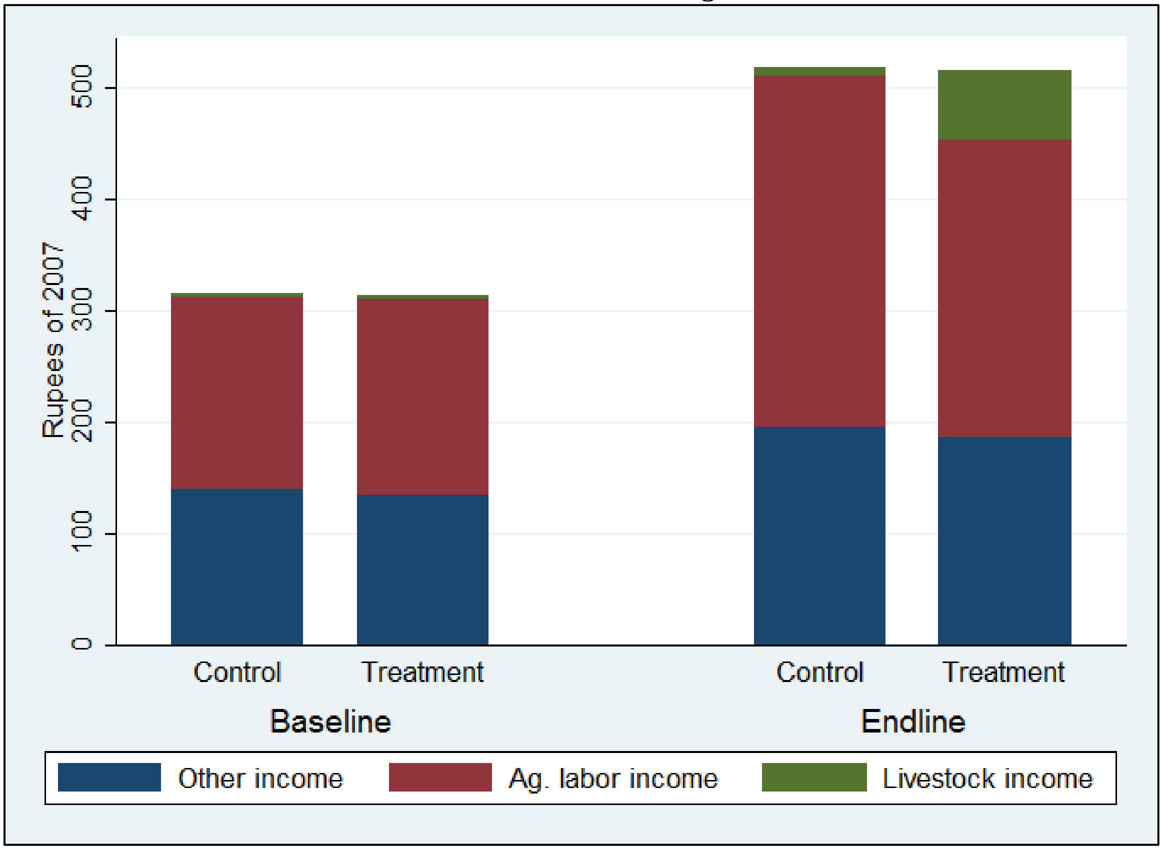
Notes: *** p<0.01, ** p<0.05, * p<0.1. All regressions include village-level fixed effects. Standard errors are clustered at the village level. Variables controlling for unbalanced characteristics of the sample (baseline values of whether the household saves, participates in EGS, receives a pension, has outstanding loan(s) from self-help groups, and own an animal) are included in the regressions but not shown. The dependent variable is the log of the total monthly per capita income (log of 1 + amount in 2007 Rupees; 1 USD ≈ 40 Rs). The means of the dependent variable at baseline are in level form.

Figure 1. Density of Monthly Per Capita Income and Expenditures



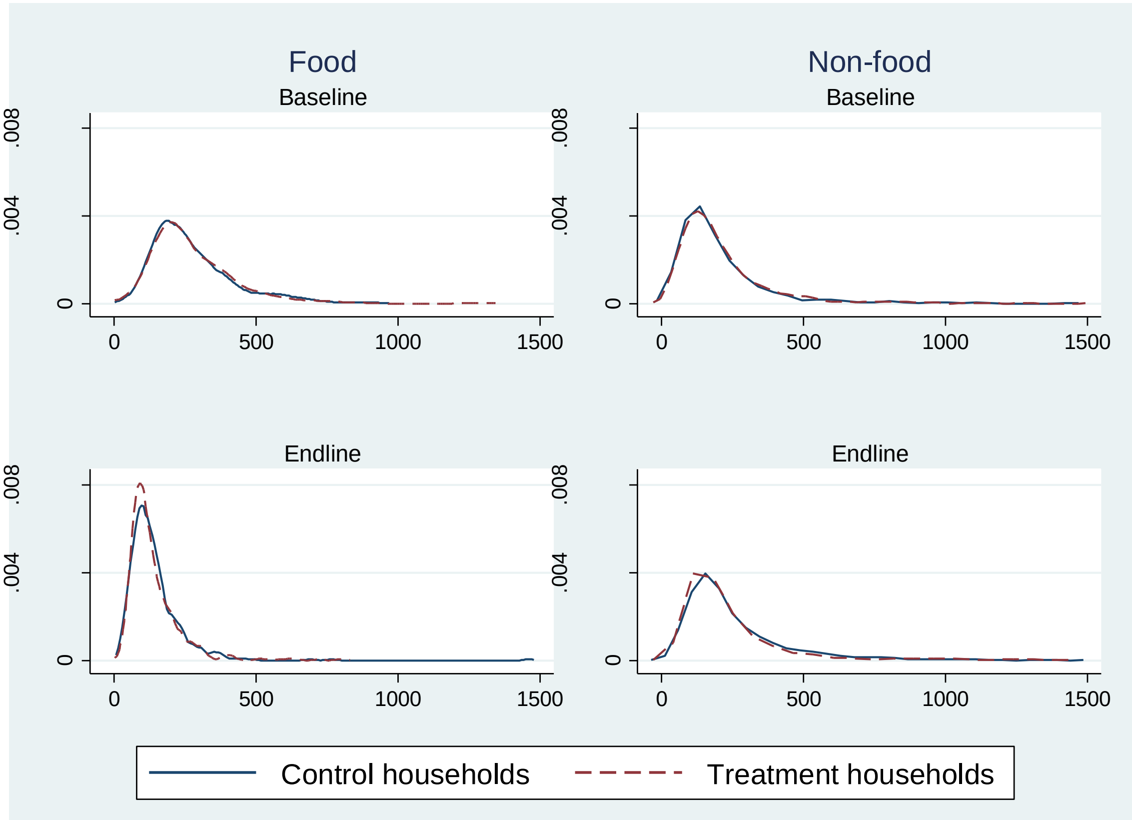
Graph shows distribution of per capita monthly total income and expenditures, truncated at Rs.1,500. Horizontal axes show amounts that are in Rupees of 2007.

Figure 2. 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.

Figure 3. Density of Monthly Per Capita Food and Non-food Expenditures



Graph shows distribution of per capita monthly food and non-food expenditures, truncated at Rs.1,500. Horizontal axes show amounts in Rupees of 2007.

Appendix Table 1. Summary statistics for attrition and non-attrition households.

	Non-attriters (1,011 hh)	Attriters (53 hh)	p-value
Individual-level data on household head			
Age (years)	46.4	51.9	0.012**
Literate (%)	4.8	11.3	0.034**
Marital status: Married (%)	18.3	18.9	0.920
Marital status: Unmarried (%)	1.0	0.0	0.467
Marital status: Divorced (%)	13.5	15.1	0.736
Marital status: Widow (%)	67.2	66.0	0.858
Household-level data			
Household assigned to the treatment group (%)	54.0	56.6	0.712
Number of household members	3.3	3.3	0.843
Average age of household members (years)	29.4	32.9	0.056*
Own their house (%)	71.4	66.0	0.402
House material: Pucca/good (%)	1.8	1.9	0.955
House material: Kuccha/medium (%)	80.1	81.1	0.857
House material: Thatched/bad (%)	18.1	17.0	0.837
Source of drinking water: Tap (%)	50.2	60.4	0.149
Source of drinking water: Well (%)	4.7	1.9	0.345
Source of drinking water: Tube well/hand pump (%)	43.8	37.7	0.389
Source of drinking water: Tank/reservoir (%)	1.3	0.0	0.406
Source of drinking water: Other (%)	0.1	0.0	0.819
Latrine is open air (%)	98.8	96.2	0.109
Any household member migrates for work (%)	15.9	12.8	0.563
Total land owned by hh (acres)	41.7	34.0	0.583
Total monthly income per capita (Rs)	316	262	0.220
Main source of income: Farming (%)	3.1	0.0	0.196
Main source of income: Livestock (%)	0.5	0.0	0.608
Main source of income: Non-ag. enterprise (%)	4.6	9.4	0.116
Main source of income: Wage labor (%)	91.8	90.6	0.753
Total monthly expenditures per capita (Rs)	568	471	0.273
Household has outstanding loans (%)	71.4	67.9	0.585
Household saves (%)	56	47.2	0.209
Sought or received work from EGS (%)	34.4	30.8	0.595
Sought or received a pension (%)	64.6	66	0.826
Sought or received government-subsidized loans (%)	2.3	3.8	0.483
Has an Antodaya, pink or white card (%)	92.7	94.3	0.649
Receives BPL rations (%)	91.9	94.2	0.546

*** p<0.01, ** p<0.05, * p<0.1; p-values are from t-tests. The table shows the mean of the indicated variables for households who were surveyed in both baseline and endline surveys ("non-attriters") and households who were surveyed in the baseline only ("attriters"). "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. BPL rations entitle families living below the poverty line to buying commodities at a government-subsidized price.

Appendix Table 2. Summary statistics for control and treatment households at baseline.

	Control group	N	Treatment group	N	p-value
Individual-level data on ultra-poor participant					
Age (years)	37.6	446	38.6	507	0.159
Literate (%)	4.3	446	4.7	508	0.731
Marital status: Married (%)	7.8	446	9.6	508	0.329
Marital status: Unmarried (%)	1.3	446	3.1	508	0.064*
Marital status: Divorced (%)	25.6	446	20.1	508	0.044**
Marital status: Widow (%)	65.2	446	67.1	508	0.541
Household-level data					
Number of household members	3.2	465	3.3	546	0.142
Average age of household members (years)	28.7	465	30.1	546	0.097*
Own their house (%)	72.6	463	70.4	544	0.449
House material: Pucca/good (%)	2.4	465	1.3	546	0.195
House material: Kuccha/medium (%)	78.9	465	81.1	546	0.381
House material: Thatched/bad (%)	18.7	465	17.6	546	0.643
Source of drinking water: Tap (%)	51.8	465	48.8	545	0.339
Source of drinking water: Well (%)	4.1	465	5.1	545	0.430
Source of drinking water: Tube well/hand pump (%)	43.4	465	44.0	545	0.849
Source of drinking water: Tank/reservoir (%)	0.4	465	2.0	545	0.026**
Source of drinking water: Other (%)	0.2	465	0.0	545	0.279
Latrine is open air (%)	98.7	462	98.9	544	0.776
Any household member migrates for work (%)	17.1	438	14.9	504	0.349
Total land owned by household (acres)	0.39	455	0.44	530	0.459
Total monthly income per capita (Rs)	315	461	316	544	0.938
Main source of income: Farming (%)	2.6	465	3.5	546	0.409
Main source of income: Livestock (%)	0.6	465	0.4	546	0.529
Main source of income: Non-agr. enterprise (%)	4.7	465	4.6	546	0.909
Main source of income: Wage labor (%)	92.0	465	91.6	546	0.787
Total monthly expenditures per capita (Rs)	594	465	545	546	0.222
Household has outstanding loans (%)	68.6	465	73.8	546	0.068*
Household saves (%)	51.0	465	60.3	546	0.003***
Sought or received work from EGS (%)	30.8	465	37.4	545	0.026**
Sought or received a pension (%)	60.4	465	68.1	545	0.011**
Sought or received government-subsidized loans (%)	2.8	465	1.8	546	0.306
Has an Antodaya, pink or white card (%)	92.5	464	92.9	546	0.808
Receives BPL rations (%)	91.0	456	92.6	544	0.345
Household owns one or more animal(s) (%)	7.3	463	13.0	540	0.004***
Experienced an event (shock) in last 12 months (%)	31.8	465	34.2	546	0.416

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. The table shows the mean of the indicated variables for households assigned to participate in the program ("treatment") and households assigned not to participate ("control"). p-values are obtained from t-tests. "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. BPL rations entitle families living below the poverty line to buying commodities at a government-subsidized price.

Appendix Table 3. Impact of the ultra-poor program on food security.

	Adults cut size or skip meals?	Adults do not eat for whole day?	Children under 16 cut size or skip meal?	All household members have enough food every day, all year?	Everyone in household eats two meals per day?
Post*Treatment	-0.039	-0.056	-0.050	-0.032	-0.014
	(0.051)	(0.044)	(0.039)	(0.045)	(0.026)
Post (0 if baseline, 1 if endline)	-0.187***	-0.023	0.120***	0.191***	0.020
	(0.040)	(0.033)	(0.030)	(0.031)	(0.020)
Constant	0.357***	0.174***	0.033	0.719***	0.928***
	(0.023)	(0.017)	(0.022)	(0.018)	(0.014)
Number of observations	1,572	1,553	1,067	1,964	1,980
R-squared	0.072	0.014	0.039	0.063	0.004
Mean of dep. var. at baseline	0.354	0.172	0.042	0.719	0.931

*** p<0.01, ** p<0.05, * p<0.1. All regressions include village-level fixed effects. Standard errors are clustered at the village level. All regressions are run as linear probability models. Variables controlling for unbalanced characteristics of the sample (baseline values of whether the household saves, participates in EGS, receives a pension, has outstanding loan(s) from self-help groups, and own an animal) are included in the regressions but not shown. Sample sizes are low in the baseline/endline analysis because of many missing values.

Appendix Table 4. Impact of the ultra-poor program on measures of physical health.

	Felt that physical health improved in last year?	Number of days unable to work because of illness	Any member went to the doctor/ hospital in last year?
Post*Treatment	-0.009	-0.400	-0.053
	(0.061)	(0.558)	(0.065)
Post (0 if baseline, 1 if endline)	-0.055	-0.924**	-0.083*
	(0.046)	(0.396)	(0.049)
Constant	0.223***	3.281***	0.506***
	(0.022)	(0.272)	(0.029)
Number of observations	1,982	1,958	1,836
R-squared	0.012	0.020	0.018
Mean of dep. var. at baseline	0.235	3.001	0.506

*** p<0.01, ** p<0.05, * p<0.1. All regressions include village-level fixed effects. Standard errors are clustered at the village level. Regressions in which the dependent variable is a binary variable are run as linear probability models. Variables controlling for unbalanced characteristics of the sample (baseline values of whether the household saves, participates in EGS, receives a pension, has outstanding loan(s) from self-help groups, and own an animal) are included in the regressions but not shown.

Appendix Table 5. Impact of the ultra-poor program on access to credit.

	Family	Com. bank	Grameen	SHG	Money-lender	Friend
Post*Treatment	-0.003	0.006	0.011	-0.041	-0.015	0.005
	(0.031)	(0.037)	(0.032)	(0.083)	(0.075)	(0.011)
Post (0 if baseline, 1 if endline)	-0.081***	0.031	-0.008	0.214***	-0.239***	-0.015**
	(0.024)	(0.030)	(0.024)	(0.068)	(0.050)	(0.007)
Constant	0.155***	0.047***	0.064***	0.131***	0.540***	0.019**
	(0.018)	(0.015)	(0.018)	(0.026)	(0.034)	(0.007)
Number of observations	1,183	1,183	1,183	1,183	1,183	1,183
R-squared	0.052	0.012	0.004	0.404	0.109	0.009
Mean of dep. var. at baseline	0.118	0.028	0.066	0.487	0.416	0.020
	Neighbor	Shop-keeper	Co-operative	MFI	Other	
Post*Treatment	0.001	0.005	-0.014	-0.012	0.007	
	(0.029)	(0.013)	(0.038)	(0.017)	(0.012)	
Post (0 if baseline, 1 if endline)	-0.086***	-0.012**	0.060**	0.041***	-0.004	
	(0.022)	(0.006)	(0.025)	(0.014)	(0.008)	
Constant	0.145***	0.016*	0.017	0.002	0.004	
	(0.023)	(0.009)	(0.011)	(0.009)	(0.005)	
Number of observations	1,183	1,183	1,183	1,183	1,183	
R-squared	0.033	0.010	0.028	0.028	0.005	
Mean of dep. var. at baseline	0.123	0.015	0.011	0.003	0.015	

*** p<0.01, ** p<0.05, * p<0.1. All regressions include village-level fixed effects. Standard errors are clustered at the village level. All regressions are run as linear probability models. Variables controlling for unbalanced characteristics of the sample (baseline values of whether the household saves, participates in EGS, receives a pension, has outstanding loan(s) from self-help groups, and own an animal) are included in the regressions but not shown. The dependent variables are binary variables set to 1 if any household member has one or more outstanding loans from that source, conditional on having one or more outstanding loans.