LABOR MARKETS AND POVERTY IN VILLAGE ECONOMIES*

ORIANA BANDIERA
ROBIN BURGESS
NARAYAN DAS
SELIM GULESCI
IMRAN RASUL
MUNSHI SULAIMAN

We study how women's choices over labor activities in village economies correlate with poverty and whether enabling the poorest women to take on the activities of their richer counterparts can set them on a sustainable trajectory out of poverty. To do this we conduct a large-scale randomized control trial, covering over 21,000 households in 1,309 villages surveyed four times over a seven-year period, to evaluate a nationwide program in Bangladesh that transfers livestock assets and skills to the poorest women. At baseline, the poorest women mostly engage in low return and seasonal casual wage labor while wealthier women solely engage in livestock rearing. The program enables poor women to start engaging in livestock rearing, increasing their aggregate labor supply and earnings. This leads to asset accumulation (livestock, land, and business assets) and poverty reduction, both sustained after four and seven years. These gains do not crowd out the livestock businesses of noneligible households while the wages these receive for casual jobs increase

*Earlier drafts were circulated under the titles "Can Basic Entrepreneurship Transform the Economic Lives of the Poor?" and "The Misallocation of Labor in Village Economies." We thank all BRAC staff and especially Sir Fazle Abed, Mushtaque Chowdhury, Mahabub Hossain, W. M. H. Jaim, Imran Matin, Anna Minj, Muhammad Musa, and Rabeya Yasmin for their collaborative efforts in this project. We thank Wahiduddin Mahmud and Hafeez Rahman of the IGC Bangladesh office and Clare Balboni for outstanding research assistance. We thank the editor, Larry Katz, three anonymous referees, and Arun Advani, Orazio Attanasio, Abhijit Banerjee, Timothy Besley, Gharad Bryan, Francisco Buera, Anne Case, Arun Chandrasekhar, Jonathan Colmer, Angus Deaton, Dave Donaldson, Esther Duflo, Pascaline Dupas, Greg Fischer, Doug Gollin, Chang-Tai Hsieh, Dean Karlan, Eliana La Ferrara, Costas Meghir, Ted Miguel, Mushfiq Mobarak, Benjamin Olken, Michael Peters, Steve Pischke, Mark Rosenzweig, Esteban Rossi-Hansberg, Juan Pablo Rud, Jeremy Shapiro, Chris Udry, Chris Woodruff, and numerous seminar and conference participants for useful comments. This project was financed by BRAC and its CFPR-TUP donors, including DFID, AusAID, CIDA and NOVIB, OXFAM-AMERICA. This document is an output from research funding by the DFID as part of the iiG. Support was also provided by the IGC. The views expressed are not necessarily those of DFID. Rasul acknowledges funding by the ESRC Centre for the Microeconomic Analysis of Public Policy at IFS (grant number RES-544-28-5001). All errors remain our own.

The Quarterly Journal of Economics (2017), 1–60. doi:10.1093/qje/qjx003.
Advance Access publication on March 20, 2017.

as the poor reduce their labor supply. Our results show that (i) the poor are able to take on the work activities of the nonpoor but face barriers to doing so, and, (ii) one-off interventions that remove these barriers lead to sustainable poverty reduction. *JEL Codes*: J22, O12.

I. Introduction

As of today, around a billion people are deemed to be living in extreme poverty. Since labor is their primary endowment, attempts to lift them out of poverty require us to understand the link between poverty and labor markets and whether policy interventions that move them into higher return labor activities can set them on a sustainable trajectory out of poverty. To shed light on the issue, we combine a detailed labor survey that tracks over 21,000 households, drawn from the entire wealth distribution in 1,309 rural Bangladeshi villages, four times over a seven-year period, with the randomized evaluation of the nationwide roll-out of a program that transfers assets and skills to the poorest women in these villages.

Our survey gathers detailed data on hours worked, days worked, and earnings for each labor activity of each household member. We find that at baseline, the choice of labor activity for women is limited as they allocate over 80% of hours worked to three activities: maid services, agricultural labor, and livestock rearing. These labor activities are strongly correlated with poverty: poor women engage mostly in casual wage labor as maids and agricultural laborers, while wealthier women specialize in livestock rearing. The main differences between these activities are that the returns to casual wage labor are lower and work is only available on some days of the year. Consequently, we find that poor women work two months less each year than wealthier women. These findings are consistent with evidence in other settings where the rural landless poor are employed in low-pay and insecure activities (Bardhan 1984a; Dreze 1988; Dreze and Sen 1991; Rose 2001; Kaur 2015).[1]

1. According to the Indian National Sample Survey (NSS), 46% of the female rural workforce have agricultural wage employment as their main occupation. As is also the case in our setting for maids and agricultural laborers, 98% of agricultural wage employment is through casual employment typified by spot markets (Kaur 2015). On the fact that such agricultural wage employment is only available on some days of the year, Khandker and Mahmud (2012) and Bryan,

The key question we examine is whether enabling the poorest women to take on the same work activities as the better-off women in their villages can set them on a sustainable path out of poverty. To answer this question we evaluate BRAC's Targeting the Ultra-Poor (TUP) program that provides a one-off transfer of assets and skills to the poorest women with the aim of instigating occupational change. Intuitively, if the poor face barriers to entering high-return work activities and this is what keeps them in poverty, we expect program beneficiaries to change their labor allocation and escape poverty once such barriers are removed. Because the intervention is bundled, however, we cannot measure the separate relevance of credit constraints and skills constraints, both of which could be relaxed by the program.[2] Of course, the one-off asset transfer mechanically reduces poverty in the very short run because it makes beneficiaries instantly wealthier and they can consume that wealth. The question of interest here is whether such one-off asset and skills transfers set the poorest households on a sustainable trajectory out of poverty, where their consumption and asset holdings keep increasing long after the one-off transfer, as they are able to alter their labor allocation permanently.

To evaluate the causal impacts of the program, we randomly assign 40 BRAC branch offices serving 1,309 villages to either treatment or control for four years. A participatory wealth ranking is conducted before baseline in both treatment and control villages, followed by the application of TUP eligibility criteria by BRAC officers. This process classifies households into four groups in all villages: ultra-poor, near-poor, middle-class, and upper-class. Ultra-poor households, who account for 6% of the population, are eligible to receive the program; other households are ineligible. We survey all the ultra-poor and near-poor households and a 10% sample of the middle- and upper-class households. Our design is

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Chowdhury, and Mobarak (2014) document how lean seasons between planting and harvesting are observed throughout South Asia and Sub-Saharan Africa and are characterized by a lack of demand for casual wage labor and higher grain prices as food becomes scarce. As a result, households face extreme poverty and food insecurity.

2. Indeed, this is a bundled, multifaceted program that also provides some consumption support in the first 40 weeks after asset transfers, as well as health support and training on legal, social, and political rights across the two years of the program. As discussed throughout, we do not aim to separate out the impacts of each component.

thus a partial population experiment (Moffitt 2001) that allows us to identify indirect treatment effects on ineligible households at different points of the wealth distribution as well as distributional effects, namely, the extent to which the ultra-poor close the gap with the next wealth class. This is relevant because the program aims to induce occupational change among ultra-poor women to take on the same work activities as richer women (livestock rearing). It is thus natural to trace through the economic impacts on richer women as they face increased competition in output markets for livestock produce and in markets related to inputs into livestock rearing.

We find the program transforms the labor activity choices of ultra-poor women. Four years after the transfer, they devote 217% more hours to livestock rearing, 17% fewer hours to agricultural labor, and 26% fewer hours to maid services relative to their counterparts in control villages. Aggregating across labor activities, there is a net positive effect on hours worked and days worked of 17% and 22%, respectively, suggesting that poor women had idle work capacity and that the program enables them to put it to a productive use by taking on livestock rearing activities. Overall, the results demonstrate that the poor are able to take on the labor activities of the nonpoor but face barriers to doing so, which the asset and skills transfers from the program relax.

The reallocation of labor supply across work activities by the ultra-poor leads their earnings to be 21% higher than their counterparts in control villages, and the probability of being below the $1.25 extreme poverty line is 14% lower. Per capita consumption expenditure is 11% higher, and the value of household durables is 57% higher, with both effects being larger after four years than after two. In line with this, earnings from livestock rearing are not entirely consumed but are used to save and invest further in productive assets. Four years posttransfer, the ultra-poor in treatment villages have more than four times the amount of savings and they are more likely to receive and give loans to other households. Moreover, the value of cows they own is over twice as large (net of the value of the asset transfer itself) and they also accumulate business assets such as livestock sheds, rickshaws, vans, pumps, and trees whose value is over 159% larger than for the controls over the same period.[3] More important, they gain access

3. Land is the key asset in the densely populated rural areas of Bangladesh we study. Laboring for others is necessary, in part because the ultra-poor do not have

to land, which is the key productive asset in these villages. Relative to controls, treatment households are 139% more likely to rent land and 45% more likely to own land, and the value of their landholdings is 82% higher.

Because individuals are likely to differ in their ability to raise livestock and manage a small business, the effect of the program is likely to be heterogeneous. The scale of our evaluation, covering more than 6,000 ultra-poor households, allows us to estimate quantile treatment effects (QTEs). These reveal a large degree of heterogeneity: the effect on the 95th centile of consumption, for instance, is 10 times larger than the effect on the 5th centile and differences for savings and productive assets are even larger.

The effects of the program on the labor allocation of the beneficiaries raise the possibility that ineligible households residing in treatment villages might be affected through general equilibrium effects, such as changes in livestock produce prices. Our estimates of the indirect treatment effects on ineligibles, however, show no evidence that the livestock rearing businesses of richer women are crowded out by the entrance of the poor into this activity: they neither reduce their labor supply nor experience a significant reduction in earnings. A likely explanation for these muted impacts is that even after four years, the ultra-poor still constitute a relatively small share of the market overall. In contrast, we do find general equilibrium impacts on the casual wage labor activities that the ultra-poor dominated at baseline: after four years, the agricultural and maid wages paid to ineligible women in treatment villages are 9% and 11% higher than in control. At the same time, the hours the ineligible devote to these work activities are lower, so their earnings are unaffected.

The partial population experiment design also allows us to estimate treatment effects of the program on the gap between wealth classes and so sheds light on the distributional consequences of the intervention. This exercise reveals that the ultra-poor close the gap with the near-poor in consumption expenditures and household assets, while on other dimensions they actually overtake this group and end up with four times the level of savings and twice the value of productive assets. The program thus has powerful distributional impacts, both between wealth classes as well as

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access to land and livestock rearing is a viable alternative and in part because it does not require a land input (Bardhan 1984a).

within the ultra-poor, as highlighted by the quantile treatment effect estimates.

At a combined cost of $1,120 in purchasing power parity (PPP) terms per household, both the asset and skill components constitute large transfers benchmarked against the baseline wealth and human capital of the ultra-poor.[4] We can use our estimates to benchmark the program's benefits against its costs. Under the assumption that the estimated consumption benefits at year 4 are repeated over 20 years, the program has an average benefit/cost ratio of 3.2. The estimated internal rate of return (IRR) of the program is between 16% and 22%, depending on the assumed opportunity cost of time that must be taken into account as the program causes the ultra-poor's labor supply to increase overall.

The final part of the analysis sheds light on long-term impacts of the intervention. To do so we surveyed the same households again in 2014, seven years after the intervention began. Although 20% of the control group residing in 49% of the control villages had been treated by then, we are able to derive a lower bound for the effect of the program after seven years, and compute other bounds by using our QTE estimates to create counterfactuals for the treated controls. This comparison reveals that changes after seven years are at least as large as the four-year impacts. Although these results must be interpreted with caution because our counterfactuals might be imperfect, a major trend break would be needed to reverse the conclusion that the original beneficiaries are escaping poverty at a steady rate.

Overall the results show that one-time asset and skills transfers to the ultra-poor enable them to overcome barriers to accessing high-return labor activities. These reallocations of labor supply across work activities lead to increases in their consumption and a diversification of their asset base, especially through accessing land, and this process sets them on a sustained trajectory out of poverty.

By the end of our study in 2014, the program had reached 360,000 households in Bangladesh containing 1.2 million individuals, and it has subsequently been piloted in other countries (Banerjee et al. 2015a). We compare our results for Bangladesh to those from six pilot studies in Ethiopia, Ghana, Honduras, India, Pakistan, and Peru (Banerjee et al. 2015a). Across 10 dimensions

4. Throughout the article we stick to the convention of reporting values in $ PPP terms.

covering consumption, food security, assets, financial inclusion, labor supply, income, physical health, mental health, political awareness, and women's empowerment, we find the three-year results for these pilot studies are strikingly similar to our four-year results. The fact that the program has positive effects across such a wide range of outcomes increases confidence that it has a profound effect on the lives of ultra-poor women. The comparison of our findings to those of other pilots suggests that specifically promoting occupational change is effective in different contexts. This lends support to the argument that the program may be able to be scaled-up in different contexts with different implementing partners to achieve sizable and sustainable improvements in outcomes for the poorest.

The article is organized as follows. Section II describes key features of rural labor markets underlying our analysis. Section III describes the TUP intervention, our data, and research design. Section IV documents treatment effects on the ultra-poor. Section V looks across the wealth distribution to provide estimates of indirect treatment effects on ineligible households and the extent to which the ultra-poor close the gap with the near-poor. Section VI presents a cost-benefit analysis and estimates internal rates of return. Section VII examines the trajectories of beneficiaries after seven-years. Section VIII concludes by discussing the broader implications of our study.

II. Labor Markets and Poverty at Baseline

II.A. Poverty and Wealth Classes

We study labor markets in 1,309 villages in Bangladesh's 13 poorest districts. These districts were chosen by BRAC to implement the TUP program based on food security maps of the World Food Program. Our sample is drawn from two randomly selected subdistricts in each district, containing 40 BRAC branches that serve the 1,309 villages where the evaluation takes place.[5]

To construct our sample we first conducted a census of the 99,775 households in the 1,309 villages. To draw a sample for

5. There is a concentration of study sites in the northern part of the country. This is because this is the poorest and most vulnerable region, often referred to as the monga or famine region (Bryan, Chowdhury, and Mobarak 2014). Our evaluation is representative of the areas in which the nationwide TUP program was scaled up after 2007.

the baseline survey, we combine these data with information on household wealth, derived from a participatory wealth-ranking organized by BRAC in each village. This exercise places all households into one of several wealth bins corresponding to the poor, the middle class, and the upper class. Before randomization, BRAC officers use inclusion and exclusion criteria to further subdivide the poorer households into the ultra-poor, who are eligible for the TUP program, and the near-poor, who are not. The four wealth classes account for 6%, 22%, 59%, and 14% of the village populations, respectively (Table I). We survey almost all ultra-poor and near-poor households, and a 10% random sample of households from higher wealth classes, at baseline in 2007 and then at follow-ups in 2009, 2011, and 2014. Overall the sample covers over 21,000 households in 1,309 villages, of which over 6,700 are ultra-poor. Our research design allows us to study the program's (i) intent-to-treat effect on the ultra-poor, where the number of ultra-poor households that we track allows us to further estimate quantile treatment effects to shed light on heterogeneous impacts of the program among the ultra-poor; (ii) general equilibrium and distributional impacts on near-poor, middle-class, and upper-class households.

The top two panels of Table I confirm that the participatory ranking exercise is successful in identifying the poorest households: 53% of the households identified as ultra-poor are below the $1.25 a day poverty line, and the corresponding figures for the near-poor, middle, and upper classes are 49%, 37%, and 12%. Due to BRAC's targeting strategy, the primary woman is the sole earner in 41% of the ultra-poor households, whereas this only occurs in 25%, 14%, and 12% of near-poor, middle, and upper-class households. Illiteracy is also much higher for ultra-poor women: a staggering 93% of them are illiterate compared with 83%, 74%, and 49% in the other three wealth classes. These data confirm that the ultra-poor are severely disadvantaged relative to their wealthier counterparts in the same village. They also confirm that these village economies have a significant fraction of middle- and upper-class households living below the extreme poverty line.

Looking across household assets, savings, livestock, land, and business assets, the distinguishing feature of the ultra-poor is that they are largely assetless. As we look across the columns of Table I all these variables are larger for wealthier households.

The value of cows owned by the ultra-poor is only 2.2% of the value owned by the upper classes, and the corresponding figure for

TABLE I

HOUSEHOLD CHARACTERISTICS AND ASSET HOLDINGS, BY WEALTH CLASS

	(1) Ultra-poor	(2) Near-poor	(3) Middle class	(4) Upper class
Household characteristics				
Share of population in this wealth class	0.061	0.219	0.585	0.135
Primary female is the sole earner	0.409	0.250	0.142	0.120
Primary female is illiterate	0.929	0.832	0.736	0.489
Consumption and assets				
Household is below the $1.25 a day poverty line	0.530	0.493	0.373	0.121
Consumption expenditure (per adult equivalent)	627.8	645.1	759.5	1,234.2
Household assets [$]	36.5	68.1	279.9	1,663.4
Household savings [$]	7.9	22.1	84.5	481.9
Household receives loans	0.191	0.393	0.498	0.433
Household gives loans	0.012	0.018	0.030	0.067
Business assets (excl. livestock and land) [$]	22.9	54.4	286.1	1,569.8
Livestock				
Household owns cows	0.055	0.154	0.469	0.733
Household owns goats	0.092	0.142	0.300	0.425
Value of cows [$]	33.8	120.2	633.8	1,559.1
Value of goats [$]	7.97	12.8	39.8	71.3
Household rents cows for rearing	0.070	0.148	0.118	0.030
Household rents goats for rearing	0.111	0.157	0.102	0.021
Land				
Household owns land	0.066	0.107	0.487	0.911
Value of land owned [$]	200.0	491.2	6,789.6	40,125.1
Household rents land for cultivation	0.060	0.143	0.276	0.168
Number of sample households	6,732	6,743	6,328	2,036

Notes. All statistics are constructed using baseline household data from both treatment and control villages. Wealth classes are based on the participatory rural assessment (PRA) exercise: the ultra-poor are ranked in the bottom wealth bins (fourth if four bins are used, fifth if five are used) and meet the program eligibility criteria, the near-poor are ranked in the bottom wealth bins and do not meet the program eligibility criteria, the middle class are ranked in the middle wealth bins (second and third if four are used; second, third, and fourth if five are used), and the upper-classes are those ranked in the top bin. The number of sample households in each wealth class at baseline is reported at the foot of the table. The poverty line threshold used is $1.25 per person per day. Consumption expenditure is defined as total household expenditure over the previous year divided by adult equivalents in the household. The adult equivalence scale gives weight 0.5 to each child younger than 10. The expenditure items covered are food (both purchased and produced), fuel, cosmetics, entertainment, transportation, utilities, clothing, footwear, utensils, textiles, dowries, education, charity, and legal expenses. Household assets include jewelry, sarees, radios, televisions, mobile phones, furniture, and so on. Household savings refers to the value of savings held at home, at any bank, at any MFI, and with saving guards. Loans are from both formal and informal sources. Business assets include pumps, livestock sheds, trees, rickshaws and others. All monetary amounts are PPP-adjusted US$ terms, set at 2007 prices and deflated using CPI published by Bangladesh Bank. In 2007, US$1 = 18.46 TK PPP.

goats is 11.1%. This gap in the value of livestock is driven both by the ultra-poor being much less likely to own livestock (particularly cows) and then conditional on owning livestock being more likely to own goats (the average value of which is close to $54 in PPP terms) rather than cows (the average value of which is $542). As households get richer they focus on accumulating cows (not goats) with the former accounting for 96% of the value of livestock owned by upper-class households. Therefore, as the comparison of cow and goat values in Table I shows, cows are the key livestock asset in these village economies. Table I also shows that rental markets do not equalize access to productive assets: only 7% of the poor in our sample rent in cows from other households. This is likely because of various transaction costs associated with renting out livestock to others, which have been shown to be relevant in rural labor markets (Shaban 1987; Foster and Rosenzweig 1994).[6]

The final panel of Table I shows that the poor are much less likely to own land than are wealthier households. Only 7% of ultra-poor households own land at baseline compared with 11%, 49%, and 91% for near-poor, middle-class, and upper-class households. In addition only a small fraction of the ultra-poor, 6%, rent land for cultivation. The majority of ultra-poor households are therefore landless, and the value of land they own is tiny compared to middle-class and upper-class households. Land is the asset that most clearly differentiates rich from poor households in these villages.

What is also clear from Table I is that inequality in asset holdings across the village wealth distribution is much more marked than inequality in consumption. Average consumption expenditure per adult equivalent for ultra-poor households is 51% of that for upper class households. The corresponding figures for household assets, savings, business assets, value of cows, value of goats,

6. Even though wealthier households can in principle gain by renting livestock to the poor to take advantage of their lower labor costs, the transaction costs from doing so are high for at least three reasons: (i) the ultra-poor lack experience of livestock rearing: for centuries they have been landless and engaged in casual wage labor activities; (ii) the quality of labor inputs in livestock rearing are critical: there can be large variations in the productivity of livestock due to differences in feeding, veterinary, and other practices; (iii) the economic opportunities of wealthier households means they face high opportunity costs of supervising or training other households when rearing livestock. More generally, Shaban (1987) and Foster and Rosenzweig (1994) provide evidence of the importance of moral hazard in labor contracts in rural India.

and value of land owned are 2.2%, 1.6%, 1.5%, 2.2%, 11%, and 0.5%. The upper classes in the villages are distinguished mainly by owning more assets, particularly agricultural land. The ultra-poor, in contrast, have negligible asset holdings.

These characteristics of ultra-poor women combined with the fact that they have a median age of 40 and an average of one dependent child below the age of 10 imply that they are likely to be captive in these village labor markets. Migration to other labor markets in towns and cities is unlikely to be a possibility for the majority of ultra-poor women. In common with many ultra-poor women around the world they have to choose from the work activities on offer within the villages where they currently reside.[7]

II.B. Labor Markets

Our survey collects information on all labor activities, for each household member, during the previous year. For each activity, we ask whether the individual was self-employed or hired by a third party as a wage laborer, the number of hours worked a day, the number of days worked per year, wage rates, and total earnings. We collect data related to the entire year because employment in casual wage jobs, especially those in agriculture, is irregular so that a shorter time frame (days, weeks) is likely to severely mismeasure aggregate hours devoted to these activities. Because the program targets the primary woman in ultra-poor households, defined as the head's spouse or the female head, we focus the analysis on women's labor market activities.[8]

Figure I, Panel A begins to describe the working lives of women in rural Bangladesh. It identifies the main labor activities in these villages by showing the share of women's work hours devoted to various work activities in each of the 40 BRAC branches our sample covers. The figure reveals that the set of labor activities that women engage in is extremely limited. Around 80% of women's labor hours are devoted to three activities: casual jobs

7. Later we present experimental evidence that the program did not lead to differential attrition in treatment versus control villages, which is consistent with this hypothesis. Cultural barriers also imply that migration, in particular seasonal migration, is typically practised by males in Bangladesh (Bryan, Chowdhury, and Mobarak 2014).

8. Bardhan (1984b) and Foster and Rosenzweig (1996) document a marked differentiation in agricultural tasks by gender, which is also observed in our setting.

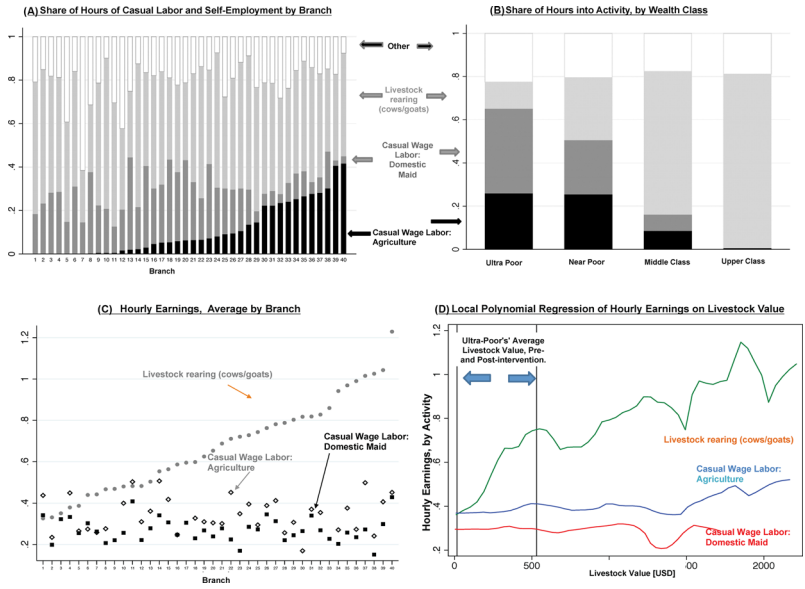

FIGURE I

Features of Rural Labor Markets for Women

All figures are derived using the baseline household survey and present statistics on the three main occupations: domestic maid (red), agricultural labor (blue), livestock rearing (green), and other (white) (for a full-color figure, please see the online version of this article). Panel A shows the share of hours devoted to the different occupations by BRAC branch, ordered by the share of hours devoted to casual labor in agriculture. Panel B shows the share of hours devoted to the different labor market activities by wealth class. Panel C shows the hourly returns to the different occupations by BRAC branch, ordered by returns to livestock rearing. For each activity, earnings per hour are calculated as total earnings from that activity divided by total hours worked in the activity, both defined over the year prior to the baseline survey for individuals who had positive hours and nonmissing earnings in that activity. Panel D graphs local polynomial regressions of the hourly returns to activities by the value of livestock owned. The vertical lines correspond to the average value of livestock owned by the ultra-poor before and after the intervention. All monetary amounts are PPP-adjusted US$ terms, set at 2007 prices and deflated using CPI published by Bangladesh Bank. In 2007, US$1 = 18.46 TK PPP.

in agriculture, casual jobs as domestic maids, and livestock rearing. The first two are activities where unskilled labor is the only input and women are hired daily without any guarantee of future employment.[9] For the third, women are self-employed, working

9. In our data 99% (96%) of women working in agricultural wage labor (as maids) report being hired and paid daily through spot contracts. This is also

with cows and goats to generate income through the sale of milk, meat, manure, and young calves. The key difference between these two sets of activities is that the latter requires a capital input. It is also likely that livestock rearing requires higher levels of skills.[10] Figure I, Panel A shows that while livestock rearing is present in all labor markets, either agricultural or maid labor tends to dominate in a particular location. Hence in most villages within a given BRAC branch, women effectively choose between two labor activities—agricultural/maid labor and livestock rearing.[11]

Figure I, Panel B presents hours of work broken out by wealth class and activity to investigate whether there is a correlation between labor market activities and poverty. The figure demonstrates that there is a pronounced shift toward livestock rearing as we move up the wealth distribution. Ultra-poor and near-poor women engage predominantly in casual wage labor, although ultra-poor women are distinguished from near-poor women by relying almost exclusively on unskilled casual labor, which requires no capital input and where they rely on others to employ them, primarily as agricultural laborers or domestic maids. In contrast, women from middle- and upper-class households are predominantly engaged in livestock rearing. Across all four wealth classes, these three activities account for 80% of hours worked.[12]

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what Kaur (2015) observes in India using NSS data. We do not therefore observe coexistence of temporary and permanent wage labor contracts (Eswaran and Kotwal 1985).

10. Expertise is needed to (i) give beef cows, dairy cows, and goats the right diets; (ii) be able to detect diseases and know when to contact the vet; (iii) know about vaccines and when they need to be given; (iv) be able to work with artificial insemination services (for cows); (v) be able to construct livestock sheds and keep them clean.

11. Due to the geographical separation of casual wage labor activities described in Figure I, Panel A, agricultural work and maid work are rarely combined to make a full-time job. Only 10% of women who report any wage activity are engaged in both casual agricultural labor and domestic maid work. We also note that 43% of poor women generate small amounts of income from poultry; however, the returns from such activities are far lower than even for casual wage labor. Following the earlier literature that has argued for buffer stock motivations of animal ownership (Rosenzweig and Wolpin 1993), we consider poultry holdings as a form of illiquid savings rather than representing a key choice over labor market activities.

12. The remaining 20% of hours is distributed across several other activities that typically account for less than 1% of hours each (where work on the household's own land is counted as own cultivation not agricultural labor). The activities

Figure I, Panel C graphs the hourly returns for the three main work activities averaged over all individuals with non missing earnings and positive hours in each of the three activities. We compute simple averages at the BRAC branch level. Hourly returns for casual jobs are equal to the hourly wage. To compute average hourly earnings for livestock rearing, we divide yearly profits (revenues minus input costs) by total hours devoted to livestock rearing over the year. Two things are apparent from this plot. The first is that the average returns for those engaged in livestock rearing are higher than those for casual wage labor in nearly all rural labor markets in our sample. Table A.I in the Online Appendix shows that, at the village level, hourly earnings in livestock rearing are \$0.72 an hour, more than double the hourly earnings for agricultural wage labor (\$0.34 an hour) and maid work (\$0.27 an hour). The choice over labor activities however depends on the marginal returns to labor in each. For competitive casual wage labor markets, that are governed by spot contracts without any future employment guarantee, the hourly wage closely matches the MP_L. For capital-intensive activities such as livestock rearing, measuring the MP_L requires knowing the production function for how capital and labor are combined. Assuming a Cobb-Douglas technology, MP_L is proportional to AP_L, with the constant of proportionality being labor's share of income. Given the measured returns across activities, we note that for the average branch, the MP_L in livestock rearing is larger than the MP_L in agricultural (maid) work as long as the labor share is larger than 0.48 (0.37). Macro-wide estimates from developing countries typically lie in the range of 0.65–0.80 (Gollin 2002).[13]

The second observation from Figure I, Panel C is that returns to casual wage labor are uniform across space, whereas returns to livestock rearing vary strongly across space. The uniformity of returns to casual labor across geography reflects the fact that there is an abundant supply of low-skilled women willing to work

that account for more than 1% for the ultra-poor are begging (6%), tailoring (4%), casual day labor outside agriculture (4%), land cultivation (1%). For the near-poor they are begging (3%), tailoring (3%), casual day labor outside agriculture (3%), land cultivation (4%). For the middle-classes they are tailoring (3%), land cultivation (4%). For the upper classes they are tailoring (1%), teacher (1%), land cultivation (5%).

13. A body of field experiments examining the returns to capital in developing country contexts find that these returns are higher than the returns to labor (De Mel, McKenzie, and Woodruff 2008; Blattman, Fiala, and Martinez 2014).

in these work activities and wages offered in village spot markets tend to fall within narrow bands (Kaur 2015). In contrast, returns to livestock rearing vary according to location-specific features such as linkages to urban markets and trade networks (Donaldson 2015).

Figure I exposes the puzzle at the heart of our study—why do the poor not allocate their labor to the activity with the highest return? One possibility is that the observed cross-sectional returns to activities might not represent the returns available to the poor if they engaged in them. The differences could be due to differences in innate ability correlated with poverty or to increasing returns to scale. To explore the latter, Figure I, Panel D graphs a local polynomial regression of hourly returns on the value of livestock owned by households. While the estimated returns need to be interpreted cautiously given that livestock holdings are endogenous, across the whole distribution the returns to livestock rearing are higher than for casual wage labor activities (that themselves do not vary with livestock ownership as expected). The vertical bars on Figure I, Panel D indicate the average value of livestock owned by the ultra-poor before and after the TUP program intervention we evaluate. Over this range, the returns to livestock rearing are higher than for both forms of casual wage labor, and these returns are also clearly rising with livestock value, indicating there might be increasing returns to livestock rearing.[14] Evaluating the TUP program allows us to assess whether differences in returns can be explained by differences in innate ability or reflect multiple barriers that the poor face in accessing labor activities that they are otherwise able to engage in.

Besides having different hourly returns and capital requirements, the two types of work activities also exhibit a different distribution of hours worked across days of the year. Table A.I shows that the average woman engaged in casual agricultural labor works in this activity for only 127 days of the year; engagement in domestic maid work is for only 167 days a year. In contrast, women engaged in livestock rearing work almost every day of the year. However, conditional on working, women employed in casual wage activities work many more hours each day: 7.6 daily

14. That there are increasing returns to livestock rearing is in line with evidence from other settings in rural South Asia (Anagol, Etang, and Karlan 2014; Attanasio and Augsburg 2014).

TABLE II
LABOR MARKET ACTIVITIES OF WOMEN, BY WEALTH CLASS

Means (std. dev.)	(1) Ultra-poor	(2) Near-poor	(3) Middle class	(4) Upper class
Engaged in any income-generating activity	0.843	0.810	0.863	0.903
Total hours worked in the past year	991	769	553	502
	(894)	(812)	(596)	(502)
Total days worked in the past year	252	265	302	325
	(137)	(142)	(123)	(103)
Casual wage labor				
Hours devoted to agricultural labor	258	196	47.7	3.05
	(533)	(467)	(236)	(49.9)
Hours devoted to domestic maid	388	193	41.9	0.648
	(708)	(516)	(251)	(22.7)
Capital-intensive activities:				
Hours devoted to livestock rearing (cows/goats)	121	221	366	404
	(265)	(341)	(390)	(370)
Number of sample households	6,732	6,743	6,328	2,036

Notes. All statistics are constructed using baseline household data from both treatment and control villages. Wealth classes are based on the participatory rural assessment (PRA) exercise: the ultra-poor are ranked in the bottom wealth bins (fourth if four bins are used, fifth if five are used) and meet the program eligibility criteria, the near-poor are ranked in the bottom wealth bins and do not meet the program eligibility criteria, the middle-class are ranked in the middle wealth bins (second and third if four are used; second, third, and fourth if five are used), and the upper classes are those ranked in the top bin. The number of households in each wealth class at baseline is reported at the bottom of the table. Engagement in any income-generating activity covers all potential activities.

hours for casual agricultural work, 7.0 for maid work, versus 1.8 daily hours for livestock rearing.[15]

Table II shows the implications of low demand for casual labor on the distribution of hours worked across wealth classes: over the course of a year, poor women bunch their work into fewer days of the year than wealthier women, but work more hours in

15. Absent large fixed costs of daily labor supply or concave daily costs of work effort, women should prefer to smooth their labor supply. The observed bunching of labor supply for casual wage activities into fewer days of the year is indicative of constrained or low aggregate demand for both forms of casual wage labor. This is not surprising for agricultural wage labor because of inherent seasonality in labor demand including the well documented preharvest lean season in the agricultural cycle in Bangladesh, during which the demand for labor is almost nonexistent (Khandker and Mahmud 2012; Bryan, Chowdhury, and Mobarak 2014).

the year overall. This bunching is driven by the concentration of poor women's labor supply into casual wage activities that are only available for less than half the year. In contrast, wealthier women specialize in livestock rearing, enabling them to smooth their labor supply over the year.

Taken together, the evidence suggests a clear correlation between poverty and labor market activities with poor women allocating most of their labor to low-return, irregular, casual jobs and richer women specializing in high-return, regular, livestock rearing. The key question is whether poor women would be better off engaging in the same activities as their wealthier counterparts but face barriers in accessing capital or skills that keep them in poverty. The beneficiaries' response to the TUP program, which simultaneously relaxes these capital and skills barriers, sheds light on this question. If ultra-poor women prefer employment in casual jobs they will sell (or rent out) the asset without changing their labor market choices. If they prefer livestock rearing but face asset and/or skills related barriers to engaging in such activities, they will retain the asset and work with it once barriers are removed.

III. Intervention and Research Design

III.A. The Intervention: TUP

The TUP program is designed and implemented by BRAC to reach the very poorest women in rural Bangladesh who are not targeted by other forms of assistance. Prerandomization, eligible households are selected by BRAC officers from the list of poor households produced by a village participatory wealth ranking.[16] To qualify for the program, the household needs to have an able adult woman present, not be borrowing from a microfinance organization or receiving transfers from government antipoverty

16. For the participatory wealth-ranking exercise, villages are asked to rank all households into wealth bins and reach a consensus on the wealth class of each household. People who own sufficient amounts of land; have a salaried job; live in a tin or paddy sheafhouse; own cows, goats, or other livestock; or own a power tiller, rice mill, and so on, are considered wealthy. People who are landless and who own nothing outside their homestead; work as casual laborers, small traders, or beg; do not own any livestock or assets; and live in straw houses are considered to be poor (BRAC 2004). Alatas et al. (2012) show that compared to proxy means tests, participatory methods result in higher satisfaction and greater legitimacy.

programs, and meet three out of five inclusion criteria.[17] Eligibility is not conditional on participating in other BRAC activities.

The program targets the leading woman in eligible ultra-poor households. Women are presented with a menu of assets, each of which can be used in an income-generating activity. These assets include livestock and those relevant for small-scale retail operations, tree nurseries, and vegetable growing. Each asset is offered with a package of complementary training and support.

Of those households identified as ultra-poor at the outset, 86% eventually receive an asset. The other 14% either cease to meet the eligibility criteria when transfers are implemented or choose not to take up the program.[18] All the offered asset bundles are similarly valued at $560 in PPP terms. The scale of asset transfers corresponds to a near doubling of baseline wealth for the ultra-poor, values that are far higher than households could borrow through informal credit markets. All eligible women chose one of the six available livestock asset bundles from the asset menu and 91% of them chose an asset bundle containing at least one cow. Before the intervention, the value of livestock owned by the 47% of ultra-poor households with either a cow or a goat at baseline is just $49.70.

Assets are typically transferred one month after choices are first made. Eligibles are encouraged by BRAC to retain the transferred asset for two years, after which they can liquidate it. Thus, whether the livestock asset is retained or liquidated by the time of our four-year follow-up is itself an outcome of interest that ultimately determines whether the program affects the long-run allocation of time across work activities or just contributes to a potentially short-run increase in household welfare.

The associated support and training package is also valued at around $560 per beneficiary. This component comprises initial classroom training at BRAC regional headquarters followed by regular assistance through home visits. A livestock specialist visits eligibles every one to two months for the first year of the

17. The eligibility criteria are (i) total land owned including homestead land does not exceed 10 decimals; (ii) there is no adult male income earner in the household; (iii) adult women in the household work outside the homestead; (iv) school-aged children work; and (v) the household has no productive assets.

18. It is likely that most did not receive assets because they had become ineligible, not because of take-up refusal. For example, compared with those receiving assets, those who did not were twice as wealthy and more likely to own land.

program, and BRAC program officers provide weekly visits for two years after transfer. As the ultra-poor have limited experience with large livestock (particularly cows), this assistance is designed to cover the life cycle of livestock. Ultimately, this training component is intended to mitigate earnings risks from working with livestock and to increase the overall return to livestock rearing.[19]

The program also provides a subsistence allowance to eligible women for the first 40 weeks after the asset transfer to help smooth any short-run earnings fluctuation due to adjustments across work activities. This allowance ends 15 months before our first follow-up and is therefore not part of the earnings measures reported. To empower ultra-poor women along noneconomic dimensions, the program also provides health support and training on legal, social, and political rights. The program also sets up committees made up of village elites which offer support to program recipients and deal with any conflicts and problems they encounter. Finally, the program encourages saving with BRAC during the program and borrowing from BRAC microfinance at the end of the program, but neither is a precondition to obtain the asset-training bundle.

The program thus represents a bundle of asset and skills transfers. Given the economic circumstances and life experiences of the ultra-poor, there are good theoretical reasons these components need to be offered together. The strong focus on continual training and support over a two-year period is one way the TUP program differs from previous asset transfer programs (Dreze 1990; Ashley, Holden, and Bazeley 1999). In short, the program can potentially change a number of dimensions of poor women's lives. Transferring assets has a large impact on their wealth and the program provides key asset and skill inputs needed to take on labor activities engaged in by richer women. Continued support during the period of learning can further improve their chances of being successful in taking on these activities. It may also make women more assured and confident that they can take on work activities other than casual labor (including those who

19. Training is designed to help women maintain the animals' health, maximize the animals' productivity through best practices relating to feed and water, learn how to best inseminate animals to produce offspring and milk, rear calves, and bring produce to market. The training is sufficiently long-lasting to enable women to learn how to rear livestock through their calving cycle and across seasons.

are not encouraged by the program) and may change cultural attitudes toward these women. We evaluate the full impacts of the bundled version of the program, and thus do not aim to identify specific constraints on occupational change that the program may be operating through.

III.B. Research Design

The TUP program evaluation sample comes from among the 13 poorest districts in rural Bangladesh, as described earlier. In most cases we randomly selected two subdistricts (*upazilas*) from each district and within each subdistrict we randomly assigned one BRAC branch office to be treated and one to be held as a control.[20] All villages within an 8 km radius of a treated BRAC branch receive the program in 2007, and villages in control branches receive it after 2011. We randomize at the branch rather than village level to mitigate spillovers between treatment and control villages either through markets or through program officers. We are evaluating a scaled version of the TUP program: by 2014, this had reached over 360,000 households containing 1.2 million individuals.[21]

For the purpose of the evaluation, the participatory wealth ranking is conducted in both treatment and control areas and BRAC officers identify eligible ultra-poor women in identical ways in both areas. To avoid anticipation effects, information about the availability of the program and eligibility status is not made public until program operations begin in a given area (in mid-2007 in treatment areas, after 2011 in control areas) and the participatory

20. The average subdistrict has an area of approximately 250 square kilometers (97 square miles) and constitutes the lowest level of regional division within Bangladesh with administrative power and elected members. For each district located in the poorer northern region we randomly select two subdistricts, and for each district located in the rest of the country we randomly select one subdistrict, restricting the draw to subdistricts containing more than one BRAC branch office. For the one district (Kishoreganj) that did not have subdistricts with more than one BRAC branch office, we randomly choose one treatment and one control branch without stratifying by subdistrict.

21. A variant of the program where the poor have to repay the cost of the asset transferred to BRAC had reached an additional 1.1 million households containing 3.6 million members by 2014 (BRAC 2015).The TUP program started in 2002, and there was a second wave in 2004. The scale of these waves was smaller than the wave that started in 2007 and these were used in part to inform the design of the scale-up that took place in 2007. The 2002–2006 period therefore involved significant piloting and experimentation (Hossain and Matin 2004).

wealth ranking is presented as a part of regular BRAC activities rather than associated with a specific program.

Table A.II in the Online Appendix provides evidence on whether the characteristics of the ultra-poor are balanced between treatment and control villages. For each outcome considered, we report means and standard deviations in treatment and control villages (columns (1) and (2)), the p-value on a test of equality of means (column (3)) and the normalized difference of means (column (4)). For each family of outcomes we also report the average standardized difference following Kling, Liebman, and Katz (2007). The samples are well balanced on outcomes: only one out of 22 tests yields a p-value below .05, and we cannot reject the null hypothesis of equal means for any of the average standardized differences. Furthermore, column (4) shows that all normalized differences are smaller than one sixth of the combined sample variation, suggesting linear regression methods are unlikely to be sensitive to specification changes (Imbens and Wooldridge 2009).

Over the four years from baseline to endline, 15% of ultra-poor households attrit, a rate comparable to other asset transfer program evaluations (Banerjee et al. 2015a). Table A.III in the Online Appendix estimates the probability of not attriting as a function of treatment status and baseline work activities. This shows (i) attrition rates do not differ between treatment and control villages; (ii) women engaged in livestock rearing are more likely to be surveyed in all three waves; (iii) crucially, there is no differential attrition by baseline work activities between treatment and control individuals: the coefficients on interaction terms between treatment status and activity choice at baseline are all precisely estimated and close to 0. To ease comparability our working sample is based on those households that are tracked in both follow-ups, covering 6,732 ultra-poor households.

IV. Treatment Effects on the Ultra-Poor

We evaluate the impacts of the TUP program on individual and household level outcomes exploiting the experimental variation caused by the random assignment of villages to treatment or control. We estimate the following difference-in-difference specification:

$$(1) \quad y_{idt} = \alpha + \sum_{t=1}^{2} \beta_t (W_t \times T_i) + \gamma T_i + \sum_{t=1}^{2} \delta_t W_t + \eta_d + \varepsilon_{idt},$$

where y_{idt} is the outcome of interest for individual/household i in subdistrict d at time t, where time periods refer to the 2007 baseline ($t = 0$), 2009 midline ($t = 1$), and 2011 endline ($t = 2$). W_t are survey wave indicators. $T_i = 1$ if individual i lives in a treated community and 0 otherwise. η_d are subdistrict fixed effects and are included to improve efficiency because the randomization is stratified by subdistrict. The error term ε_{idt} is clustered by BRAC branch, the unit of randomization. All monetary values are deflated to 2007 prices using the Bangladesh Bank's rural CPI estimates and converted into $ PPP.

β_t identifies the intent-to-treat impact of the program on ultra-poor individual/household i under the twin identifying assumption of random assignment and no spillovers between treatment and control villages. This estimate compares changes in outcomes among ultra-poor residing in treated villages before and after intervention, to changes among counterfactual ultra-poor in control villages in the same subdistrict. As discussed earlier, the ultra-poor are identified in identical ways in treatment and control locations prerandomization. To benchmark the magnitude of the effects we report the four year effects in percentage of the control mean in the same period throughout. Specification (1) controls for time-varying factors common to ultra-poor in treatment and control villages, and for all time-invariant heterogeneity within subdistrict. Tables A.VA and A.VB in the Online Appendix probe robustness to using an analysis of covariance (ANCOVA) specification both pooling the survey waves and running each separately.[22] Table A.VI in the Online Appendix probes robustness to different inference methods that correct for the small number of clusters: the Young (2016) degrees of freedom correction and the Cameron, Gelbach, and Miller (2008) wild-bootstrap method. All results are quantitatively and qualitatively robust to both sets of changes.

The subsections below test the impact of the program at each step of the causal chain that links choices over labor activities to

22. Table A.VA in the Online Appendix reports the estimates of $y_{id} = \alpha + \beta T_i + \mu y_i^0 + \eta_d + \varepsilon_{id}$ run separately on the cross-section of eligible households in 2009 and 2011, where y_i^0 is the baseline (2007) value of y_i and all other variables are as defined above. Table A.VB reports the estimates of $y_{idt} = \sum_{t=1}^{2} \beta_t (W_t \times T_i) + \sum_{t=1}^{2} \nu_t (W_t \times y_i^0) + \sum_{t=1}^{2} \delta_t W_t + \eta_d + \varepsilon_{idt}$, where $t \in [1, 2]$ (1=2009, 2=2011), y_i^0 is the baseline (2007) value of y_i and all other variables are as defined above.

earnings, consumption, savings and investment. The comparison between two and four year effects reveals whether the effects become stronger over time, which is important for understanding whether the program sets the ultra-poor on a sustainable trajectory out of poverty.

IV.A. Labor Supply and Earnings

Table III shows program impacts on labor supply (Panel A) and earnings (Panel B) for the three main labor activities for women in Bangladeshi villages. Column (1) of Panel A shows that the program succeeds in its aim to induce ultra-poor women to take up livestock rearing: four years after baseline ultra-poor women allocate 415 more hours to livestock rearing each year, a 217% increase relative to controls in the same time period. This corresponds to ultra-poor women working 172 days in this activity per annum representing an increase of 181% relative to controls (column (2)). Comparing two- and four-year impacts, we note that the change in hours devoted to livestock rearing is immediate, in line with the fact that beneficiaries move into livestock rearing as soon as they receive the assets. The increase represents 1.14 more hours a day, which matches well with the time allocation to this activity observed at baseline (Table II).

In short, livestock rearing has become a central element in the working lives of ultra-poor women. The findings further indicate that beneficiaries continue to own livestock instead of liquidating it for consumption, despite the fact that the value of the transfer is equal to one year's worth of consumption for the average adult. They also indicate that beneficiaries are able to maintain the asset once assistance is removed as the effects are sustained after the two-year mark.

Columns (3)–(6) show evidence that ultra-poor women start pulling out of casual wage labor activities. Although the change in hours devoted to livestock rearing is immediate, the effect on casual labor hours is gradual. The reduction in agricultural labor (46 hours, 17% relative to controls) is not precisely estimated, while the fall in maid hours increases in magnitude between two and four years and is significant only after four years (117 hours, 26% relative to controls). This is consistent with the fact that the wage rate for agricultural labor is higher than that for maid work (Figure I, Panel C and D and Table A.I in the Online Appendix). Overall, ultra-poor women are dropping some of the least

TABLE III

TREATMENT EFFECTS ON THE LABOR SUPPLY AND EARNINGS OF ULTRA-POOR WOMEN

	Livestock		Agriculture		Maid		All activities	
	(1) Hours	(2) Days	(3) Hours	(4) Days	(5) Hours	(6) Days	(7) Hours	(8) Days
Panel A: Labor supply								
Program impact after 2 years	488***	205.5***	−42.3	−3.54	−57.4	−8.45	341***	72.4***
	(30.7)	(11.1)	(53.0)	(7.02)	(42.9)	(5.88)	(67.9)	(10.0)
Program impact after 4 years	415***	171.6***	−46.2	−4.77	−117**	−16.77***	206***	61.1***
	(38.9)	(10.9)	(42.7)	(5.43)	(45.0)	(5.82)	(73.0)	(12.5)
Control mean at 4-year follow-up	191.00	94.76	278.14	35.40	447.05	63.97	1,217.00	277.40
4-year impact: % change	217%	181%	−17%	−13.5%	−26%	−26%	17%	22%
2-year impact = 4-year impact [p-value]	0.111	0.023	0.930	0.831	0.125	0.125	0.080	0.179
Adjusted R-squared	0.335	0.367	0.184	0.183	0.067	0.061	0.072	0.069
Number of ultra-poor women	6,732	6,732	6,732	6,732	6,732	6,732	6,732	6,732
Number of observations	20,196	20,196	20,196	20,196	20,196	20,196	20,196	20,196
(clusters)	(40)	(40)	(40)	(40)	(40)	(40)	(40)	(40)

TABLE III
(CONTINUED)

	Livestock	Agriculture		Maid		All activities
	(9) Earnings	(10) Wage	(11) Earnings	(12) Wage	(13) Earnings	(14) Earnings
Panel B: Earnings						
Program impact after 2 years	80***	0.028	−9.99	0.034	−11.48	62.3**
	(14.0)	(0.021)	(13.98)	(0.022)	(11.36)	(30.17)
Program impact after 4 years	115***	0.053**	−3.89	0.074***	−25.25**	87.8***
	(14.1)	(0.024)	(13.97)	(0.019)	(11.57)	(28.58)
Control mean at 4-year follow-up	18.48	0.441	96.44	0.354	112.84	410.92
4-year impact: % change	16%	12%	−4%	21%	−22%	21%
2-year impact = 4-year impact [p-value]	0.049	0.219	0.701	0.080	0.205	0.455
Adjusted R-squared	0.127	0.486	0.178	0.241	0.095	0.088
Number of ultra-poor women	6,732	6,732	6,732	6,732	6,732	6,732
Number of observations	20,120	5,227	19,883	5,833	19,796	20,135
(clusters)	(40)	(40)	(40)	(40)	(40)	(40)

Notes. Sample: ultra-poor women (std. err. in parentheses), clustered by BRAC branch area. *** (**) (*) indicates significance at the 1% (5%) (10%) level. Intent-to-treat estimates are reported based on a difference-in-difference specification estimated using OLS. This regresses the outcome of interest for woman i in village v in survey wave t on a constant, a dummy for whether the woman resides in a treated village, dummies for the two follow-up survey waves (two and four years postintervention), the interaction between the treatment assignment dummy and each survey wave dummy, and a set of strata (subdistrict) fixed effects. The coefficients shown are those on the treatment-survey wave interaction terms. Standard errors are clustered by BRAC branch area. All outcomes are measured at the individual level (for the ultra-poor woman in the household) and defined for the year prior to survey date. We report the mean of each dependent variable as measured at baseline in treated villages. In all columns we report the p-value on the null hypothesis that the two- and four-year ITT impacts are equal. The number of ultra-poor is the number of eligible women observed at baseline and in both follow-up survey waves. All monetary amounts are PPP-adjusted US$ terms, set at 2007 prices and deflated using CPI published by Bangladesh Bank. In 2007, US$1 = 18.46 TK PPP.

attractive casual labor hours but still hold on to the majority even as they significantly increase livestock hours.[23]

Aggregating across labor activities, columns (7) and (8) show that four years postintervention total hours worked increases by 206 (17%) and days worked a year increase by 61 (22% more than in control). This suggests that the poor had idle labor capacity at baseline that they were able to successfully combine with the bundled asset-skills transfer as a result of the program. This improvement in the regularity of employment is a key labor market impact of the program. At baseline ultra-poor women, like many of the poorest women in rural parts of the developing world, were captive in occupations at the bottom of the employment ladder using labor, their only endowment. Significantly, demand for this labor was highly irregular. The opportunity to engage in livestock rearing that the program provides allows the women to fill in the days when they had previously been idle. The shift away in hours devoted to casual wage labor is more gradual. Although economically significant, the magnitude of the reduction in hours devoted to casual wage labor implies that four years after the program ultra-poor women still engage in these activities so that differences in labor activities relative to middle- and upper-class women remain.

Table III, Panel B then focuses on earnings from work activities. In column (9) we see that earnings from livestock rearing increase from \$80 to \$115 between years two and four postintervention. The four-year effect is significantly larger than the two-year effect despite a modest drop in labor supply (column (1)), indicating that ultra-poor women are becoming more productive in this activity over time.

In columns (10) and (12) we see that declines in supply of agricultural labor and maid services are associated with significant increases in wage rates in those activities after four years (by 12%

23. The small scale of livestock rearing that ultra-poor women operate at, corresponding to keeping a couple of cows or a cow and several goats, may constrain both the labor input and returns to this activity, making continued engagement in casual wage labor necessary. In other settings, there is also evidence that even small-scale farmers resort to these occupations because they are unable to cover short-term consumption needs with savings or credit (Fink, Jack, and Masiye 2014). The slightly smaller daily time allocation of ultra-poor women to livestock rearing relative to other women (Table II shows that before intervention, women allocated 1.8 hours a day to livestock rearing) might also be due to them operating at a smaller scale than middle- and upper-class women.

and 21%, respectively). These wage effects are insightful as they rule out that the aggregate supply of casual labor by ultra-poor women is perfectly elastic, as in Lewis (1954) and Fei and Ranis (1964). They are consistent with an upward-sloping supply curve because as ultra-poor women remove their labor from village labor markets for these activities, prices need to rise to clear the market (Rosenzweig 1978, 1988; Rose 2001; Jayachandran 2006; Kaur 2015; Goldberg 2016).[24] The removal of ultra-poor labor from these activities and the consequent rise in wages therefore may have positive general equilibrium effects for the wages received by women in other wealth classes who continue to work in these activities. We examine this issue in further detail in Section V.

Increased wages will also benefit the majority of ultra-poor women who continue to devote some hours to agricultural labor and maid services. For agricultural labor we see that the modest reduction in labor supply and the modest increase in wages cancel out so that there is no significant impact on earnings from this activity (column 11). In column (13) we see, however, that for maid labor the reduction in labor supply dominates the increase in wages and total earnings from maid labor fall by 22% after four years. This equates to a statistically significant loss of $25 from casual wage labor per annum after four years (column (12)). This, however, is modest relative to the gain of $115 from livestock rearing over the same period (column (9)).

Aggregating across activities, the reallocation of time from casual labor to a more-than-offsetting increase in livestock rearing leads to a significant increase in net annual earnings (earnings net of input costs of livestock rearing) of 21% relative to controls in the same time period (column (14)). A key impact of the program therefore is to make earnings from livestock a significant additional source of income for ultra-poor households. In short, the program allows women to both raise their net earnings and to smooth their labor supply and earnings stream over the year. Taken together, these imply that the poorest women in these villages are able and willing to take on the same labor activities as

24. We can rule out that the wage increases are due to selection, namely, to lower paid individuals dropping out of these activities. Indeed, the estimated effect on wages is the same in the balanced sample of individuals that engage in these activities in all three waves of the survey (see Section V). This is consistent with these being low-skilled activities that pay similar wages across locations and across the wealth distribution as shown in Figure I, Panel C.

their wealthier counterparts, suggesting that the program lifted barriers they must have faced to entering such work activities at baseline.[25]

It is possible that the program may affect the labor market choices of household members other than the targeted female and these must be taken into account to evaluate the effects on household welfare. In Table A.IV in the Online Appendix we show that while all household members devote some more hours to livestock rearing, the effect is about one tenth of the size of that on ultra-poor women and does not crowd out other work activities or schooling. This allays the potential concern that the program increases women's earnings at the expense of the earnings of other family members or children's education. Another possible channel through which the program might affect the labor market choices of other household members is by inducing some of them to migrate. We find no evidence that this occurs in our setting, likely because 47% of ultra-poor households have no adult members other than the main woman and her husband (if present) and 35% have just one, and because women do not typically engage in seasonal migration in Bangladesh for cultural reasons (Bryan, Chowdhury, and Mobarak 2014). Given these null impacts on migration, migrant remittances are likely to play a minor role.[26]

IV.B. Consumption Expenditures, Savings, and Credit

Table IV analyzes the consequences of ultra-poor women reallocating their labor supply across activities for the welfare of their households. Column (1) shows that relative to the controls, the share of households below the $1.25 poverty line drops by 8.4 percentage points, or 14% after four years. In column (2) we see that consumption expenditure per adult equivalent is 11% higher in treatment relative to control households after four years.[27]

25. The stability of the impact on net earnings at two and four years after intervention suggests the ultra-poor are not necessarily being exposed to more intertemporal risk in livestock rearing, even though 2009 was a low rainfall year in many parts of rural Bangladesh. This is of note given the findings in Attanasio and Augsburg (2014).

26. On the migration channel we find that (i) household size actually increases, rather than decreases, for treated households; (ii) this is partly driven by more adults remaining in the household; and (iii) there is no significant change in outmigration.

27. The consumption expenditure items covered are food (both purchased and produced, accounting for the number of people taking meals in the household),

TABLE IV

TREATMENT EFFECTS ON CONSUMPTION, HOUSEHOLD, AND FINANCIAL ASSETS OF ULTRA-POOR HOUSEHOLDS

	Poverty and consumption			Financial assets		
	(1)	(2)	(3)	(4)	(5)	(6)
	Below poverty line	Consumption expenditure (per adult equivalent)	Value of household assets	Household cash savings	Household receives loans	Household gives loans
Program impact after 2 years	−0.051	30.19	6.86	54.54***	0.123***	0.042***
	(0.046)	(25.34)	(7.26)	(4.60)	(0.03)	(0.01)
Program impact after 4 years	−0.084**	62.62***	39.65***	53.22***	0.110***	0.051***
	(0.038)	(20.82)	(9.08)	(4.01)	(0.03)	(0.01)
Control mean at 4-year follow-up	0.624	575.73	69.69	425	0.220	0.016
Four-year impact: % change	−13.5%	11%	57%	24%	50%	319%
2-year impact = 4-year impact [p-value]	0.379	0.111	0.000	0.781	0.714	0.527
Adjusted R-squared	0.032	0.044	0.082	0.204	0.086	0.026
Number of ultra-poor women	6,732	6,732	6,732	6,732	6,732	6,732
Observations	18,882	18,838	20,196	20,179	20,196	20,196
(clusters)	(40)	(40)	(40)	(40)	(40)	(40)

Notes. DiD ITT estimates: household-level outcomes. Sample: ultra-poor households. Standard errors are in parentheses, clustered by BRAC branch area. *** (**) (*) indicates significance at the 1% (5%) (10%) level. Intent-to-treat (ITT) estimates are reported based on a difference-in-difference specification estimated using OLS. All outcomes are measured at the household level, using data on ultra poor households with an eligible woman resident in them at baseline. This regresses the outcome of interest for household h in village v in survey wave t on a constant, a dummy for whether the household resides in a treated village, dummies for the two follow-up survey waves (two and four years postintervention), the interaction between the treatment assignment dummy and each survey wave dummy, and a set of strata (subdistrict) fixed effects. The coefficients shown are those on the treatment-survey wave interaction terms. Standard errors are clustered by BRAC branch area. In column (1), the poverty line threshold used is $1.25 per person per day, as measured in 2007 prices. In column (2), consumption expenditure is defined as total household expenditure over the previous year divided by adult equivalents in the household. The adult equivalence scale gives weight 0.5 to each child younger than 10. The expenditure items covered are food (both purchased and produced), fuel, cosmetics, entertainment, transportation, utilities, clothing, footwear, utensils, textiles, dowries, education, charity, and legal expenses. In column (3), household assets include jewelry, sarees, radios, televisions, mobile phones, furniture, and so on. In column (4), household cash savings refers to the value of savings held at home, at any bank, at any MFI, and with saving guards. We report the mean of each dependent variable as measured at baseline in treated villages. In all columns we report the p-value on the null hypothesis that the two- and four-year ITT impacts are equal. The number of ultra-poor is the number of eligible women observed at baseline and in both follow-up survey waves. All monetary amounts are PPP-adjusted US$ terms, set at 2007 prices and deflated using CPI published by Bangladesh Bank. In 2007, US$1 = 18.46 TK PPP.

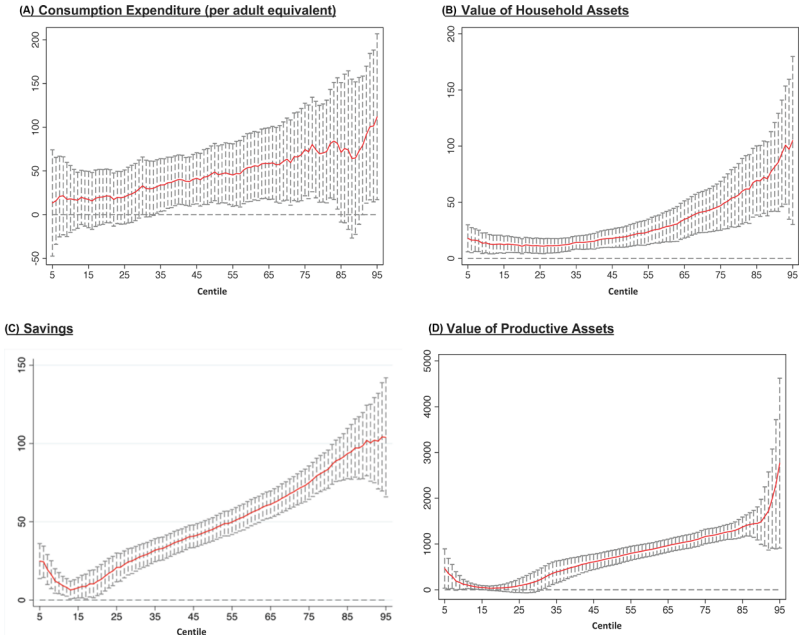

FIGURE II

Four-Year Quantile Treatment Effects

Quantile treatment effect (QTE) estimates of the differences in outcomes between four-year follow-up and baseline are presented in each panel. Each specification controls for randomization strata. Bootstrapped 95% confidence intervals (using 500 replications) are based on standard errors clustered by BRAC branch. Consumption expenditure includes food (both purchased and produced), fuel, cosmetics, entertainment, transportation, utilities, clothing, footwear, utensils, textiles, dowries, education, charity, and legal expenses. Household assets include jewelry, sarees, radios, televisions, mobile phones, furniture, and so on. Productive assets include livestock, land, agricultural equipment, and other machinery used for production. Savings equals the total value of savings held at home, at any bank, at any MFI, and with saving guards. All monetary amounts are PPP-adjusted US$ terms, set at 2007 prices and deflated using CPI published by Bangladesh Bank. In 2007, US$1 = 18.46TK PPP.

Program effects are likely to be heterogeneous depending on unobservables such as the innate ability for livestock rearing and

fuel, cosmetics, entertainment, transportation, utilities, clothing, footwear, utensils, textiles, dowries, education, charity, and legal expenses. Further decomposition of consumption expenditures into food and nonfood reveals the effect is driven mostly by the latter but nutrition improves as the consumption of milk and meat increases.

the underlying constraints faced. We test for heterogeneity by estimating the following quantile treatment effects (QTE) specification:

$$\text{Quant}_{\tau}(\Delta y_{id}) = \beta_{\tau}^{i} T_i + \vartheta_{\tau} \eta_d, \tag{2}$$

where Δy_{id} corresponds to the difference between the four-year and baseline values of outcome y for individual i in subdistrict d.

Figure II, Panel A shows that treatment effects on consumption are nonnegative at each centile, but they are significantly larger at higher centiles with the effect on the 5th centile being roughly one tenth that at the 95th centile. Thus even within the narrow group of ultra-poor households, there is significant variation in the effect of treatment. Uncovering the root causes of these differences among the ultra-poor represents a key priority for future research.

In column (3) of Table IV we see that after four years, household assets (which include jewelry, sarees, radios, televisions, cell phones, bicycles, and furniture) increase in value by 57% relative to control. The increase in the value of household assets is significantly larger after four years relative to two years. In Figure II, Panel B we see that although household asset effects are positive and significant for all centiles, asset accumulation is much more pronounced in the upper centiles.[28]

Columns (4) to (6) of Table IV analyze the impact of the program on financial assets. In column (4) we see that household cash savings held with microfinance organizations, banks, and saving guards increase significantly after two and four years. Given that ultra-poor household savings are negligible in the absence of treatment, the increase in savings of $53 after four years is highly significant and represents a fourfold increase relative to controls. Though it remains a choice variable, households are encouraged to open and manage savings accounts during the first two years. The fact that the savings effect remains significant after four years indicates that households are choosing to save more two years after there is any encouragement to do so. Figure II, Panel C shows that as with consumption expenditure and household assets, the program impact on savings is highly heterogeneous.

28. This is consistent with the pattern of effects on consumption, although we cannot say whether those who experience the largest increases in consumption are the same as those who experience large increases in assets.

In column (5) of Table IV we see that after four years, households are 11 percentage points more likely to receive loans, which represents a 50% increase relative to controls. The program is thus enabling ultra-poor households to obtain access to credit two years after they are encouraged to do so as part of the program. On the other side of financial intermediation, at baseline only 1% of ultra-poor households give loans (Table I). Column (6) shows that they are 5 percentage points more likely to do so after four years relative to controls.

The savings, borrowing, and lending results all point to improved financial inclusion for ultra-poor households. Moreover, the enhanced lending by the ultra-poor to others is a key indicator that their financial position in the village has improved—a proportion of ultra-poor households now have surplus capital that they lend to others. This creates another channel through which the program can affect other households in the village, discussed further in Section V.

IV.C. Productive Assets

Table V examines the program's impacts on the accumulation of productive assets, as this is central to whether the one-off asset and skills transfers lead to sustainable gains in welfare. Columns (1) and (2) analyze the effect on the value of assets transferred by the program, that is, cows and goats. The first thing to note is that ultra-poor women mainly choose cows in their asset transfer package: the mean value of goats transferred is only 8.6% of the value of cows transferred. In column (1) we see that after four years, the value of cows owned by ultra-poor households has increased by 122% (net of the transfer value) relative to controls. At year 4 the value of cows is 16% larger than the value of the asset transfer: the value of cows has increased from $485 to $540 between years 2 and 4 where the original value of the cows transferred was $464. This signals that the majority of ultra-poor households have been able to grow the value of this productive asset via the enlargement of herds. Consistent with this, we can reject the null that the estimated effect on the number of cows (not shown) is equal to the number transferred.[29]

29. Set against a backdrop where attempts to transfer cattle to the poor have a highly checkered history this is a significant finding (Dreze 1990; Ashley, Holden, and Bazeley 1999).

TABLE V

TREATMENT EFFECTS ON PRODUCTIVE ASSETS HELD BY ULTRA-POOR HOUSEHOLDS

	(1) Value of cows	(2) Value of goats	(3) Rents land	(4) Owns land	(5) Value of land owned	(6) Value of other business assets
Program impact after 2 years	484.65***	28.11***	0.069***	0.005	39.80	23.84***
	(19.46)	(3.77)	(0.020)	(0.011)	(75.23)	(6.85)
Program impact after 4 years	539.66***	20.57***	0.110***	0.026*	326.98**	64.76***
	(45.16)	(4.12)	(0.022)	(0.012)	(131.27)	(11.91)
Control mean at 4-year follow-up	61.89	9.26	0.079	0.058	400.61	40.72
Mean value of asset transfer from program	464.03	39.9	–	–	–	–
4-year impact: % change (net of transfer if positive)	122%	−208%	139%	45%	82%	159%
2-year impact = 4-year impact [p-value]	0.148	0.004	0.054	0.005	0.002	0.000
Adjusted R-squared	0.390	0.109	0.077	0.034	0.019	0.066
Number of ultra-poor women	6,732	6,732	6,732	6,732	6,732	6,732
Observations	20,182	20,072	20,196	20,196	20,195	20,195
(clusters)	(40)	(40)	(40)	(40)	(40)	(40)

Notes. DiD ITT estimates: household-level outcomes. Sample: ultra-poor households. Standard errors in parentheses, clustered by BRAC branch area, *** (**) (*) indicates significance at the 1% (5%) (10%) level. Intent-to-treat (ITT) estimates are reported based on a difference-in-difference specification estimated using OLS. All outcomes are measured at the household level, using data on ultra-poor households with an eligible woman resident in them at baseline. This regresses the outcome of interest for household h in village v in survey wave t on a constant, a dummy for whether the household resides in a treated village, dummies for the two follow-up survey waves (two and four years postintervention), the interaction between the treatment assignment dummy and each survey wave dummy, and a set of strata (subdistrict) fixed effects. The coefficients shown are those on the treatment-survey wave interaction terms. Standard errors are clustered by BRAC branch area. In column (6), business assets include pumps, livestock sheds, trees, rickshaws, and others. We report the mean of each dependent variable as measured at baseline in treated villages. In all columns we report the p-value on the null hypothesis that the two- and four-year ITT impacts are equal. The number of ultra-poor is the number of eligible women observed at baseline and in both follow-up survey waves. All monetary amounts are PPP-adjusted US$ terms, set at 2007 prices and deflated using CPI published by Bangladesh Bank. In 2007, US$1 = 18.46 TK PPP.

Column (2) shows that the value of goats held by ultra-poor households (net of the transfer value) actually declines after four years, suggesting that some animals have been liquidated or have died. However, after four years, the cow value effect is 26 times the goat value effect, so overall ultra-poor households experience a large and significant increase in the value of livestock held as a result of the program.

Land is the key asset in the densely populated rural areas of Bangladesh, which are dominated by agriculture, and ultra-poor households have very limited access to cultivable land (see Table I). In columns (3)–(5) we see that the program affects the access ultra-poor households have to land, even though this is not an explicit aim of the program. Ultra-poor households become 11 percentage points more likely to rent land after four years, representing a 139% increase relative to controls. In column (4) we see that ultra-poor households are 2.6 percentage points more likely to own land after four years, representing a 45% increase, and the value of land owned increases significantly by an average of $327 by four years postintervention (column (5)). This accumulation of land takes place between years 2 and 4 with the four-year effect being significantly higher than the two-year effect. This indicates, importantly, that ultra-poor households are using part of the surpluses generated by their reallocation of labor supply toward livestock businesses to invest in land acquisition.

The acquisition of assets also extends to other business assets, such as livestock sheds, rickshaws, vans, pumps, and trees: column (6) shows that after four years the value of such assets held by the ultra-poor is 159% higher relative to the controls. As with land, accumulation of these assets accelerates between years 2 and 4, with the latter effect being significantly larger than the former. This is mostly driven by the acquisition of livestock sheds (an obvious complement to livestock) and means of transport, such as rickshaws and vans.

Combining all productive assets—livestock, land, and other business assets—the QTE estimates in Figure II, Panel D reveal considerable heterogeneity in gains across the productive asset holding distribution. No ultra-poor households reduce their holding of productive assets, but households in the lower centiles gain little. At higher centiles the gains increase markedly. Understanding the causes of this heterogeneity in returns is critical to comprehending how to reach all ultra-poor households and is an important matter to take up in future research.

The materialization of asset accumulation and diversification after four years underscores the value of having longer run data to study poverty trajectories. We return to examine the issue in Section VII, where we exploit data tracking the same ultra-poor households seven-years after the program first started.

IV.D. Comparison with Program Effects in Other Contexts

The program evaluated in this article was started by BRAC in 2002 in Bangladesh and is still the only fully scaled version of the program, which by the end of our study in 2014 had reached over 360,000 ultra-poor households containing 1.2 million individuals. It has served as a template for similar programs that have been implemented in a variety of contexts by different implementing partners. Results from randomized evaluations of pilots of these programs in six countries—Ethiopia, Ghana, Honduras, India, Pakistan, and Peru—have recently been published (Banerjee et al. 2015a).[30] Our analysis differs from those in Banerjee et al. (2015a) in four respects: (i) we collect information on hours worked in every labor activity over the course of one year rather than the past 24 hours or week, and this allows us to minimize measurement error due to the fact that most jobs are seasonal or casual; (ii) we survey all beneficiaries in the scaled-up version of the program rather than a sample in pilot versions and this allows us to estimate the full distribution of treatment effects; (iii) we survey a representative sample of households across the entire wealth distribution rather than ultra-poor households only, and this allows us to quantify general equilibrium effects as well as the distributional effects of the program; (iv) we track beneficiaries four and seven years after the intervention rather than three, and this allows us to study poverty trajectories.[31]

30. The implementing partners, mainly nongovernmental organizations, some of which received state support (e.g., Pakistan, Ethiopia), visited or were visited by BRAC Bangladesh at least twice during the design phase to seek guidance on program design. Thus, though they had to be adapted to particular circumstances of a country, these programs share many of the features of the Bangladeshi BRAC TUP program.

31. In three sites, Ghana, Honduras, and Peru, Banerjee et al. (2015a) randomize the treatment both within and across villages and thus measure spillovers on nontreated ultra-poor. Our design, in contrast, allows us to measure spillovers on households across the wealth distribution and distributional changes. In one site, West Bengal, beneficiaries are resurveyed seven-years after the intervention

Using our data from Bangladesh we replicate the ten key outcome variables studied in Banerjee et al. (2015a). These are all index variables capturing changes along ten dimensions: consumption, food security, assets, financial inclusion, labor supply, income, physical health, mental health, political awareness, and women's empowerment.[32]

Table VI contains a comparison of the effects we observe in our study after four years relative to those observed by Banerjee et al. (2015a) after three years. What is striking is how similar the pattern of effects is across the broad set of ten outcome variables. In all settings: (i) per capita (nondurable) consumption and food security (which captures food adequacy and whether meals are skipped) is significantly increased by the program (columns (1) and (2)); (ii) households are accumulating more household and productive assets as well as saving, borrowing, and lending more (columns (3) and (4)); (iii) adult labor supply, for the main woman in Bangladesh (column (5)) and for all adults in the six pilots (column (6)) also increases; and (iv) income and revenues received by the main ultra-poor woman are increased (column (7)).[33]

This comparison of studies bolsters the external validity of the scaled version of the program we evaluated in Bangladesh. In a variety of settings the combined evidence suggests the arrival of livestock rearing opportunities for the ultra-poor, through asset and skill transfers and other components of the TUP approach, enables them to expand their labor supply, increase their income, and accumulate assets. This in turn leads to improvements in welfare along consumption and food security dimensions. A key

and a preliminary note (Banerjee et al. 2016) reports that, consistent with our evidence in Section VII, the program has lasting impacts.

32. The Online Appendix describes the construction of outcome variables that we compare with Banerjee et al. (2015a) and notes any differences in how our variables are constructed. Even though the survey instruments were designed independently, we are able to construct similar variables along each of the ten outcome dimensions. The exceptions are mental health and political awareness where we use variables that differ somewhat from Banerjee et al. (2015a). Furthermore, for labor supply we use annual labor supply converted to a daily measure to account for seasonal variation, whereas Banerjee et al. (2015a) use labor supply as measured for the past 48 hours or week.

33. Our estimated treatment effects are generally larger than those in Banerjee et al. (2015a). This is likely driven by the fact that the latter are an average across sites, some of which had small or zero treatment effects. Our estimated effects are similar in magnitude to Banerjee et al. (2015a)'s estimates for West Bengal, which is the most similar setting to ours.

TABLE VI

COMPARISON WITH PILOT RESULTS FROM SIX COUNTRIES

	(1) Total per capita consumption, standardized	(2) Food security index	(3) Asset index	(4) Financial inclusion index	(5) Total time spent working by main woman, standardized	(6) Total time spend working by both respondents pooled, standardized	(7) Incomes and revenues index
Panel A							
Treatment effect, 4-year endline	0.314***	0.256***	0.327***	0.313***	0.122*	0.065	0.627***
	(0.034)	(0.079)	(0.029)	(0.040)	(0.065)	(0.047)	(0.074)
Treatment effect in Banerjee et al. (2015a), 3-year endline	0.120***	0.113***	0.249***	0.212***	n/a	0.054***	0.273***
	(0.024)	(0.022)	(0.024)	(0.031)		(0.018)	(0.029)
	(8) Physical health index	(9) Mental health index	(10) Political awareness index	(11) Women's empowerment index			
Panel B							
Treatment effect, 4-year endline	0.108***	0.077*	0.269***	0.077			
	(0.027)	(0.043)	(0.091)	(0.056)			
Treatment effect in Banerjee et al. (2015a), 3-year endline	0.029	0.071***	0.064***	0.022			
	(0.020)	(0.020)	(0.019)	(0.025)			

Notes. *** (**) (*) indicates significance at the 1% (5%) (10%) level. Standard errors in parentheses, clustered by BRAC branch area. Following Banerjee et al. (2015a), we estimate ITT by regressing endline outcomes on baseline outcomes and randomization strata (subdistricts). We construct indices first by defining each outcome so that higher values correspond to better outcomes. We then standardize each outcome into a *z*-score, by subtracting the control group mean and dividing by the control group standard deviation at the corresponding survey round. We then average all of the *z*-scores, and again standardize to the control group within each round. The variables used for each index are described in detail in the Online Appendix. All indices but mental health and political awareness are directly comparable. Column (1) reports standardized total per capita consumption per month. The food security index in column (2) is based on survey responses regarding whether the household had a food surplus or deficit, enough food to eat over the last month, and could afford to have two meals per day most of the time during the last year. The asset index in column (3) is constructed based on the total value of productive and household assets measured in terms of a numeraire asset and standardized. The financial inclusion index in column (4) is constructed based on the amount borrowed in the past 12 months from all sources, informal sources and formal sources, and total savings at the time of the survey. Column (5) reports a standardized measure of the total time the main female household member spent in productive activities on a typical day during the past year, and column (6) pools the same measure for both the female respondent and the male household head where applicable. The incomes and revenues index in column (7) is constructed based on monthly household livestock revenue and income from agriculture, nonfarm micro-enterprises and paid labor as reported by the main female respondent. The physical health index in column (8) is constructed based on respondents' self-reported ability to perform physical tasks, whether any household member had an illness in the 15 days before the survey and whether this interfered with any income-generating activity, and the respondent's self-perception of her current health. The mental health index in column (9) is constructed based on self-reported happiness and mental anxiety. The political awareness index in column (10) is based on whether the respondent can correctly name politicians at different levels and is aware of the lowest legal age for voting. The women's empowerment index in column (11) is based on women's responses to a series of questions regarding their influence over household decision making in several scenarios. Our estimates are based on the sample of 6,732 eligible women used throughout the article. The second row reports the endline 2 estimates from Table 3 in Banerjee et al. (2015a), based on a sample that varies from 9,482 to 9,508.

difference of the TUP program from cash or food transfer programs is this focus on occupational change. The fact that the program has proven to be effective in reducing poverty through occupational change in different contexts makes us more confident that this type of program can be successfully implemented in contexts other than Bangladesh and by organizations other than BRAC.[34]

In Panel B of Table VI we compare noneconomic impacts of the program across studies. Physical health, covering ability to perform physical tasks, work interruptions due to ill health and self-perception of physical health, is significantly improved by the program (column (8)). Mental health, captured by a happiness perception measure and measures of experiencing anxiety and worry, is also improved (column (9)), and in column (10) we see that the program enhances political awareness, captured by political activity or awareness of political representatives at different levels of government. Women also exert greater influence over household decisions after they become beneficiaries of the program (column (11)). Across contexts, the program thus seems to have far reaching effects on physical and mental health, political empowerment and empowerment within the household for ultra-poor women. Economic and social empowerment are key objectives of the program and may reinforce one another. Duflo (2012), for example, hypothesizes that improved mental health may (partly) be what gave ultra-poor women in the India pilot the energy to work more, save, and invest in their children. Looking at these links and interactions to better understand the mechanisms behind the Table VI results represents a fertile area for future research.

V. General Equilibrium and Distributional Effects

The magnitude of the asset and skills transfers, and the fact that treated ultra-poor households make up, on average, 6% of the village population imply that the program might also affect economic outcomes for households in other wealth classes through general equilibrium effects and other spillovers. In Section V.A we provide evidence on these indirect effects, which could be

34. Despite being given a choice, livestock was the main asset taken up in all six pilots, as was the case in Bangladesh. The type of livestock, however, varied strongly—sheep, goats, and oxen in Ethiopia, goats and hens in Ghana, chickens and pigs in Honduras, goats and cows in India, goats in Pakistan, guinea pigs and hens in Peru.

negative or positive. For instance, the new engagement in livestock rearing activities started by the ultra-poor could compete away the financial returns to nonpoor women already engaged in these activities. Alternatively, the additional income generated by the ultra-poor could allow them to increase financial intermediation, thus developing village credit markets to the benefit of all. Our partial population experiment also allows us to quantify distributional effects and, in Section V.B, we focus on the extent to which the program enables the ultra-poor to close the gap with the near-poor.

V.A. Indirect Treatment Effects on Ineligible Households

To estimate the indirect treatment effect on ineligible households we can simply estimate the same difference-in-difference specification (1) on the sample of ineligibles (Angelucci and De Giorgi 2009). To estimate the indirect treatment effect (ITE) on each wealth class of ineligible households, we further interact treatment and survey waves indicators with class indicators:

$$
\begin{aligned}
(3)\quad y_{idt} = & \sum_{t=1}^{2}\sum_{c=1}^{3}\beta_t^c\left(W_t \times T_i \times C_i^c\right) + \gamma T_{id} \\
& + \sum_{t=1}^{2}\delta_t W_t + \sum_{c=1}^{3}\vartheta^c C_i^c + \sum_{t=1}^{2}\sum_{c=1}^{3}\nu_t^c W_t C_i^c \\
& + \sum_{c=1}^{3}\rho^c T_i C_i^c + \eta_d + \varepsilon_{idt},
\end{aligned}
$$

where C_i^c are dummies that take value 1 if i belongs to class c (near poor, middle, and upper class) and all other variables are as defined previously. We thus evaluate the effect of the program on the ineligibles by comparing the change in their outcomes in treated villages to the change in their outcomes in control villages. To benchmark the magnitude of the effects, we report the four-year effects in percentage of the control mean for the same wealth class in the same period.

Because the primary objective of the program is to induce occupational change of ultra-poor women by enabling them to shift their labor supply toward livestock rearing, Table VII first examines general equilibrium impacts on the livestock businesses of ineligible women. Panel A shows indirect treatment effects pooling all ineligible households, and Panel B breaks these out by wealth group. In columns (1) and (2) we see that the program has no significant impact on the value of cows or goats held by

TABLE VII

INDIRECT TREATMENT EFFECTS ON LIVESTOCK AND CASUAL LABOR MARKETS OF WOMEN IN NONELIGIBLE HOUSEHOLDS

	(1) Value of cows	(2) Value of goats	(3) Hours devoted to livestock rearing (main woman)	(4) Wage-maids (main woman)	(5) Wage-agriculture (main woman)	(6) Hours devoted to maid jobs (main woman)	(7) Hours devoted to agricultural wage jobs (main woman)	(8) Yearly earnings (main woman)
Panel A: Pooled								
Program impact after 4 years	−9.53	0.885	5.28	0.044**	0.043*	−16.10	−18.25	−28.75
	(23.02)	(2.49)	(43.89)	(0.020)	(0.024)	(18.99)	(25.47)	(31.26)
4-year impact: % change	−2%	4%	1%	11%	9%	−13%	−13%	−9%
Adjusted R-squared	0.029	0.050	0.044	0.208	0.460	0.021	0.113	0.069
Number of observations	48,212	48,303	48,891	5,055	6,117	48,891	48,891	48,094
(clusters)	(40)	(40)	(40)	(40)	(40)	(40)	(40)	(40)
Panel B: By wealth class								
Program impact on near-poor after 4 years	−24.27	1.72	51.97	0.040**	0.046*	−24.81	−35.45	−26.77
	(21.74)	(2.24)	(44.60)	(0.02)	(0.03)	(32.72)	(45.02)	(22.64)
Program impact on middle-classes after 4 years	28.16	1.85	−30.41	0.052*	0.020	−20.36	−1.38	−14.16
	(30.88)	(3.37)	(46.01)	(0.03)	(0.03)	(14.22)	(12.08)	(49.43)

TABLE VII
(CONTINUED)

	(1) Value of cows	(2) Value of goats	(3) Hours devoted to rearing (main woman)	(4) Wage-maids (main woman)	(5) Wage-agriculture (main woman)	(6) Hours devoted to maid jobs (main woman)	(7) Hours devoted to agricultural wage jobs (main woman)	(8) Yearly earnings (main woman)
Program impact on upper classes after 4 years	−30.03	−1.23	−40.23	–	–	–	–	−63.05
	(72.65)	(6.03)	(54.23)	–	–	–	–	(69.50)
4-year impact on near-poor: % change	−16%	14%	18%	10%	9%	−13%	−14%	−8%
4-year impact on middle classes: % change	6%	7%	−7%	14%	4%	−28%	−2%	−5%
4-year impact on upper classes: % change	−3%	−3%	−8%	–	–	–	–	−18%
Adjusted R-squared	0.213	0.094	0.089	0.207	0.462	0.063	0.150	0.081
Number of observations	48,212	48,303	48,891	5,055	6,117	48,891	48,891	48,094
(clusters)	(40)	(40)	(40)	(40)	(40)	(40)	(40)	(40)

Notes. DiD ITE: program impact after four years. Sample: noneligible households. Standard errors in parentheses, clustered by BRAC branch area. *** (**) (*) indicates significance at the 1% (5%) (10%) level. The sample comprises all ineligible households who are present in the three survey waves. Panel A reports indirect treatment effect (ITE) estimates based on a difference-in-difference specification estimated using OLS in the whole sample. This regresses the outcome of interest for household h in village v in survey wave t on a constant, a dummy for whether the household resides in a treated village, dummies for the two follow-up survey waves (two and four years postintervention), the interaction between the treatment assignment dummy and each survey wave dummy, and a set of strata (subdistrict) fixed effects. The coefficients shown are those on the treatment-survey wave interaction terms four years postintervention. Standard errors are clustered by BRAC branch area. Panel B reports the corresponding coefficients from a specification that allows treatment, survey waves, and their interactions to vary according to social class. All monetary amounts are PPP-adjusted US$ terms, set at 2007 prices and deflated using CPI published by Bangladesh Bank. In 2007, US$1 = 18.46 TK PPP.

ineligible households, and column (3) shows that annual hours devoted by ineligible women to livestock rearing are also unaffected. The point estimates are small relative to the effects on the ultrapoor and relative to ineligible households in control villages.[35] This is prima facie evidence that the entry of ultra-poor women into this work activity does not crowd out richer women who were the main participants in these markets at baseline. In line with this, village-level regressions on the price of milk and the transaction value of cows show no significant reductions.

Part of the explanation for these muted general equilibrium effects is that the cows transferred to the ultra-poor through the program only constitute 7% of the baseline village level stock of cows. So although the gains in cow holdings brought about by the program are highly significant for the ultra-poor, they only have modest effects on the total number of village cows as the herds of wealthier women are much larger. Markets where livestock and livestock products are sold tend to cover a larger area than the area of operations of a BRAC office, with subdistrict and regional markets being particularly important in the Bangladesh context. Also important is the fact that the livestock transferred to the ultra-poor are procured in regional markets (and not from livestock owners within villages).

Although ultra-poor women have limited involvement in livestock rearing at baseline, they are heavily involved in casual wage labor activities, accounting for 47% (58%) of the aggregate hours supplied in agricultural labor (maid services). The changes in labor allocation of the beneficiaries residing in treatment villages might therefore have general equilibrium effects on ineligible households in the village, and these might differ by wealth class. In columns (4) and (5) of Table VII, Panel A, we see that agricultural labor and maid wages for ineligible women rise significantly as a result of the program. This result was already observed for ultra-poor women in Table III as a result of them significantly reducing their casual labor supply. What Table VII illustrates is that ineligible women who continue to work in these labor activities also benefit from these wage increases.

35. It should be noted that the standard errors are large, suggesting that effects are heterogeneous. This notwithstanding, even the largest effect we cannot reject is orders of magnitudes smaller than the effect on the ultra-poor. For instance, the program increases the value of cows by 540 for the ultrapoor while the largest decrease we cannot rule out on the ineligibles is 56.

When we break out the results by wealth class in Panel B, columns (4) and (5), we see that upper-class households do not participate in casual wage labor and that effects are similar across other ineligible wealth classes, consistent with the fact that these are unskilled activities where the return does not vary much across individuals. In columns (6) and (7) we see that ineligible women respond to the wage increase by reducing hours worked, although none of the effects are precisely estimated. Given the muted responses of labor supply across the three main female work activities practiced in these village economies, it is not surprising that the yearly earnings of ineligible women are unaffected by the program (column (8)).

In Table VIII we estimate indirect treatment effects to gauge if there are spillovers of the program on the expenditures and asset accumulation of ineligible households. Columns (1) and (2) show no changes in poverty rates or consumption expenditure per equivalent adult. This is true for ineligible households taken as a whole (Panel A) and when we break out by wealth class (Panel B). All coefficients are small and precisely estimated. This is a key result as it shows that ineligible households are not being made worse or better off by the program. In Figure A.IA in the Online Appendix we graph out the four-year quantile treatment effects on consumption for ineligible households. Unlike Figure II, Panel A, which shows large positive effects for eligibles, this figure is flat and lies along the zero line for the entire consumption distribution.

Column (3) of Table VIII shows that there is no spillover effect of the program on the value of household assets held by ineligible households taken together (Panel A), but we do see a positive effect that is significant at the 10% level for middle-class households when we break out by wealth class (Panel B). In Figure A.IB in the Online Appendix we see some limited evidence of an effect in higher quantiles but this is very muted. Columns (4)–(6) of Table VIII show no significant changes in the value of savings for ineligibles, nor in the probability that these households give or receive a loan. Though imprecisely estimated, there is some suggestion that middle- and upper-class households are less likely to give loans after the program.

Land is an important asset to examine because it is a fixed resource in the village. Column (7) shows that although it is not precisely estimated there is evidence that ineligibles are losing land as a whole (Panel A), and this is almost entirely coming

TABLE VIII

INDIRECT TREATMENT EFFECTS ON CONSUMPTION, HOUSEHOLD AND FINANCIAL ASSETS OF NONELIGIBLE HOUSEHOLDS

	Poverty and consumption			Financial assets			Productive assets	
	(1) Below poverty line	(2) Consumption expenditure (per adult equivalent)	(3) Value of household assets	(4) Household cash savings	(5) Household receives loans	(6) Household gives loans	(7) Value of land owned	(8) Value of other business assets
Panel A: Pooled								
Program impact after 4 years	−0.011	−0.46	33.22	3.69	−0.002	−0.013	626.14	63.55**
	(0.05)	(29.90)	(28.60)	(6.03)	(0.04)	(0.01)	(1,182.80)	(29.35)
4-year impact: % change	−2%	−0.1%	8%	8%	−0.4%	−28%	5%	23%
Adjusted R-squared	0.041	0.038	0.017	0.007	0.055	0.029	0.024	0.018
Number of observations	46,046	45,440	48,200	48,217	48,891	48,891	48,201	48,201
(clusters)	(40)	(40)	(40)	(40)	(40)	(40)	(40)	(40)
Panel B: By wealth class								
Program impact on	−0.015	5.31	11.13	2.52	0.007	−0.003	−32.18	29.35**
near-poor after 4 years	(0.04)	(24.31)	(17.51)	(4.05)	(0.05)	(0.01)	(282.52)	(14.43)
Program impact on middle	−0.030	11.17	53.72*	5.54	−0.003	−0.024	51.16	97.23***
classes after 4 years	(0.05)	(36.57)	(31.80)	(8.06)	(0.04)	(0.02)	(1,425.75)	(34.01)
Program impact on upper	0.011	−27.06	55.03	6.34	−0.054	−0.031	−566.68	63.75
classes after 4 years	(0.05)	(47.32)	(101.20)	(21.97)	(0.04)	(.02)	(3775.98)	(118.79)

TABLE VIII.
(CONTINUED)

	Poverty and consumption			Financial assets			Productive assets	
	(1) Below poverty line	(2) Consumption expenditure (per adult equivalent)	(3) Value of household assets	(4) Household cash savings	(5) Household receives loans	(6) Household gives loans	(7) Value of land owned	(8) Value of other business assets
4-year impact on near-poor: % change	−3%	1%	8%	11%	2%	−9%	−3%	34%
4-year impact on middle-classes: % change	−6%	2%	12%	10%	−0.6%	−27%	0.3%	34%
4-year impact on upper classes: % change	4%	−3%	4%	6%	−9%	−30%	−1%	6%
Adjusted R-squared	0.100	0.156	0.304	0.066	0.079	0.046	0.366	0.204
Number of observations	46,046	45,440	48,200	48,217	48,891	48,891	48,201	48,201
(clusters)	(40)	(40)	(40)	(40)	(40)	(40)	(40)	(40)

Notes. DiD ITE: program impact after four years. Sample: noneligible households. Standard errors in parentheses, clustered by BRAC branch area. *** (**) (*) indicates significance at the 1% (5%) (10%) level. The sample comprises all ineligible households who are present in the three survey waves. Panel A reports indirect treatment effect (ITE) estimates based on a difference-in-difference specification estimated using OLS in the whole sample. This regresses the outcome of interest for household h in village v in survey wave t on a constant, a dummy for whether the household resides in a treated village, dummies for the two follow-up survey waves (two and four years postintervention), the interaction between the treatment assignment dummy and each survey wave dummy, and a set of strata (subdistrict) fixed effects. The coefficients shown are those on the treatment-survey wave interaction terms four years postintervention. Standard errors are clustered by BRAC branch area. Panel B reports the corresponding coefficients from a specification that allows treatment, survey waves, and their interactions to vary according to social class. In column (1), the poverty line threshold used is $1.25 per person per day, as measured in 2007 prices. In column (2), consumption expenditure is defined as total household consumption expenditure over the previous year divided by adult equivalents in the household. The adult equivalence scale gives weight 0.5 to each child younger than 10. The expenditure items covered are: food (both purchased and produced), fuel, cosmetics, entertainment, transportation, utilities, clothing, footwear, utensils, textiles, dowries, education, charity and legal expenses. In column (3), household assets include jewelry, sarees, radios, televisions, mobile phones, furniture, and so on. In column (4), household cash savings refer to value of savings held at home, at any bank, at any MFI and with saving guards. In column (8), business assets include pumps, livestock sheds, trees, rickshaws, and others. All monetary amounts are PPP-adjusted US$ terms, set at 2007 prices and deflated using CPI published by Bangladesh Bank. In 2007, US$1 = 18.46 TK PPP.

from upper-class households (Panel B). The magnitude of the gain in value of land for ultra-poor households (Table V) is similar to the loss for upper-class households (Table VIII). This provides suggestive evidence that land is transferred from the richest to the poorest in these villages, but what are relatively large gains for the ultra-poor are relatively small losses for the upper classes.

Finally, column (8) shows that the value of other business assets (livestock sheds, rickshaws, vans, pumps, etc.) significantly increases overall (Panel A) and for the near-poor and middle-class wealth classes (Panel B). The effect represents a 23% increase overall and a 34%, 34%, and 6% increase for near poor, middle-, and upper-class households, respectively. This could be due to the ultra-poor channeling some of their newly accumulated resources to others in the village or to other households reducing support to the ultra-poor. These findings are consistent with earlier studies that have shown causal links between savings behavior of the poor and improved outcomes for the nonpoor through greater financial intermediation (Angelucci and De Giorgi 2009; Dupas, Keats, and Robinson 2015).[36] However, the value of these business assets is low relative to the value of livestock and land (see Table I), thus the indirect treatment effect on total productive assets is negligible. Figure A.ID in the Online Appendix, which plots quantile treatment effects for the combined value of all productive assets (livestock, land, and other productive assets), shows that although there is evidence of asset accumulation in upper quantiles, none of these effects are statistically significant.

V.B. Distributional Effects

Table I documented that at baseline, the near-poor were better off than ultra-poor households. The partial population experiment allows us to compare how the lives of ultra-poor households have changed relative to the near-poor after four years. To do so we estimate a triple difference specification between baseline and year 4, treatment and control villages and ultra-poor

36. Dupas, Keats, and Robinson (2015) estimate how access to bank accounts affects household financial engagement, where they vary the spouse within the household to whom the bank account is assigned. The spillover effects are estimated through how treated households report changes in transfers they send and receive from others. Although this and other papers have used field experiments to estimate spillover and general equilibrium impacts, our data also allow us to compare changes in outcomes for ultra-poor households relative to near-poor households as is discussed in the next subsection.

and near-poor households. All outcomes are divided by the average difference between ultra-poor and near-poor in treatment villages at baseline, thus an estimated triple difference ζ equal to 1 indicates that the gap has entirely closed between the two groups. We estimate:

$$\begin{aligned} y_{idt} = \alpha &+ \sum_{t=1}^{2} \beta_t^1 (W_t T_i) + \sum_{t=1}^{2} \beta_t^2 N_i W_t + \sum_{t=1}^{2} \beta_t^3 N_i T_i \\ &+ \sum_{t=1}^{2} \zeta_t (N_i \times W_t \times T_i) + \gamma T_i + \gamma^2 N_i \\ &+ \sum_{t=1}^{2} \gamma_t^3 W_t + \eta_d + \varepsilon_{idt}, \end{aligned} \tag{4}$$

where N_i equals 1 if i belongs to the near-poor class and all other variables are as defined previously. The results from this exercise are shown in Figure III. The first bar in the figure indicates that by four years postintervention, ultra-poor households have closed the (small) gap with near-poor households in terms of consumption expenditure. More remarkably, the same is true for the value of household assets, as shown in the second bar, despite the value of household assets held by the ultra-poor being half of that held by the near-poor at baseline. When we examine savings in the third bar, we see that financial savings held by ultra-poor households are four times those held by near-poor households, from a baseline ratio of $\frac{1}{3}$. This is a striking result because this effect is measured four years after the program first starts, and so two years after BRAC's direct involvement ended and when there is no encouragement to hold savings. The result for productive assets in the final bar in Figure III is also striking as we see that ultra-poor households now hold twice the value of productive assets held by the near-poor, including in areas that are not covered by the program, such as land and business assets.

This set of findings suggest the program has significant distributional impacts between the ultra-poor and near-poor, and that on many dimensions the ultra-poor can be classified as firmly entrenched within or above the near-poor wealth class, four years after the program began.

VI. Cost-Benefit Analysis

Table IX makes use of the estimated program impacts to gauge the magnitude of the benefits relative to the program costs

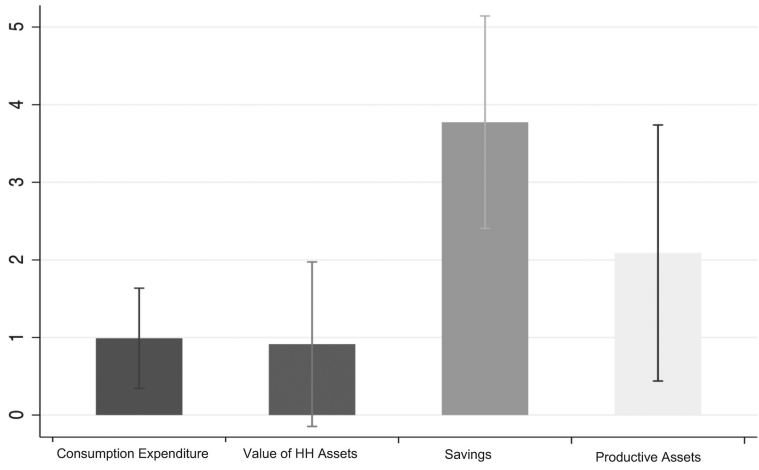

FIGURE III

Four-Year Treatment Effects on the Gap between Ultra-Poor and Near Poor

Estimates are based on a triple-difference specification between baseline and year 4, treatment and control, eligibles and noneligibles. estimated using OLS with standard errors clustered at the branch level. All outcomes are divided by the average difference between eligibles and noneligibles in treatment at baseline, thus a measured impact of 1 indicates that the gap has closed. Consumption expenditure includes food (both purchased and produced), fuel, cosmetics, entertainment, transportation, utilities, clothing, footwear, utensils, textiles, dowries, education, charity, and legal expenses. Household assets include jewelry, sarees, radios, televisions, mobile phones, furniture, and so on. Productive assets include livestock, land, agricultural equipment, and other machinery used for production. Savings equal the total value of savings held at home, at any bank, at any MFI, and with saving guards. All monetary amounts are PPP-adjusted US$ terms, set at 2007 prices and deflated using CPI published by Bangladesh Bank. In 2007, US$1 = 18.46 TK PPP.

and to estimate its internal rate of return (IRR). The average cost per treated household for the two-year program is $1,120 in 2007 PPP terms. We initially set the social discount rate at 5% in line with World Bank guidelines and report sensitivity analysis to alternative rates.

Since the ultimate goal of the program is to reduce poverty, we follow Banerjee et al. (2015a) and use changes in household consumption as our core measure of benefits. These include yearly changes in consumption expenditure and a one-time change in household assets as measured in year 4. The underlying assumption is that the effect of increased financial and productive assets is fully incorporated in consumption changes. To the extent

TABLE IX

COST-BENEFIT ANALYSIS

Panel A: External parameters	
Cost per household at year 0	1,121.34
Cost per household discounted at year 4	1,363.00
Social discount rate = 5%	
Panel B: Estimated consumption benefits	
1 Change in household consumption expenditure year 1	61
2 Change in household consumption expenditure year 2	106
3 Change in household consumption expenditure year 3	237
4 Change in household consumption expenditure year 4	345
5 NPV Change in household consumption expenditure from year 5 for 20 years	3,581
6 Change in household assets year 4	40
7 Total benefits (1+2+3+4+5+6)	4,369
8 Benefits/cost ratio (assuming benefits last 20 years from transfer date)	3.21
Sensitivity to different discount rates/time horizons	
Social discount rate = 10%	2.50
Benefits last 10 years from transfer date	1.86
Benefits last 5 years from transfer date	0.82
9 IRR (assuming benefits last 20 years from transfer date)	0.22
Sensitivity to different outside options/time horizons	
Wage jobs available all year at $0.34 per hour	0.16
Benefits last 10 years from transfer date	0.17
Benefits last 5 years from transfer date	−0.01
Panel C: Estimated asset benefits	
10 Change in productive assets year 4	1,030.50
11 Change in financial assets year 4	85.10
12 Increase in assets/asset cost	1.85

Notes. Household consumption includes food (both purchased and produced), fuel, cosmetics, entertainment, transportation, utilities, clothing, footwear, utensils, textiles, dowries, education, charity, and legal expenses. Productive assets include livestock, land, agricultural equipment, and other machinery used for production. Financial assets equal the value of savings (held at home, at any bank, at any MFI, and with saving guards) plus loans owed to the HH minus loans the HHs owes to others. The IRR is based on estimated nondurable consumption gains, assuming that these last for the expected productive life of the beneficiaries, set at 20 years. When we assume that wage jobs are always available at the observed agricultural wage we deduct the estimated increase in labor supply (206 hours) multiplied by the wage from the consumption benefits. All monetary amounts are PPP-adjusted US$ terms, set at 2007 prices and deflated using CPI published by Bangladesh Bank. In 2007, US$1 = 18.46 TK PPP.

that asset accumulation as of year 4 will lead to even greater increases in consumption in the future, we will underestimate the benefits of the program. Moreover, we make no attempt to price the utility gains to the ultra-poor arising from a smoother

allocation of labor hours across days of the year (as was shown in Table III).[37]

The first four rows in Table IX report ITT estimates of the program on consumption, for every year after the intervention up to year 4. The year 2 and 4 effects are estimated from our midline and endline surveys, respectively, whereas the 1 and 3 year effects are imputed using linear interpolation. The fifth row reports the net present value of future consumption changes from year 5 onward, assuming that year 4 changes are repeated for 20 years from the transfer date (so 16 more years after year 4). Our choice of time horizon is dictated by three facts: (i) the average beneficiary was 40 years old when she received the asset in 2007, (ii) women in these villages work the same number of hours at 60 and older as they do at 40, (iii) the female life expectancy at birth is 71 today. As these women were born when life expectancy was lower and they live in the poorest areas of the country, we assume they will be able to continue working with the assets until they are 60, so 20 years from the transfer date. Below we present sensitivity analysis to shorter time horizons. In every case we assume that the benefits cease with the death of the original beneficiaries, which is a lower bound if other family members inherit the asset or continue to benefit from it after the death of the beneficiary.[38] The sixth row reports the year 4 change in the value of household assets (i.e., durables), and the seventh row adds these up to compute the net present value of benefits. This is divided by the program cost to obtain the benefit/cost ratio in the eighth row.

The estimates show that the average benefits of the program are 3.21 times larger than its cost.[39] Table A.VII in the Online Appendix uses our quantile treatment effects to compute the ratio at different quantiles—it shows that the ratio is above 1 throughout.

37. We focus on the benefits accruing to the ultra-poor alone as the program had no effect on the consumption of ineligible households (Table VIII, column (2)). Table VIII shows that after four years the program increases the business asset holdings of ineligible households. We therefore underestimate the benefits accruing to these households to the extent that this will allow them to increase future consumption.

38. For instance, Roy et al. (2015) show that men belonging to the households of the treated women benefited indirectly by being able to purchase productive assets from the women's additional earnings.

39. Using the same methods, Banerjee et al. (2015a) report an average benefit/cost ratio of 1.59 for the six pilots.

The eighth row of Table IX investigates sensitivity to different values of the discount rate and different time horizons. The ratio of average benefits to costs remains above 1 in all cases except if we assume that benefits disappear the year after our endline, in which case the ratio falls below the break-even point for the average ultra-poor household. If benefits last two years after endline, that is, six years after transfer, the benefit to cost ratio is 1.06.

The ninth row shows the IRR under alternative assumptions about outside options and time horizons. The average internal rate of return in our baseline specification is 22%, and it is positive and clearly above the discount rate; it goes to 0 only when we assume that benefits disappear altogether one year after our endline (five years after the transfer).[40] Although these calculations take into account that beneficiaries substitute away from casual wage labor and hence lose some earnings from that activity (see Table III), they do not take into account that beneficiaries work 206 more hours and 61 more days over the course of a year. The value of this time depends on its opportunity cost. We consider two scenarios: (i) assuming aggregate demand constraints for wage labor bind so there is zero opportunity cost of spending additional hours in livestock rearing; (ii) assuming unconstrained demand in casual wage labor and so the lost hourly wage is \$0.34 an hour, that for agricultural wage labor (which is higher than for casual maid work, as Table I shows). This is likely to be an upper bound as recent micro studies suggest the true opportunity cost of labor is likely below the prevailing wage rate (Foster and Rosenzweig 2010; Kaur 2015).[41] The final row of Panel B in Table IX reports lower bounds for the IRR under the latter assumption as we deduct the value of 206 hours at \$0.34 an hour from estimated consumption benefits. With this adjustment the IRR falls from 22% to 16% but remains positive and larger than the social discount rate of 5%.

40. This is also above the average internal rate of return of 12% reported in Banerjee et al. (2015a).

41. Foster and Rosenzweig (2010) use data from rural India to document that various market imperfections such as supervision costs, credit market imperfections, and scale economies lead to a surplus of labor on small farms: they quantify that 20% of the Indian agricultural labor force is surplus to requirement. Kaur (2015) finds that casual wage labor markets in rural India are well characterized by downward nominal wage rigidity (that are driven by fairness concerns of employers).

Finally, Panel C of Table IX measures program benefits in terms of productive asset accumulation (livestock, land, agricultural equipment, and other machinery used for production) and financial assets (savings plus net lending). The twelfth row shows that four years after the asset transfers, the average household has further accumulated productive assets valued at almost twice as much as the original transfer. Financial assets are included in this calculation but they account for less than 10% of the total. The high rates of asset accumulation suggest that future consumption gains might be sustainable. The next section uses descriptive data from seven-year follow-up on the same households to provide indicative evidence on this issue.

VII. The Ultra-Poor in the Long Run

To assess whether the one-off asset and skills transfers provided by the program set the ultra-poor on a long-run trajectory out of poverty, we fielded a survey to the same ultra-poor households in 2014, seven-years after the baseline. We were able to trace 93% of the households. As described already, the evaluation design was such that the program would be offered in control villages starting in 2011 (i.e., after the year 4 follow-up survey). By 2014, every control BRAC branch office had treated some villages within its radius. To choose which villages and which individuals to treat, BRAC program officers followed the same process as in 2007, namely, they made a list of all villages in the branch ranked from poorest (i.e., with the largest number of poor households) to least poor, and then implemented a participatory rural appraisal (PRA) to identify the beneficiaries in each village starting from the poorest villages and stopping when they reached their target number of beneficiaries.

In practice this implies that 49% of the villages originally assigned to control have at least one woman treated, and 20% of the originally selected beneficiaries plus 10% of the original "near poor" were treated. In 2014 we thus have three groups: the early treated (in 2007), the late treated (in 2011), and the untreated controls. The challenge in identifying the effect of the program in 2014 is that the selection of the late treated is correlated to the outcome of interest: poverty. Indeed, given BRAC's targeting strategy, the late treated have lower consumption expenditures, durables, and other assets than those left untreated in 2011.

To provide evidence on the long-run impact of the program we follow two strategies. The first simply extends specification (1) to include the 2014 survey wave and all control villages/individuals regardless of whether they are late treated. We note that to the extent that the program has some effect on the late treated after three years, this strategy yields a lower bound on the actual effect because one fifth of the control group is actually treated. The second strategy requires making assumptions about the size of the effect on the late treated. To this purpose we exploit the QTE estimates on the original treated to create counterfactuals of the effect of the program on the late treated. Because by 2014 these have been treated for three years, we interpolate between our two- and four-year estimates of the ITT on the originally treated group to derive a counterfactual effect for the treated controls in 2014. Table X reports three difference-in-difference estimates derived by assuming that the effect on the late treated is equal to the median, 75th percentile, and 25th percentile treatment effect on the early treated. Throughout we focus on the outcomes used in the cost-benefit analysis above: household consumption expenditures, household assets, savings, and productive assets.

Table X reports difference-in-difference estimates at each survey wave (2009, 2011, 2014) using the two strategies above. As for the earlier estimates, Table A.VIII in the Online Appendix reports the equivalent ANCOVA specifications. The results are consistent across outcomes and specifications: the seven-year effects are positive and precisely estimated. Moreover we never reject the null that the seven-year effects on consumption are equal to the four-year effects, thus reinforcing the conclusions of the cost-benefit analysis. The only effect that is systematically smaller after seven-years is that on savings, which falls by about 50%, depending on the specification. Further analysis shows that this is coupled with an increase in land access through purchases, which are captured in the value of productive assets, and especially rentals, which are not. The most conservative estimate suggests that average quantity of land rented increase by 3.5 decimals after four years and by 4.4 decimals after seven. Given that agricultural land is a key asset in the villages we study and is also the asset which most clearly differentiates poor from nonpoor households, this is a striking change.

Overall, though these seven-year results must be interpreted with caution as the responses of the original beneficiaries might be an imperfect counterfactual for the responses of the late treated controls, a major difference would be needed to reverse the

TABLE X

SEVEN-YEAR TREATMENT EFFECTS ON CONSUMPTION, SAVINGS, AND ASSETS OF ULTRA-POOR HOUSEHOLDS

	(1) Household consumption expenditure	(2) Value of household assets	(3) Household cash savings	(4) Value of productive assets
Program impact after 2 years	112.2*	6.860	54.69***	606.4***
	(62.62)	(7.262)	(4.601)	(92.05)
Program impact after 4 years	358.2***	39.65***	53.22***	972.6***
	(63.54)	(9.075)	(4.007)	(158.3)
Program impact after 7 years				
adjustment for program effect on the late treated:				
1. none	281.0**	27.09*	21.43***	662.0***
	(119.6)	(13.93)	(3.935)	(214.4)
2. = median 3Y treatment effect on the early treated	327.2***	30.36**	31.84***	782.8***
	(119.5)	(13.94)	(4.054)	(214.6)
3. = 75th ptile 3Y treatment effect on the early treated	338.9***	33.52**	36.34***	830.9***
	(119.6)	(13.96)	(4.222)	(215.0)
4. = 25th ptile 3Y treatment effect on the early treated	315.5**	28.36**	27.90***	751.1***
	(119.5)	(13.93)	(3.962)	(214.5)

TABLE X
(CONTINUED)

	(1) Household consumption expenditure	(2) Value of household assets	(3) Household cash savings	(4) Value of productive assets
p-values:				
4-year impact = 7-year impact (row 1)	0.563	0.354	0.000	0.052
4-year impact = 7-year impact (row 2)	0.816	0.496	0.000	0.233
4-year impact = 7-year impact (row 3)	0.749	0.409	0.000	0.374
4-year impact = 7-year impact (row 4)	0.885	0.652	0.001	0.164
Observations	25,176	26,437	26,437	26,435
(clusters)	(40)	(40)	(40)	(40)

Notes. DiD ITT estimates: household-level outcomes. Sample: ultra-poor households. Standard errors in parentheses, clustered by BRAC branch area. *** (**) (*) indicates significance at the 1% (5%) (10%) level. Intent-to-treat (ITT) estimates are reported based on a difference-in-difference specification estimated using OLS. All outcomes are measured at the household level, using data on ultra-poor households with an eligible woman resident in them at baseline. We estimate seven-year treatment effects under different assumptions on the effect of the program on the late treated households in control villages. Row 1 assumes the program effect on the late treated is 0 and includes all control households regardless of whether late treated or not. Rows 2 (3,4) assume that the program effect on the late treated is equal to the median (75th, 25th percentile) effect on the early treated at the same point in time. In these rows we adjust the seven-year outcomes of the late treated by adding the estimated treatment effect of the early treated. In all specifications we regress the outcome of interest for household h in village v in survey wave t on a constant, a dummy for whether the household resides in a treated village, dummies for the three follow-up survey waves (two, four, and seven-years postintervention), the interaction between the treatment assignment dummy and each survey wave dummy, and a set of strata (subdistrict) fixed effects. The coefficients shown are those on the treatment-survey wave interaction terms. Standard errors are clustered by BRAC branch area. In column (1), consumption expenditure is defined as total household expenditure over the previous year. The expenditure items covered are food (both purchased and produced), fuel, cosmetics, entertainment, transportation, utilities, clothing, footwear, utensils, textiles, dowries, education, charity, and legal expenses. In column (2), household assets include jewelry, sarees, radios, televisions, mobile phones, furniture, and so on. In column (3), household cash savings refer to the value of savings held at home, at any bank, at any MFI, and with saving guards. In column (4) productive assets include livestock, land and business assets. We report the mean of each dependent variable as measured at baseline in treated villages. In all columns we report the p-value on the null hypothesis that the four- and seven-year ITT impacts are equal. All monetary amounts are PPP-adjusted US$ terms, set at 2007 prices and deflated using CPI published by Bangladesh Bank. In 2007, US$1 = 18.46 TK PPP.

conclusion that a one-off transfer of assets and skills allows the ultra-poor to escape poverty in a sustainable way.

VIII. Conclusions

The question of how to eliminate extreme poverty by 2030 has now risen to the top of the development policy agenda, and there is a growing realization that the poorest may be being bypassed both by economic growth and by current antipoverty programs.[42] Our results suggest the labor activities the poor can access and their ability to exit poverty are intrinsically linked. The women we study possess no means of production other than their labor and lie at the bottom rung of the employment ladder in rural villages, facing low returns to and irregular demand for their labor. They live predominantly in the *monga* or famine areas of Bangladesh, and in the work they do they are not very different from the majority of Indian famine victims in the nineteenth and twentieth centuries (Dreze 1988).

We find that the TUP program enables these ultra-poor women to take on the labor market activities of better off women in the same villages as they dramatically expand overall labor supply, principally by working more hours in livestock rearing. As their labor supply expands and their employment becomes more regular, they experience a 21% increase in earnings, which allows them to accumulate further productive assets and set off on a sustainable trajectory out of poverty.

Our evidence demonstrates that enabling the poor to allocate their labor to the activities chosen by richer women in their villages may have a central role to play in eliminating extreme poverty. However, given that the TUP program has multiple components bundled together, we understand little so far about which elements are critical to unleashing this process of change. Getting a better sense of this is therefore a key priority. Understanding why we observe heterogeneity in program returns is also critical for gaining a better understanding of the determinants of poverty.

After four years we find that the program was highly cost effective with an IRR of 22%, and that a sizable fraction of ultra-poor households would have enjoyed positive returns had they been able to finance these investments from either the formal

42. This was part of a longer set of Sustainable Development Goals agreed in 2015.

or microfinance sectors. Given these findings, it would also seem worthwhile exploring versions of the program where households have to repay some fraction of the cost of the asset transfer as a means of reducing program costs.

What is also important is to understand how different ways of financing the program affect the cost benefit analysis. Buera, Kabowski, and Shin (2014) study the scale-up properties of TUP-style programs using a quantitative general equilibrium model of occupational choice with credit market imperfections to simulate the aggregate impacts of a one-time redistribution (not transfer from outside) of assets (ignoring skill transfers). Their simulations generate muted long-run impacts because they find only the top quartile most productive individuals transition to capital intensive activities. This does not match our micro evidence where the TUP program appears well targeted so the share of ultra-poor engaged in livestock rearing rises by 48 percentage points four years after the transfer. More work needs to be done to bring together these macro and micro approaches, including developing models that incorporate the skills transfer component of the program and which model transfers as coming from outside the village.

A key difference of the TUP program from most cash or food transfer programs is that it is a one-off, big push intervention. Though big push programs require large up-front investment, our evidence suggests they are cost-effective and lead to sustained increases in household welfare. Indeed, the observed pattern of asset accumulation between years 2, 4, and 7 indicates that although the cost of the two-year program is fixed, the benefits grow in the short term and stabilize in the medium term. This may be a key advantage relative to cash and food transfer programs which do not encourage occupational change, where annual costs are lower but need to be recurrent to exert an influence on consumption (see also Blattman, Fiala, and Martinez 2014; Banerjee et al. 2015a; Banerjee 2016).[43]

Understanding whether and how governments can take up these programs and whether they can be adapted to urban settings are all unknowns that will have a critical bearing on whether this idea spreads and scales. The juxtaposition of the goal of

43. At the same time, Banerjee et al. (2015b) analyze data from seven randomized controlled trials of government-run cash transfer programs in six developing countries and find no systematic evidence that cash transfer programs discourage work.

eliminating extreme poverty by 2030 and the promising set of initial results in this and related papers does, however, suggest that taking up these research challenges would be a worthwhile endeavor.

DEPARTMENT OF ECONOMICS, IGC AND STICERD, LONDON SCHOOL OF ECONOMICS
DEPARTMENT OF ECONOMICS, IGC AND STICERD, LONDON SCHOOL OF ECONOMICS
RESEARCH AND EVALUATION UNIT, BRAC
DEPARTMENT OF ECONOMICS AND IGIER, UNIVERSITA' BOCCONI
DEPARTMENT OF ECONOMICS, UCL, IFS AND IGC
RESEARCH AND EVALUATION UNIT, BRAC INTERNATIONAL

SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

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