Assessing the Frontiers of Ultrapoverty Reduction: Evidence from Challenging the Frontiers of Poverty Reduction/Targeting the Ultra-poor, an Innovative Program in Bangladesh

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I. Introduction

It is widely appreciated, both by practitioners and academics, that extreme poverty (or ultrapoverty) may be different from other forms of poverty and deprivation (see, e.g., Lipton 1986; World Bank 2006; IFPRI 2007; Matin, Sulaiman, and Rabbani 2008).[1] Ultrapoverty differs from conventional poverty

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[1] Although there is a growing consensus that extreme or ultrapoverty is an important and difficult problem requiring novel intervention strategies, the concept of "ultrapoverty" remains unsettled. There are different definitions in the literature: Lipton (1986) defines ultrapoverty in terms of a calorie intake threshold (a person is ultrapoor if he or she gets 80% or less of an appropriate poverty-line calorie benchmark); a recent IFPRI report (2007) identifies an individual as ultrapoor if he or she lives on less than 54 cents per day. Emran, Shilpi, and Stiglitz (2009) define ultrapoverty in terms of endowments and access to markets; physical and human capital endowment of the ultrapoor is so low that it results in exclusion from both formal labor and credit markets. In this article, we take the BRAC (Building Resources across Countries) identification scheme as given for the empirical analysis. The BRAC

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in terms of depth (higher degree of deprivation), length (longer duration), and breadth (larger number of dimensions such as illiteracy, malnutrition, etc.). The possible complementarity among the different dimensions can potentially result in multiple mutually reinforcing poverty traps, thus making ultrapoverty an especially difficult problem to address.[2]

The experience of the last few decades suggests that while the poverty programs of nongovernmental organizations (NGOs) including microcredit programs have, in general, been successful in reaching the moderate poor (i.e., households below the poverty line but relatively close to it), the poorest of the poor are more often inadequately served or completely bypassed by such programs.[3] This appreciation led to the development and implementation of innovative antipoverty programs that are designed especially for the ultrapoor. These programs are supposed to address simultaneously the multitude of interrelated factors that create extreme poverty and make it a trap difficult to escape from.

BRAC (Building Resources across Countries), formerly known as the Bangladesh Rural Advancement Committee, is one of the first NGOs to implement a program specifically designed to address extreme poverty. In 2002, BRAC implemented an ultrapoverty program called Challenging the Frontiers of Poverty Reduction: Targeting the Ultra-poor, Targeting Social Constraints. The first phase of the Targeting the Ultra-poor (TUP) program was implemented over 2002–6. It covered 100,000 ultrapoor households from 15 of the poorest districts of Bangladesh over 5 years.[4] TUP is a multidimensional program that incorporates both livelihood protection and promotion components and features significant innovations in targeting (through participatory wealth ranking by the villagers) and harnessing social capital (through village support networks and sponsorship of community leaders). It

definition refers to "not being able to meet even the barest of the basic needs" (Rabbani, Prakash, and Sulaiman 2006, v). For recent analysis of issues related to identification and proper targeting of the ultrapoor, see Sulaiman and Matin (2006) and Banrejee et al. (2008).

[2] For discussions, see World Bank (2000), Chronic Poverty Research Center (2008), and Kwak and Smith (2013).

[3] The lack of participation by the ultrapoor in standard microfinance may be a result of both a household's self-selection and screening by the NGOs. The ultrapoor may find it difficult to participate because of rigid repayment schedules and the high time costs involved, e.g., in regular meetings. The NGOs, however, may try to screen out the poorest because of a lack of complementary inputs such as human capital (little education and ill health). It is easier for an NGO to show effectiveness of its program and thus attract donor funds by concentrating on the households marginally below the poverty line (i.e., moderate poor).

[4] A second phase of the TUP program covering 40 districts was initiated in 2007: 863,000 households are expected to participate in the second phase over 5 years (2007–11). This article provides evidence of the effects of the first phase of the TUP program. For more details on the second phase of the TUP program, see BRAC (2007).

focuses on developing human capital (health, education, and training) and physical capital (asset transfers) for poor women. The program provides training in enterprise activities using the transferred asset and also health services. A more complete discussion of the program is provided in Section II. TUP as a strategy to tackle ultrapoverty has attracted much attention over the last few years among NGO communities and academic researchers. Similar programs are being replicated in several other countries including India, China, Uganda, Tanzania, and Peru.[5]

This article uses a two-period panel data set (2002, 2005) to analyze the effects of TUP program participation on a set of household outcomes including income, food security, clothing, health, child labor, schooling, and asset accumulation. The first phase of the TUP program was not a randomized intervention. Thus, one has to carefully address the selection biases to identify and estimate the treatment effects of the program. Given the importance of the program, a careful analysis of the TUP program (phase 1) with household panel data is of significant value.

To provide credible evidence on the treatment effect of participation in the TUP program, we address the possible biases due to omitted heterogeneity. We use BRAC's selection criteria and the assignment errors to construct an alternative treatment-comparison pair that represents the target households more faithfully and also identifies an appropriate comparison group. An advantage of the panel data is that we can credibly address the time-invariant household-level heterogeneity (additive) by using household fixed effects. This is important for understanding the effects of TUP (or any other microfinance program), as two of the salient omitted variables in this context are innate entrepreneurial ability and risk preference, both of which are effectively time invariant. The main results in this article thus use household-level fixed effects. To estimate the effects of TUP membership on household outcomes, we take advantage of the recent heteroskedasticity-based identification approach due to Klein and Vella (2009). We also report data from two recent estimators that are especially useful for the binary outcomes because the Klein and Vella (2009) approach is not designed for the binary outcomes.

The two estimators used in this article are the minimum biased inverse probability weighted (MB-IPW) estimator due to Millimet and Tchernis (2013) and the difference-in-difference matching (DIDM) estimator due to Heckman, Ichimura, and Todd (1998).[6] The treatment effects from the

[5] Other examples of programs for ultrapoverty include the Grameen beggars program and the Bandhan Chartering into Unventured Frontiers—Targeting the Hardcore Poor program.

[6] The DIDM approach reduces the bias in estimated treatment effects compared to cross-sectional studies with or without matching (Blundell and Costa-Dias 2009). The DIDM approach has been used by Berlinski and Galiani (2004) and Petkova (2009), among others.

DIDM approach can be biased if the counterfactual trend in the treatment group is different from the trend found among the comparison households, after matching on observables and household fixed effects are used to control for potential heterogeneity. The MB-IPW estimator developed by Millimet and Tchernis (2013) minimizes the bias that arises from the violation of the conditional independence assumption, by appropriately trimming the sample around the bias-minimizing propensity score.

A standard approach to tackling selection on unobservables and measurement error is to develop an instrumental variables (IV) strategy that exploits some features of the program design or implementation to isolate exogenous variations in program participation. Unfortunately, it was not possible to develop a credible and strong enough IV strategy based on the features of the TUP program. In the absence of credible exclusion restrictions, we employ the heteroskedasticity-based identification approach that does not rely on the standard exclusion restrictions (Rigobon 2003; Klein and Vella 2009, 2010; Lewbel 2012).[7] In particular, we implement the heteroskedasticity-based IV approach due to Klein and Vella (2009).

An innovative feature of our study is that it uses alternative treatment and comparison groups using the type 1 assignment errors in the TUP selection process. BRAC's own treatment group is called the "selected ultrapoor" (SUP), which includes all the actual participants in the program irrespective of whether they meet the eligibility criteria set out by BRAC itself. The corresponding comparison group is called the "not selected ultrapoor" (NSUP). However, there is significant mistargeting (assignment errors) in the TUP program when judged by the inclusion and exclusion criteria set out by BRAC for the TUP program participants. Such targeting errors usually result in a treatment group that is on average richer than the intended target group of a program.[8] We use the assignment errors in the selection of participants in the TUP program to partition the sample to generate alternative treatment and comparison groups.[9] The assignment errors can be used to create two

[7] For recent applications of heteroskedasticity-based identification, see Rigobon (2003), Rigobon and Rodrik (2005), Farré, Klein, and Vella (2010), Schroeder (2010), Emran and Sun (2011), Emran and Shilpi (2012), Berg, Emran, and Shilpi (2013), Chowdhury et al. (2013), Emran and Hou (2013), and Millimet and Tchernis (2013).

[8] Mistargeting is common in microcredit programs including most well-known programs such as Grameen. For discussions on mistargeting in Grameen microfinance programs, see Matin (1998), and for evidence on BRAC programs, see Montgomery, Bhattacharya, and Hulme (1996) and Zaman (1998). For a recent discussion on issues related to targeting in microcredit programs, see Banerjee et al. (2008). Although mistargeting has become a concern in microfinance programs, it is well known that optimal targeting does not imply zero assignment errors (Ravallion 2008; Kanbur 2010).

[9] A descriptive analysis of the TUP program with some preliminary impact evaluation was done by BRAC's in-house Research and Evaluation Division (RED) using the same panel data set (see Rabbani et al. 2006). They use SUP as the treatment group and NSUP as the comparison group.

treatment-comparison pairs based on type 1 and type 2 errors in assignment. The treatment and comparison groups based on type 1 errors are called SB_1, or "should be, one," and SB_0, or "should be, zero." The treatment group consists of the households who satisfy the BRAC inclusion and exclusion criteria and thus are correctly selected into the program, while the comparison group consists of the households who are incorrectly excluded from the program according to the stated criteria. As we show later, this comparison group is very similar to the treatment group in terms of initial characteristics, and thus the possibility of selection bias is much lower compared to the SUP-NSUP groups. Also, the treatment group SB_1 consists of the poorest of the households in our sample (i.e., the "true" ultrapoor). This treatment-comparison pair thus allows us to estimate the treatment effect of program participation on the intended beneficiaries of the TUP program. Given that the focus of the study is on the effects of the TUP program on the poorest of the poor, we omit the results from the treatment-comparison pair based on type 2 errors because the treatment group consists of the richest households in the sample.

The evidence from the DIDM, MB-IPW estimators, and Klein and Vella (2009) approach, implemented with household fixed effects, shows that there is a significant positive effect of participation in the TUP program on food security, cash savings, livestock (cows/bulls), housing (homestead land and tin roof house), and shoes, for both the treatment groups: SUP and SB_1. The evidence is also consistent with a positive effect of TUP membership on per capita income of a participant household, especially for the SB_1 group.[10] There is, however, no evidence of a significant effect of the TUP program on subjective health outcomes, productive assets (such as fishing nets, big trees, and rickshaw vans), child labor, and women's empowerment.[11]

We also find that there are some important differences in the program effects between the two treatment groups: SUP and SB_1. For example, while the evidence that participation increases net income is very strong for the SB_1 group, it is weaker, in terms of both magnitude and statistical significance, for the SUP group. The households in both treatment groups seem to invest in productive assets such as livestock, but the effect is stronger for the SB_1 group. Only the relatively richer SUP households seem to acquire luxury goods such as a radio/TV. In fact, the program effect on radio/TV ownership is negative for the SB_1 households according to the Klein and Vella (2009) estimate, which is consistent with the idea that the poorest-of-the-poor households cut back their

[10] The definition of income is a broad one that takes into account the change in the value of household assets due to activities such as livestock fattening commonly undertaken by TUP participants. It is thus a comprehensive measure of the household welfare for the TUP participants.

[11] We have limited indicators of women's empowerment, however. The conclusion that there is no program impact on women's empowerment thus should be interpreted with the appropriate caveat.

other expenditure in order to make productive investments. When the differences in the initial conditions are taken into account, the program effects are significantly larger for the SB_1 treatment group compared to the BRAC treatment group SUP. Had the program concentrated on the poorest of the poor, the program effects would have been larger than those found.

The rest of the article is structured as follows. Section II provides a brief discussion of the BRAC TUP program. Section III describes our data set. Section IV discusses the empirical strategy for identification and estimation of the treatment effects in full detail. Section V reports the results of our empirical analysis on the treatment effect of program participation in a sequential manner starting from a simple difference-in-difference approach. The article concludes with a brief summary of the findings.

II. The BRAC Ultrapoverty Program

One of the most comprehensive approaches to redressing ultrapoverty has been developed and implemented by BRAC in Bangladesh. BRAC is the world's largest NGO when measured in terms of membership, scope, and budget. Founded in 1972, it started microfinance in 1974, which by 2010 included approximately 8 million women members. The BRAC Education Program serves over 1 million (approximately 10%) Bangladeshi primary students in some 35,000 informal schools. Over 110 million individuals receive BRAC health and other services in Bangladesh.

TUP (phase 1) was launched in 2002 in three of the poorest districts in northwest Bangladesh (Rangpur, Kurigram, and Nilphamari), identified on the basis of poverty mapping and selected from a larger group of potential participants, who together form the basis for our panel data set. All members of treatment and comparison groups were selected by villagers as among the poorest local families. A subset was selected by BRAC according to exclusion and inclusion criteria. The exclusion criteria required that participating women must be capable of doing work outside the home, must not belong to another NGO program, and must not receive a food benefits card. In the inclusion criteria, participating women have to meet three of the following criteria: child labor is present, ownership of less than 10 decimals of land (a tenth of an acre), lack of a male earner at home, adult women selling labor outside of the household, and lack of any productive assets (Noor et al. 2004, ix; BRAC 2007, 24).

To identify the ultrapoor women, several strategies were used. One is participatory wealth ranking, which uses local information available to the villagers. A meeting is held in which a village map is drawn on the ground with each household labeled. The villagers agree on a wealth ranking among the households, to identify those who are the poorest of the poor. Those who can

afford tin plate walls or roofs are less poor than those with straw walls or thatched roofs. Those who are known to have a steady, formal job are categorized as among the well off. To keep the process manageable, only about 150 households were included in each wealth-ranking exercise.

There are incentives for people to try to rank themselves as poor to receive assistance, but the multiple checks done on family status means their ability to get away with this is limited. To supplement community meetings, BRAC staff members walk through the village, looking for any hut that gives the appearance of extreme poverty. They then try to bring potentially overlooked ultrapoor people to the attention of the community meetings. Village leaders, generally people who are relatively well educated such as the school teachers, were actively involved in all stages of the process. Although the BRAC selection mechanism was imperfect, it is important to appreciate that the resulting mistargeting may actually have made BRAC's own treatment and comparison groups (SUP-NSUP) more comparable than they otherwise would be.[12]

The TUP program aims to improve the physical, human, and social capital of the poorest of the poor. A core activity of the program is to provide participants with a grant of specific physical assets. The TUP program then provides assistance for using the transferred assets effectively as a microenterprise. In particular, BRAC staff members offer ongoing training in specific enterprise activities, notably livestock and poultry rearing, operation of tree nurseries, and village vending such as circulating around the village with a pushcart. Each training program is targeted to the specific asset transferred; periodic refresher training is offered. After enterprises are established, microfinance and related services are eventually provided through the equivalent of BRAC's primary village organizations. A goal of mainstreaming these clients into microfinance is to enable them to maintain and expand their businesses over time.

The TUP program works to develop human capital through the microenterprise training, as well as general education including functional literacy and improved health. BRAC provides the program participants (SUPs) with health services. BRAC staff including village health volunteers known as Shastho Shebikas provide training, basic care, and referrals. Financial assistance for illness is also provided. Direct services include child health, immunization, diarrheal disease control, vitamin A supplements for children under 5, tuberculosis control, and family planning services and pregnancy care. Yet another activity is to install tube-wells and sanitary latrines, which are expected to provide health benefits.

[12] This point can be seen clearly if we compare the subgroups that would result from perfect targeting in the sample of households in our data set. The differences between the treatment and the comparison groups are most pronounced in this case when compared to both the treatment-comparison pairs used for the analysis in this article (see table 3).

The program also seeks to build social capital through village support networks and sponsorship of community leaders for extremely poor women. The village support committees engage elites, often individuals who are known for public-spirited or religiously motivated charitable works. The committees are expected to assist the TUP participants when they are subjected to various types of shocks, such as by helping them to recover lost assets.

III. Data and Variables Description

For the empirical analysis, we use the BRAC TUP panel data set. This is a 2-year panel of about 5,000 households. The baseline survey of 5,626 households was done in 2002. In 2005, 5,288 households were resurveyed, along with 278 newly formed households that had split from the initial set of households. Attrition was moderate and was due to migration, death, and marriage. The final matched panel contained 5,067 households and was provided to us by BRAC's RED.[13]

The BRAC TUP panel data set provides information on a wide range of household characteristics and outcomes. The survey contains a rich body of information regarding the asset base of the household that includes physical (land, rickshaw vans, fishing nets), human (schooling, child labor, health), and financial (cash savings) capital. The data include information about basic needs (food security, clothing, and shoes/sandals), stock of household durables, and income. The data set also includes some information that can be used as indicators of women's empowerment, but it is limited in scope and likely to provide only an imperfect and partial view of women's status in a household.

The income variable we use includes both the level of income in the last year and the change in the value of assets over the last year. It is very important to include a measure of the change in the value of assets in this context because many households fatten livestock, and almost all their returns are realized only when they actually sell the livestock. The income variable is thus a comprehensive measure of household welfare, much more informative than the standard income variable that fails to account for changes due to activities such as livestock fattening. We use per capita income as the relevant measure of household welfare.

The cash savings is a binary variable that takes on the value of 1 if a household has cash savings in a given year.[14] Food security is measured by two in-

[13] We appreciate the suggestion made by an anonymous referee to investigate whether the attrition was systematically related to program participation. This is an important point, and it would have been an interesting area to explore. But unfortunately, we do not have access to the full data set in the baseline. We thus cannot pursue the panel attrition issue.

[14] It is important to note that most of the savings is not voluntary; it is part of the program. One might thus wonder whether it is useful to look at cash savings as an outcome. Our view is that it

dicators; the first is "food availability," which ranks food availability in a household among four possible outcomes: always deficit (1), deficit some times (2), neither deficit nor surplus (3), or food surplus (4). The second indicator is a binary measure called "meals twice a day" that takes on the value of 1 when the household members can have at least two meals a day, and 0 otherwise. The clothing variables refer to the main type of male and female clothing in Bangladesh: saris (female clothing) and lungis (male clothing). "Shoes" is a binary variable with a value of 1 when all the household members own shoes or sandals, and 0 otherwise.

The physical assets include livestock (cows/bulls, ducks/hens, etc.) and other productive assets (such as fishing nets, rickshaw vans, and "big trees"). The asset measures do not include any assets transferred from the TUP program.[15] Household durable goods include tube-wells as well as chairs, beds, radios, TVs, and quilts. Some of the durable goods such as beds, chairs, and quilts may be considered as basic needs of a household, and others such as TVs and radios are "luxury" goods, given the low level of income and assets of the households. There are two health indicators on subjective health conditions reported by the respondents. The "health status" variable asks the respondent to rank his or her perceived current health status given five options: excellent (5), very good (4), good (3), fair (2), poor/bad (1). The second health indicator is "health improvement," which ranks one's health compared to last year among five possible cases: much better than 1 year ago (5), somewhat better now (4), about the same (3), somewhat worse (2), much worse (1). Note that the subjective health indicators may have systematic measurement errors because the TUP program raises health awareness of the participants. Thus, a negative response might reflect the fact that an individual is more aware of the preexisting conditions, rather than a worsening health status.

As indicators of women's welfare and empowerment, we use the ratio of saris (female clothing) to lungis (male clothing), child labor among girls, and schooling of girls.[16] In Bangladeshi society, a sari is prized by women, and for the poor households, the ratio of saris to lungis is a reasonable indicator of

depends on what one is interested in. If the interest is to understand the changes in the economic lives of the poor due to participation in the TUP program, the cash savings is clearly an outcome of the program participation. But if one is interested in behavioral responses in savings due to the income effect of the program, the "forced" savings is clearly not informative.

[15] As pointed out by an anonymous referee, the assets transferred by the TUP program are part of the endowment of a household in 2005, and thus one should include them as part of the asset measures. However, information on the transferred assets in 2005 was not included in the data set.

[16] We thank an anonymous referee for suggesting child labor and schooling among girls as indicators of women's empowerment. Note, however, that the presence of child labor is one of the five inclusion criteria for TUP eligibility. This means that the treatment households are expected to have more incidence of child labor at the baseline by design.

relative expenditure on "feminine goods" in the household.[17] There is a large literature that uses relative expenditure on feminine goods as an indicator of female empowerment (see, e.g., Deaton 1989). These indicators of women's welfare and empowerment, although useful, are admittedly limited, and thus the conclusions about women's empowerment in this article should be treated with appropriate caution.

Our analysis covers both flow and stock variables, and one might plausibly argue that 3 years may not be enough to capture long-term effects of the program, and thus the evidence on the stock variables should be interpreted with appropriate caveats. It is possible that our analysis underestimates of the long-run effects of program participation on the stock variables.

Tables 1 and 2 present the descriptions and summary statistics of the relevant variables used in this article. One can see some interesting changes from 2002 to 2005 for the sample of households in the panel. There are significant improvements for an average household from 2002 to 2005 in terms of most of the indicators, including large gains in per capita income, food availability, housing (tin roof for main living room), livestock, and most of the other assets. There is, however, some evidence that the ownership of homestead land has worsened on average from 2002 to 2005.

IV. Empirical Strategy

Since BRAC did not incorporate any randomized control trials in TUP phase 1, we have to rely on the nonexperimental data to estimate program effects on household outcomes. To address potential bias due to the nonexperimental nature of the data, we use a two-pronged strategy: (1) we pay close attention to potential selection issues and use a rich set of econometrics techniques to tackle them, and (2) we use alternative treatment and comparison groups.

A. Alternative Treatment and Comparison Groups

Like many other microfinance programs, there is significant mistargeting (or assignment errors) in the TUP program. On the basis of the formal selection criteria of BRAC, one can partition the sample of households in the panel data set into four subsets. They are (i) households that are eligible according to the stated criteria and are included in the program (the subset called the "should be, one" group: SB_1), (ii) the eligible households not selected (called the "should be, zero" group: SB_0), (iii) households ineligible according to formal criteria but

[17] For middle class and richer households in Bangladesh, a particularly revealing indicator of "feminine good" is gold jewelry.

TABLE 1
VARIABLE DESCRIPTION

	Description
Income and savings:	
Per capita income	Summary variable for the response to "Last year employment and income related information— Increased net income in Tk" for the TUP member divided by household size
Cash savings (dummy)	Binary variable equal to 1 if the answer to the question "Do you have any cash savings?" is yes
Food security:	
Food availability	"What would you say the status of your household is in terms of food availability?" Always deficit (1), deficit some times (2), neither deficit nor surplus (3), food surplus (4)
Meals twice a day (dummy)	Binary variable equal to 1 when the answer to the following question is yes: "Could your household afford two meals per day most of the time during last year?"
Land and housing:	
Total land owned	Total amount of land owned by the household (in tenths of acres)
Own homestead land (dummy)	Binary variable that equals 1 if the household owns homestead land
Roof made of tin (dummy)	Binary variable that equals 1 if the material of household's main living room is tin (sign of good quality)
Clothing:	
Number of saris	Number of saris (female clothing) owned by the TUP member
Number of lungis	Number of lungis (male clothing) owned by the household head
Shoes (dummy)	Answer to the question "Do all household members have shoes/sandals?" yes (1), no (0)
Livestock:	
Number of cows/bulls	Number of assets owned, not including program transfers
Number of goats/sheep	Number of assets owned, not including program transfers
Number of ducks/hens	Number of assets owned, not including program transfers
Productive assets:	
Number of fishing nets	Number of assets owned, not including program transfers
Number of big trees	Number of assets owned, not including program transfers
Number of rickshaw vans	Number of assets owned, not including program transfers
Number of bicycles	Number of assets owned, not including program transfers
Household durables:	
Number of chairs/tables	Number of assets owned
Number of beds	Number of assets owned
Number of radios/TVs	Number of assets owned
Number of quilts/blankets	Number of assets owned
Number of tube-wells	Number of assets owned
Indicators of health:	
Health status	Answer to the following question: "How do you perceive your current health status?" Excellent (5), very good (4), good (3), fair (2), poor/bad (1)
Health improvement	Answer to "How do you consider your health compared to last year?" Much better than 1 year ago (5), somewhat better now (4), about the same (3), somewhat worse (2), much worse (1)
Indicators of female empowerment:	
Ratio of saris to lungis	Ratio of the female clothing to male clothing
Presence of female children working (dummy)	Presence of female child labor
Ability of female children to read and write a letter (dummy)	Binary variable equal to 1 when the child is a girl and the answer to the following question is yes: Can read and write letter (7+ years)

TABLE 1 (*Continued*)

	Description
Years of schooling of female children	Answer to the following question when the child is a girl: Education (5+ years)
Child labor:	
Presence of child labor (dummy)	Binary variable equal to 1 if the household declares that there are children under age 15 working
Observable characteristics:	
Household size	Size of household
Female working as day laborer (dummy)	Binary variable equal to 1 if the household female works as a day laborer
Sex of household head (dummy)	Binary variable equal to 1 if the household head is a female

Note. TUP = Targeting the Ultra-poor.

selected in the program (called the "should not be, one" group: SNB_1), and (iv) households ineligible and not selected (called the "should not be, zero" group: SNB_0). For details on the construction of these four subsets, please see the appendix. As discussed above, our empirical analysis is based on two pairs of treatment-comparison groups: BRAC's own SUP-NSUP and SB_1-SB_0.

B. Potential Selection Issues

There are two levels of selection issues for any given treatment-comparison pair: (i) BRAC's selection process and (ii) the participation decision by a household. As discussed earlier, BRAC's selection process was based on a set of explicit inclusion and exclusion criteria. To understand the nature of potential selection bias arising from BRAC's selection process, we need to have an implicit model of the actual decision making by the BRAC employees.

A simple but not implausible model is to assume that BRAC employees were following the set of inclusion and exclusion criteria, and thus the assignment errors discovered in the data are largely either due to randomness arising from human fallibility and other factors or due to the fact that some eligible households declined to participate in the program. If self-selection out of the program by eligible households is important, then households in the treatment group may systematically differ from the comparison group. This potential bias arising from self-selection out of the TUP program, however, is not likely to be important, as nonparticipation in phase 1 of this program by a selected ultrapoor was reported by BRAC to have been uncommon.

An alternative model is to assume that BRAC employees were using both the formal criteria and private information available to them. In this case, the objective function of the BRAC employees becomes relevant. If the objective was to identify the true ultrapoor, then the group of households who should have been in the program according to the set of formal criteria but were not selected (i.e., SB_0) must be relatively well off (more advantaged) in terms of

TABLE 2
SUMMARY STATISTICS

	Mean	SD	*N*
Income and savings:			
Per capita income:			
2002	2,660.30	2,258.69	5,035
2005	3,980.69	2,647.00	5,067
Cash savings (dummy):			
2002	.15	.35	5,067
2005	.61	.49	5,067
Food security:			
Food availability:			
2002	1.55	.63	5,067
2005	2.06	.78	5,067
Meals twice a day (dummy):			
2002	.60	.49	5,067
2005	.40	.49	5,067
Land and housing:			
Total land owned:			
2002	4.30	14.57	5,067
2005	4.36	15.11	5,067
Own homestead land (dummy):			
2002	.54	.50	5,067
2005	.53	.50	5,067
Roof made of tin (dummy):			
2002	.50	.50	5,067
2005	.78	.41	5,067
Clothing:			
Number of saris:			
2002	1.81	.59	5,067
2005	2.21	.82	5,067
Number of lungis:			
2002	1.75	.54	3,644
2005	1.59	1.25	5,067
Shoes (dummy):			
2002	.62	.48	5,067
2005	.90	.30	5,067
Livestock:			
Number of cows/bulls:			
2002	.11	.51	5,067
2005	.94	1.21	5,067
Number of goats/sheep:			
2002	.11	.49	5,067
2005	.34	.97	5,067
Number of ducks/hens:			
2002	1.15	2.83	5,067
2005	2.53	3.69	5,067
Productive assets:			
Number of fishing nets:			
2002	.00	.05	5,067
2005	.15	.60	5,067
Number of big trees:			
2002	.89	5.97	5,067
2005	.61	2.76	5,067
Number of rickshaw vans:			
2002	.03	.27	5,067
2005	.07	.28	5,067

TABLE 2 (*Continued*)

	Mean	SD	*N*
Number of bicycles:			
2002	.01	.08	5,067
2005	.02	.15	5,067
Household durables:			
Number of chairs/tables:			
2002	.37	.80	5,067
2005	.65	1.05	5,067
Number of beds:			
2002	.88	.73	5,067
2005	1.14	.76	5,067
Number of radios/TVs:			
2002	.01	.12	5,067
2005	.03	.18	5,067
Number of quilts/blankets:			
2002	.03	.21	5,067
2005	.16	.44	5,067
Number of tube-wells:			
2002	.03	.16	5,067
2005	.45	.50	5,067
Indicators of health:			
Health status:			
2002	2.32	.97	5,055
2005	2.50	1.07	5,013
Health improvement:			
2002	2.61	1.10	5,055
2005	2.93	1.06	5,013
Indicators of female empowerment:			
Ratio of saris to lungis:			
2002	1.11	.42	3,627
2005	1.03	.36	3,514
Presence of female children working (dummy):			
2002	.07	.26	5,067
2005	.11	.32	5,067
Ability of female children to read and write a letter (dummy):			
2002	.08	.27	5,067
2005	.07	.26	5,067
Years of schooling of female children:			
2002	.35	.48	5,067
2005	.23	.42	5,067
Child labor:			
Presence of child labor (dummy):			
2002	.15	.35	5,067
2005	.19	.39	5,067

initial economic conditions and characteristics in 2002. Under the alternative assumption that the objective was to identify and exclude potentially high-risk households so as to help ensure the "success" of the program, then the SB_0 group is likely to be systematically more disadvantaged in 2002. In the presence of heterogeneity among the BRAC employees, both positive and negative selections are likely to characterize our data set. The relevant issue is

whether such heterogeneity cancels out on average or whether there is either positive or negative selection in net terms. The evidence presented below is consistent with the interpretation that in net terms the negative selection dominates for both the treatment-comparison pairs; the treatment groups had systematically disadvantaged initial conditions in 2002.

Table 3 reports the differences in means and the associated standard errors for a set of observable characteristics in the baseline (2002) across different pairs of treatment-comparison groups. Column 1 gives the initial difference in means for SB_1-SB_0, column 2 for SUP-NSUP, column 3 for SNB_1-SNB_0, and column 4 for SB_1-SNB_0. The first striking feature in table 3 is that most of the entries are negative, implying that a treatment group in general exhibits adverse initial conditions compared to the respective comparison group. The fact that the treatment group SUP was systematically disadvantaged in 2002 indicates that, even in the presence of mistargeting, the TUP program participants are on average poorer households among the poor. As noted before, this also points to the possibility of negative selection biases under the plausible assumption that selection on unobservables is similar to the selection on observables (Altonji, Elder, and Taber 2005).

The evidence in table 3 also shows that the difference in the means is, in general, much lower for the treatment-comparison pair SB_1-SB_0. In contrast, there are some significant and relatively large differences in the initial conditions in 2002 between the treatment and the comparison groups as defined by BRAC (i.e., the subsets SUP and NSUP) and used by BRAC's RED in its "descriptive analysis" of the TUP program (Rabbani et al. 2006).[18] Consider, for example, total land and the number of cows and bulls owned by a household, which are among the most important household assets in rural Bangladesh. The differences in means for total land owned are -0.59 (SB_1-SB_0) and -3.94 (SUP-NSUP), and for the number of cows and bulls, they are -0.03 (SB_1-SB_0) and -0.15 (SUP-NSUP). The same pattern holds for most of the other variables in table 3.

It is reassuring that the subsamples SB_1 and SB_0 closely resemble each other according to the observable characteristics reported in table 3. It thus seems most appropriate to use SB_0 as the comparison group to estimate the effects of the TUP program on the treatment group SB_1.[19] To tackle any remaining selection biases, we use a battery of recent econometric approaches (see below).

[18] The standard errors reported in this article are clustered at the village level. There are 27 villages in the data set.

[19] Note that the treatment group SB_1 and the comparison group SNB_0 satisfy the BRAC inclusion and exclusion criteria perfectly. The difference in means in 2002 between these two groups is much more pronounced than the differences across SUP and NSUP (see table 3).

TABLE 3

TEST OF DIFFERENCES IN MEAN CHARACTERISTICS BETWEEN TREATMENT AND CONTROL GROUPS IN 2002

	SB_1-SB_0 (1)	SUP-NSUP (2)	SNB_1-SNB_0 (3)	SB_1-SNB_0 (4)
Income and savings:				
Per capita income	−106.75	−323.75***	−371.40***	−437.55***
	(94.30)	(63.62)	(86.12)	(87.96)
Cash savings (dummy)	−.01	−.12***	−.15***	−.19***
	(.01)	(.01)	(.01)	(.01)
Food security:				
Food availability	−.12***	−.29***	−.31***	−.42***
	(.03)	(.02)	(.02)	(.02)
Meals twice a day (dummy)	−.15***	−.18***	−.18***	−.22***
	(.02)	(.01)	(.02)	(.02)
Land and housing:				
Total land owned	−.59**	−3.94***	−4.69***	−5.73***
	(.21)	(.41)	(.62)	(.66)
Own homestead land (dummy)	−.08***	−.14***	−.12***	−.25***
	(.02)	(.01)	(.02)	(.02)
Roof made of tin (dummy)	−.09***	−.11***	−.13***	−.10***
	(.02)	(.01)	(.02)	(.02)
Clothing:				
Number of saris	−.05	−.15***	−.16***	−.23***
	(.03)	(.02)	(.02)	(.02)
Number of lungis	−.10*	−.13***	−.13***	−.16***
	(.05)	(.02)	(.02)	(.03)
Shoes (dummy)	−.06*	−.11***	−.12***	−.12***
	(.02)	(.01)	(.02)	(.02)
Livestock:				
Number of cows/bulls	−.03***	−.15***	−.17***	−.22***
	(.01)	(.01)	(.02)	(.02)
Number of goats/sheep	−.01	−.04**	−.03	−.09***
	(.02)	(.01)	(.02)	(.02)
Number of ducks/hens	−.05	−.57***	−.65***	−.96***
	(.09)	(.08)	(.11)	(.12)
Productive assets:				
Number of fishing nets	.00	.00	.00	.00
	(.00)	(.00)	(.00)	(.00)
Number of big trees	−.22	−.74***	−.85***	−1.04***
	(.11)	(.17)	(.25)	(.27)
Number of rickshaw vans	.01	−.03***	−.04***	−.04***
	(.01)	(.01)	(.01)	(.01)
Number of bicycles	.00	−.01***	−.01**	−.01**
	(.00)	(.00)	(.00)	(.00)
Household durables:				
Number of chairs/tables	−.12***	−.31***	−.33***	−.45***
	(.03)	(.02)	(.03)	(.03)
Number of beds	−.07*	−.26***	−.30***	−.38***
	(.03)	(.02)	(.03)	(.03)
Number of radios/TVs	.00	−.01**	−.01**	−.02***
	(.00)	(.00)	(.00)	(.01)
Number of quilts/blankets	.00	−.03***	−.04***	−.05***
	(.01)	(.01)	(.01)	(.01)
Number of tube-wells	−.01	−.02***	−.03***	−.02***
	(.01)	(.00)	(.01)	(.01)

TABLE 3 (*Continued*)

	SB_1-SB_0 (1)	SUP-NSUP (2)	SNB_1-SNB_0 (3)	SB_1-SNB_0 (4)
Indicators of health:				
Health status	.03	−.02	.02	−.17***
	(.05)	(.03)	(.03)	(.04)
Health improvement	.12*	.02	.04	−.14***
	(.05)	(.03)	(.04)	(.04)
Indicators of female empowerment:				
Ratio of saris to lungis	.03	−.01	−.01	−.02
	(.04)	(.01)	(.02)	(.02)
Presence of female children working (dummy)	−.02	.02*	.01	.09***
	(.02)	(.01)	(.01)	(.01)
Ability of female children to read and write a letter (dummy)	−.01	−.02**	−.03**	−.03**
	(.01)	(.01)	(.01)	(.01)
Years of schooling of female children	.04	.00	.02	−.07***
	(.03)	(.02)	(.02)	(.02)
Child labor:				
Presence of child labor (dummy)	−.02	.02*	−.02*	.17***
	(.02)	(.01)	(.01)	(.01)
Number of observations	1,727	5,067	3,340	3,137

Note. Standard errors in parentheses. SB_1 = should be, one; SB_0 = should be, zero; SUP = selected ultrapoor; NSUP = not selected ultrapoor; SNB_1 = should not be, one; SNB_0 = should not be, zero.
* $p < .1$.
** $p < .05$.
*** $p < .01$.

The evidence indicates that there are important differences in the initial conditions across the different treatment groups. Table 4 reports the group averages of a set of variables in 2002 across the treatment groups. Although the groups are similarly situated according to some outcomes such as food availability and quality of houses as indicated by the roof made of tin, the SB_1 group is the poorest among them. An average SUP household owns 35% more land than an average SB_1 household, and the SNB_1 households own more land than the SB_1 households. While the percentage of households who own their homestead land is 40% for the SB_1 group, the corresponding numbers for SUP and SNB_1 are 47% and 53%, respectively. The per capita income measure was Tk 2,452.63 for an average SB_1 household, Tk 2,488.62 for SUP, and Tk 2,518.78 for SNB_1. Given the above analysis, our focus is on the estimates of the average treatment effect on the treated (ATT), which is the average improvement due to treatment for those who actually were treated by the program, from two alternative pairs of treatment-comparison groups: SB_1 (treatment) and SB_0 (comparison) and SUP (treatment) and NSUP (comparison). Note that the SUP comprises both SB_1 and SNB_1, although we do not discuss separately the treatment effect estimates for the treatment group SNB_1, as it is composed of the richer households and thus clearly not the target group of the TUP program.

TABLE 4

MEAN VALUES OF OUTCOME VARIABLES IN 2002 FOR DIFFERENT TREATMENT GROUPS

	SB_1	SUP	SNB_1
Income and savings:			
Per capita income	2,452.63	2,488.62	2,518.78
	(1,896.60)	(2,078.63)	(2,219.92)
Cash savings (dummy)	.06	.08	.10
	(.01)	(.01)	(.01)
Food security:			
Food availability	1.33	1.39	1.44
	(.02)	(.01)	(.02)
Meals twice a day (dummy)	.48	.51	.53
	(.02)	(.01)	(.01)
Land and housing:			
Total land owned	1.64	2.21	2.68
	(.12)	(.11)	(.18)
Own homestead land (dummy)	.40	.47	.53
	(.01)	(.01)	(.01)
Roof made of tin (dummy)	.45	.44	.42
	(.02)	(.01)	(.01)
Clothing:			
Number of saris	1.70	1.73	1.76
	(.02)	(.01)	(.01)
Number of lungis	1.65	1.68	1.68
	(.03)	(.01)	(.02)
Shoes (dummy)	.56	.57	.57
	(.02)	(.01)	(.01)
Livestock:			
Number of cows/bulls	.01	.04	.06
	(.00)	(.01)	(.01)
Number of goats/sheep	.06	.09	.12
	(.01)	(.01)	(.02)
Number of ducks/hens	.67	.84	.99
	(.06)	(.04)	(.06)
Productive assets:			
Number of fishing nets	.00	.00	.00
	(.00)	(.00)	(.00)
Number of big trees	.40	.50	.58
	(.06)	(.06)	(.09)
Number of rickshaw vans	.01	.02	.02
	(.01)	(.01)	(.00)
Number of bicycles	.00	.00	.00
	(.00)	(.00)	(.00)
Household durables:			
Number of chairs/tables	.14	.21	.26
	(.01)	(.01)	(.02)
Number of beds	.70	.74	.78
	(.02)	(.01)	(.02)
Number of radios/TVs	.00	.01	.01
	(.00)	(.00)	(.00)
Number of quilts/blankets	.01	.02	.02
	(.00)	(.00)	(.00)
Number of tube-wells	.02	.01	.01
	(.00)	(.00)	(.00)

TABLE 4 (*Continued*)

	SB_1	SUP	SNB_1
Indicators of health:			
Health status	2.21	2.31	2.40
	(.03)	(.02)	(.03)
Health improvement	2.52	2.62	2.70
	(.03)	(.02)	(.03)
Indicators of female empowerment:			
Ratio of saris to lungis	1.10	1.11	1.11
	(.02)	(.01)	(.01)
Presence of female children working (dummy)	.13	.08	.04
	(.01)	(.01)	(.01)
Ability of female children to read and write a letter (dummy)	.07	.07	.07
	(.01)	(.01)	(.01)
Years of schooling of female children	.30	.35	.39
	(.02)	(.01)	(.02)
Child labor:			
Presence of child labor (dummy)	.26	.16	.07
	(.01)	(.01)	(.01)
Number of observations	1,086	2,375	1,289

Note. Standard errors in parentheses. SB_1 = should be, one; SUP = selected ultrapoor; SNB_1 = should not be, one.

C. *Econometric Approaches*

Difference-in-Difference Matching and Minimum Biased Propensity Score Reweighting Estimators

Following Heckman et al. (1998), we combine the difference-in-difference approach with matching (the DIDM estimator). As mentioned earlier, the DIDM approach purges any time-invariant additive heterogeneity at the individual level by time differencing (i.e., household fixed effects), and then matching on the preintervention characteristics takes care of selection on observables in a flexible way without imposing any particular functional form. This, however, may not adequately address the possibility that the estimated treatment effect may be contaminated by selection on unobservables that vary over time. The assumption that most unobservables are time invariant or change very slowly over time may not be appropriate in our context. The households are given training to use the transferred assets, and various forms of information (such as health) are provided on an ongoing basis by the TUP program. The sphere of their social interactions also broadens through the "village elders." The learning-by-doing effect may be substantial because many borrowers manage productive assets for the first time in their life.[20] Also, the

[20] The household fixed effects take care of the heterogeneity in the innate entrepreneurial ability but not the dynamic learning-by-doing effect. Also, it is possible that the entrepreneurs themselves do not know their ability and learn about it over time when they participate in the TUP program. For a theoretical model of credit market failure in entrepreneurial development in such cases of discovery of ability, see Emran and Stiglitz (2009).

BRAC field officers went through a learning process themselves, especially because it was the first phase of a new program. It is plausible that there is substantial heterogeneity in the ability of BRAC field officers, and this influenced the learning by the borrowers differentially over time.

We use two alternative approaches to address possible biases in the DIDM estimates. First, we implement the MB-IPW estimator developed by Millimet and Tchernis (2013). The MB-IPW estimator starts from the inverse probability weighted matching estimator developed by Hirano and Imbens (2001), which weights the observations on the treatment group by the probability of being treated (the inverse of the propensity score) and weights the observations in the control group by the probability of not being treated (i.e., 1 minus the inverse of the propensity score) and minimizes any possible bias in the estimates arising from the failure of the conditional independence assumption by using an appropriately trimmed sample around the bias-minimizing propensity score.[21] In our case, we used $\theta = 0.25$, which means that at least 25% of both the treatment and the control groups have a propensity score in the interval used (described below). Second, and more important, we provide estimates of the effects of the TUP program that address possible selection on unobservables (omitted-variables bias) by using the heteroskedasticity-based identification approach due to Klein and Vella (2009).[22]

Heteroskedasticity-Based Identification: The Klein and Vella Approach

There is now a substantial econometric literature that shows that in the absence of credible exclusion restrictions required for an IV strategy, one can use heteroskedasticity for identification (see Lewbel 1997, 2012; Rigobon 2003;

[21] Millimet and Tchernis call it the minimum bias (MB) estimator, but we find it informative to use the term minimum biased inverse probability weighted (MB-IPW) estimator because it builds on the IPW estimator due to Hirano and Imbens (2001).

[22] An increasingly popular approach to understanding the implications of violation of the conditional independence assumption in a matching estimator is to use sensitivity analysis following Aakvik (2001). An earlier version of this article reported results from such sensitivity analysis in the context of the DIDM estimator. We, however, chose to omit those results from the article because such a sensitivity analysis is based on an arbitrary and implausible assumption that selection on unobservables is always positive. Such a sensitivity analysis is of little value, if not counterproductive, for two reasons. First, in table 3 we provide evidence consistent with negative selection in our data set. The existing econometric evidence in fact supports the notion that selection may be negative in the case of the microfinance programs in Bangladesh (see, e.g., Pitt and Khandker 1998; Schroeder 2010). Also, the sign of the selection bias (or omitted-variables bias) is likely to depend on the outcome under focus. The evidence of negative selection provided by both Pitt and Khandker (1998) and Schroeder (2010) concerns household consumption as the relevant outcome. We are not aware of any formal evidence that shows positive selection into microfinance or TUP programs in Bangladesh. Second, sensitivity analysis using Rosenbaum bounds does not address the possible bias from measurement error.

Klein and Vella 2009, 2010). As noted by Rigobon (2003), analogous to the standard instrumental variables, heteroskedasticity can be understood as an exogenous "probabilistic shifter" of the endogenous treatment variable that helps us trace out the causal relation between a dependent variable (household outcomes such as food availability) and the endogenous treatment variable (microfinance membership). In recent papers, Klein and Vella (2009, 2010) show that when the treatment equation in a triangular model exhibits heteroskedasticity, this effectively induces an exclusion restriction even though there is no standard exclusion restriction available. Monte Carlo evidence from a number of recent studies shows that the Klein and Vella (K-V) approach is effective in correcting for the endogeneity bias and also the bias from measurement error (Ebbes, Wedel, and Boeckenholt 2009; Klein and Vella 2009, 2010; Millimet and Tchernis 2013).

To provide the intuition behind the approach, we consider the following triangular model:

$$\Delta Y_i = \alpha_0 + \alpha_i + \alpha M_i + X_i'\gamma_1 + \xi_i, \tag{1}$$

$$M_i = \mathbf{1}\{\beta_0 + \beta_i + X_i'\beta_1 + u_i > 0\}, \tag{2}$$

where ΔY_i is the change from 2002 to 2005 for an outcome of interest (such as income, asset, etc.) of household i, M_i is a dummy that equals 1 if household i is a participant of the TUP program and 0 otherwise, and α_i and β_i are household fixed effects. The focus is on the identification and estimation of the parameter α. The model does not impose any exclusion restrictions on equation (1), and identification of the causal effect α is not possible if the error terms are homoskedastic.[23] Assume that the error term in the treatment equation is heteroskedastic of the following form:

$$u_i = S_u(\tilde{X}_i'\pi)\tilde{u}_i, \tag{3}$$

[23] As noted before, one can argue that identification in the above model can be achieved without exclusion restrictions because the treatment equation is nonlinear. But such identification depends critically on the validity of the normality assumption and the nonlinearity of the normal cumulative distribution function. The model is in general poorly identified. For discussions, see Altonji et al. (2005) and Kelin and Vella (2009).

where $\tilde{u}_i$ is a 0 mean homoskedastic error, $\tilde{X}_i \subseteq X_i$ are the variables generating heteroskedasticity, and $S_u(\tilde{X}_i)$ is a positive and nonconstant function. In this case, the probability of treatment (probability of TUP membership) can be written as follows (ignoring the fixed effects):

$$\Pr(M_i = 1) = P\left(\frac{X_i'\beta_1}{S_u(\tilde{X}_i'\pi)}\right), \tag{4}$$

where $P(\cdot)$ is the distribution function for $\tilde{u}_{it}$. With homoskedastic errors, $S_u(\tilde{X}_i'\pi)$ is a constant, and the only source of identification is possible nonlinearity of the $P(\cdot)$ function such as a normal distribution. However, such identification based on the nonlinearity in the tails of the distribution may not be very useful because it relies on a small fraction of the data (for discussions on this point, see Altonji et al. 2005; Klein and Vella 2009). In contrast, when there is heteroskedasticity, $S_u(\tilde{X}_i'\pi)$ is not a constant function, and identification also exploits data from the region where $P(\cdot)$ is approximately linear. The predicted probability of treatment from estimating equation (2) above becomes a valid instrument in the presence of heteroskedasticity because it is no longer a linear combination of the control variables in the outcome equation (1). Note that if the amount of heteroskedasticity is not substantial, then there is little identifying variation in $S_u(\tilde{X}_i'\pi)$, and the predicted probability may be a weak instrument. For the specification of the $S_u(\tilde{X}_i'\pi)$, we follow the parametric approach developed in Farré et al. (2010), which is based on the model of heteroskedastic probit due to Harvey (1976):

$$S_u(\tilde{X}_i) = e^{\tilde{X}_i'\pi}. \tag{5}$$

A limitation of the heteroskedasticity-based identification is that it is not appropriate for the binary outcomes. Another disadvantage of the heteroskedasticity-based IV approach is that the estimates are likely to be less efficient than the usual IV estimates because identification here relies on information about the second moment of the data (Lewbel 2012).

V. Estimated Effects of the TUP Program

In this section, we report the estimated treatment effects on a set of household outcomes including income, land ownership, basic needs such as food availability and clothing, assets, child labor, schooling, and health-related indicators. As a benchmark, we report estimates from an augmented difference-in-difference specification.

We use the following difference-in-difference specification:

$$Y_{it} = \alpha_0 + \alpha_1 d_{05} + \alpha_{1R}(d_{05} \times d_R) + \alpha_{1K}(d_{05} \times d_K) + \alpha_2 d_T + X'_{02}\Pi + \beta(d_T \times d_{05}) + \varepsilon_{it}, \quad (6)$$

where Y_{it} is the outcome variable of interest for household i in year t, d_{05} is a dummy that equals 1 for the year 2005, and d_T is a dummy that equals 1 when household i belongs to an appropriately defined treatment group (i.e., SB_1, SUP) and equals 0 when a household belongs to the corresponding comparison group (i.e., SB_0, NSUP). The parameter of interest is β, which isolates the treatment effect on outcome Y under certain assumptions. The crucial difference-in-difference identification assumption is that the treatment and comparison groups would follow the same trend in the absence of the program. If this assumption is not satisfied, the estimate of the treatment effect $\hat{\beta}$ will be biased when we use ordinary least squares to estimate equation (1). Thus, we augment the difference-in-difference specification in two ways to make it more plausible that the counterfactual trend for the treatment group is well represented by the actual trend in the comparison group. First, we allow for differential time trends in the different districts in which the households are situated: d_R and d_K are dummies for Rangpur and Kurigram districts, respectively.[24] In addition, we allow for the possibility that the trends might differ across households with different observable characteristics. Thus, we also control for a set of observables that are likely to be important for selection into the treatment (either because of BRAC's criteria or the household's own outside option). Results are reported in column 1 of tables 5 (for the SUP group) and 6 (for SB_1).

The difference-in-difference estimates, although useful as a benchmark, do not fully exploit the panel dimension of the data and also rely on the assumption that the control variables do not have any nonlinear effects on the outcomes. To address these twin issues, we implement the DIDM approach with household fixed effects. The DIDM estimates are reported in column 2 of tables 5 (for SUP) and 6 (for SB_1). An important step in implementing the DIDM approach is to choose an appropriate set of observable characteristics that are likely to be important in determining the selection into treatment and that may also affect the outcome variables (Heckman and Navarro-Lozano 2004; Blundell and Costa-Dias 2009). As discussed above, we need to consider two levels of selection: BRAC's selection process and also the participation de-

[24] The omitted district is Nilphamari.

TABLE 5

EFFECTS OF THE TARGETING THE ULTRA-POOR (TUP) PROGRAM ON TREATMENT GROUP "SELECTED ULTRAPOOR" (SUP)

	DID (1)	DIDM (2)	MB-IPW (3)[a]	K-V (4)
Income and savings:				
Per capita income	1,126.69	1,067.81	932.58	1,377.06
	(110.72)***	(93.1)***	[678.86, 1,175.82]	(968.62)
Cash savings (dummy)	.83	.81	.81	
	(.01)***	(.01)***	[.79, .83]	
Food security:				
Food availability	.66	.65	.66	.99
	(.03)***	(.03)***	[.62, .71]	(.27)***
Meals twice a day (dummy)	.37	.37	.37	
	(.02)***	(.02)***	[.34, .40]	
Land and housing:				
Total land owned	1.41	−.10	−.07	10.94
	(.52)**	(.42)	[−.47, .28]	(13.14)
Own homestead land (dummy)	.09	.05	.05	
	(.01)***	(.02)***	[.02, .08]	
Roof made of tin (dummy)	.13	.13	.13	
	(.01)***	(.02)***	[.11, .16]	
Clothing:				
Number of saris	.29	.28	.28	.22
	(.03)***	(.03)***	[.23, .32]	(.16)
Number of lungis[b]	.06	.22	.20	.70
	(.04)	(.04)***	[.15, .25]	(.38)*
Shoes (dummy)	.15	.14	.14	
	(.01)***	(.02)***	[.11, .17]	
Livestock:				
Number of cows/bulls	1.74	1.71	1.71	1.31
	(.05)***	(.03)***	[1.67, 1.75]	(.41)***
Number of goats/sheep	.46	.45	.45	.36
	(.08)***	(.03)***	[.39, .50]	(.29)
Number of ducks/hens	.64	.56	.56	.34
	(.12)***	(.13)***	[.39, .73]	(1.30)
Productive assets:				
Number of fishing nets	.03	.05	.06	−.02
	(.01)*	(.02)***	[.03, .09]	(.24)
Number of big trees	.55	.24	.25	−1.14
	(.22)**	(.18)	[.09, .41]	(5.00)
Number of rickshaw vans	.04	.04	.04	.06
	(.01)***	(.01)***	[.02, .06]	(.08)
Number of bicycles	.01	.01	.01	.00
	(.01)	(.00)	[.00, .01]	(.04)
Household durables:				
Number of chairs/tables	.10	.11	.12	.08
	(.03)***	(.03)***	[.07, .17]	(.40)
Number of beds	.17	.16	.16	.21
	(.02)***	(.02)***	[.12, .20]	(.21)
Number of radios/TVs	.00	.01	.01	.14
	(.01)	(.01)	[.00, .02]	(.05)***
Number of quilts/blankets	.16	.16	.16	.44
	(.02)***	(.01)***	[.14, .18]	(.11)***
Number of tube-wells	.09	.14	.15	−.53
	(.02)***	(.02)***	[.12, .17]	(.13)***

TABLE 5 (*Continued*)

	DID (1)	DIDM (2)	MB-IPW (3)[a]	K-V (4)
Indicators of health:[c]				
Health status	.07	.06	.06	−.16
	(.04)	(.04)	[−.02, .12]	(.47)
Health improvement	.13	.11	.10	−.03
	(.06)**	(.05)***	[.04, .17]	(.28)
Indicators of female empowerment:				
Ratio of saris to lungis[d]	.03	.03	.02	−.21
	(.02)	(.02)	[.00, .04]	(.13)
Presence of female children working (dummy)	.00	.01	.01	
	(.01)	(.01)	[−.01, .02]	
Ability of female children to read and write a letter (dummy)	.00	.01	.01	
	(.01)	(.01)	[−.01, .03]	
Years of schooling of female children	.01	.02	.02	−.17
	(.01)	(.01)	[−.01, .03]	(.14)
Child labor:				
Presence of child labor (dummy)	.00	.03	.03	
	(.01)	(.01)*	[.00, .05]	
Number of observations	9,708	4,854	4,854	4,854

Note. Robust standard errors in parentheses are clustered at the village level. There are 24 clusters. Estimates in col. 1 come from the augmented difference-in-difference (DID) specification, and for the binary outcomes they correspond to marginal effects from probit regressions; in col. 2, from the difference-in-difference matching (DIDM) estimator due to Heckman, Ichimura, and Todd (1998); in col. 3, from the minimum biased inverse probability weighted (MB-IPW) estimator due to Millimet and Tchernis (2013); in col. 4, from the K-V (Klein and Vella 2009) identification through heteroskedasticity method. The variables included in the selection equation are household size, dummy if day labor activities, total amount of land owned, sex of household head (female = 1), and the five inclusion criteria (IC) established by Building Resources across Countries (IC1, <10 decimals of land [a tenth of an acre] including homestead; IC2, no male income earner at home; IC3, presence of child labor; IC4, female working outside the household; IC5, lack of productive assets). Kleibergen-Paap Wald *F*-statistic = 35.43; likelihood ratio (LR) test of heteroskedasticity in the probit model for selection into the TUP program = 30.89; *p*-value for the LR test = .00.

[a] 95% confidence intervals in brackets calculated by the bootstrap percentile method, using 250 replications.

[b] Number of observations is 8,343 in col. 1 and 3,489 in cols. 2–4.

[c] Number of observations is 9,647 in col. 1 and 4,795 in cols. 2–4.

[d] Number of observations is 6,845 in col. 1 and 3,230 in cols. 2–4.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

cisions of the households. We thus use observables that reflect these two levels of selection problems for matching. To account for the BRAC selection process, we use the TUP program's own set of inclusion criteria.

Moreover, we include indicators of physical and human capital (e.g., land owned, household size, gender dummy, and the indicator of women working as day laborers). As emphasized by Emran, Morshed, and Stiglitz (2007), the outside option of a household and thus the net return from participation in the TUP or other NGO programs depends on the nature of labor market

TABLE 6

EFFECTS OF THE TARGETING THE ULTRA-POOR (TUP) PROGRAM ON THE POOREST OF THE POOR, SB_1 TREATMENT GROUP

	DID (1)	DIDM (2)	MB-IPW (3)[a]	K-V (4)
Income and savings:				
Per capita income	1,313.12	1,325.01	1,328.36	1,779.59
	(159.82)***	(168.15)***	[684.40, 1,701.99]	(910.96)*
Cash savings (dummy)	.86	.80	.80	
	(.02)***	(.02)***	[.76, .84]	
Food security:				
Food availability	.67	.67	.69	.83
	(.04)***	(.05)***	[.60, .78]	(.38)**
Meals twice a day (dummy)	.36	.35	.36	
	(.04)***	(.03)***	[.29, .43]	
Land and housing:				
Total land owned	.04	−.06	−.07	2.45
	(.31)	(.37)	[−.73, .59]	(4.41)
Own homestead land (dummy)	.10	.07	.07	
	(.03)***	(.03)***	[.02, .12]	
Roof made of tin (dummy)	.13	.13	.13	
	(.02)***	(.03)***	[.08, .18]	
Clothing:				
Number of saris	.27	.26	.27	.69
	(.04)***	(.04)***	[.18, .34]	(.35)**
Number of lungis[b]	.08	.29	.16	−1.28
	(.07)	(.12)	[.01, .29]	(.80)
Shoes (dummy)	.10	.10	.09	
	(.02)***	(.03)***	[.05, .14]	
Livestock:				
Number of cows/bulls	1.69	1.68	1.69	1.57
	(.07)***	(.04)***	[1.62, 1.75]	(.31)***
Number of goats/sheep	.45	.46	.45	.59
	(.08)***	(.04)***	[.39, .52]	(.34)*
Number of ducks/hens	.60	.67	.62	.13
	(.15)***	(.17)***	[.33, .91]	(1.37)
Productive assets:				
Number of fishing nets	.02	.02	.03	−.06
	(.02)	(.02)	[−.01, .06]	(.12)
Number of big trees	.27	.25	.29	1.63
	(.18)	(.16)	[.04, .51]	(1.23)
Number of rickshaw vans	.01	.02	.01	.08
	(.01)	(.01)	[−.01, .04]	(.10)
Number of bicycles	−.00	−.00	.00	−.04
	(.01)	(.01)	[−.01, .01]	(.05)
Household durables:				
Number of chairs/tables	.13	.14	.15	.01
	(.04)***	(.05)***	[.08, .23]	(.42)
Number of beds	.16	.17	.17	.59
	(.04)***	(.04)***	[.10, .24]	(.34)*
Number of radios/TVs	.01	.01	.01	−.09
	(.01)	(.01)	[.00, .02]	(.06)*
Number of quilts/blankets	.21	.22	.21	.11
	(.02)***	(.02)***	[.18, .25]	(.19)
Number of tube-wells	.15	.16	.16	−.08
	(.03)***	(.02)***	[.12, .29]	(.23)

TABLE 6 (*Continued*)

	DID (1)	DIDM (2)	MB-IPW (3)[a]	K-V (4)
Indicators of health:[c]				
Health status	.11	.08	.09	.07
	(.05)**	(.07)	[−.04, .21]	(.60)
Health improvement	.11	.09	.09	.92
	(.06)*	(.08)	[−.04, .20]	(.77)
Indicators of female empowerment:				
Ratio of saris to lungis[d]	−.01	−.03	−.02	.56
	(.05)	(.06)	[−.26, .16]	(.48)
Presence of female children working (dummy)	.02	.01	.01	
	(.02)	(.02)	[−.03, .03]	
Ability of female children to read and write a letter (dummy)	−.01	−.00	.00	
	(.01)	(.02)	[−.03, .03]	
Years of schooling of female children	−.02	−.02	−.01	.13
	(.02)	(.03)	[−.05, .03]	(.19)
Child labor:				
Presence of child labor (dummy)	.02	.01	−.01	
	(.03)	(.03)	[−.04, .03]	
Number of observations	3,314	1,657	1,657	1,657

Note. Robust standard errors in parentheses are clustered at the village level. There are 23 clusters. Estimates in col. 1 come from the augmented difference-in-difference (DID) specification, and for the binary outcomes they correspond to marginal effects from probit regressions; in col. 2, from the difference-in-difference matching (DIDM) estimator due to Heckman, Ichimura, and Todd (1998); in col. 3, from the minimum biased inverse probability weighted (MB-IPW) estimator due to Millimet and Tchernis (2013); in col. 4, from the K-V (Klein and Vella 2009) identification through heteroskedasticity method. The variables included in the selection equation are household size, dummy if day labor activities, total amount of land owned, sex of household head (female = 1), and the five inclusion criteria (IC) established by Building Resources across Countries (IC1, <10 decimals of land [a tenth of an acre] including homestead; IC2, no male income earner at home; IC3, presence of child labor; IC4, female working outside the household; IC5, lack of productive assets). Kleibergen-Paap Wald F-statistic = 33.98; likelihood ratio (LR) test of heteroskedasticity in the probit model for selection into the TUP program = 18.45; p-value for the LR test = .03.

[a] 95% confidence intervals in brackets calculated by the bootstrap percentile method, using 250 replications.

[b] Number of observations is 2,217 in col. 1 and 560 in cols. 2–4.

[c] Number of observations is 3,301 in col. 1 and 1,644 in cols. 2–4.

[d] Number of observations is 1,002 in col. 1 and 373 in cols. 2–4.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

interactions and the shadow value of labor, especially of women's labor. In a perfect labor market, the labor and land endowments would not affect the incentives to take credit, given the wage rate (i.e., separability holds). In contrast, in an imperfect labor market, more labor endowment, ceteris paribus, implies a low shadow value of labor, and such a household would find it more attractive to borrow from microfinance NGOs. In effect, the microcredit allows a household to create demand for labor within the household; this is especially valuable when they cannot find employment outside, for example,

because of a high unemployment rate. We thus expect that the effect of labor endowment will be positive on the probability of joining into the microfinance programs. Similar arguments imply that the effects of higher land endowment will be negative, as more land, ceteris paribus, increases the shadow price of labor.

We include household size as an indicator of labor endowment of the household, and the variable "day labor" as a measure of labor market participation by women. We also include "land owned," as it is a crucial variable for the determination of the shadow price of labor and also whether a woman is excluded from the formal credit market (lack of collateral) and the labor market (efficiency wage effects). All of the matching variables are from the 2002 baseline survey. The estimated selection equations for the SUP and SB_1 treatment households are reported in table A1 in the appendix. The results from the probit regressions show that, for SUP, participation in/selection into the TUP program depends on a household's land and labor endowments. Consistent with the theoretical predictions of Emran et al. (2007), land has a negative—and labor endowment a positive—effect on the probability of participation in the TUP program. Also, among the five inclusion criteria, the fourth criterion (adult women working outside the home) and the fifth (no productive assets) have significant positive effects on the probability of selection into the program. The results from probit regressions for the SB_1 households show the importance of inclusion criteria 4 and 5 for their selection into the program. However, land and labor endowment do not have a significant effect in the case of the SB_1 households, although they bear theoretically consistent signs.

Another important issue in the implementation of any matching estimator is the common support. We chose the set of covariates so as to ensure ex ante a balanced matching quality, which we tested with a visual inspection of the density function of the propensity score in both treatment and control groups and with a formal test of differences of the mean propensity score for treatment and control. We imposed the common support region for each of our samples, SB_1/SB_0 and SUP/NSUP. In the SB_1/SB_0 group, the common support region is [0.22, 0.80] and used 1,656 of the 1,657 observations available. In the SUP/NSUP case, the common support region is [0.00, 0.78] and uses 4,842 of the 4,854 observations available. Thus, there is no selection in the information lost by the common support imposition. The results for the DIDM estimator reported in this article are based on the radius caliper algorithm for matching. However, note that the estimates from alternatives such as kernel matching are very similar (the results are available from the authors).

The estimates from MB-IPW estimator with household fixed effects are reported in column 3 of tables 5 and 6. We use a wide radius ($\theta = 0.25$) for

trimming the sample around the bias-minimizing propensity score,[25] as the Monte Carlo results in Millimet and Tchernis (2013) show that MB-IPW with a wide radius effectively corrects for endogeneity bias.

Finally, in column 4 of tables 5 and 6, we report estimates from the Klein and Vella (2009) estimator, with household fixed effects that correct for selection on unobservables and measurement error. They are our preferred estimates for continuous outcomes.[26] Following Klein and Vella (2009) and Farré et al. (2010), we include the full set of explanatory variables in the heteroskedastic probit specification, thus allowing for the possibility that potentially all of the variables can give rise to heteroskedasticity in the microfinance participation equation. An important issue here is whether the heteroskedasticity-based instrument derived from the heteroskedastic probit model is strong enough to identify the effects of the TUP program participation.[27] The likelihood ratio test for the null of homoskedasticity is convincingly rejected in both the samples; the p-value is .00 for SUP and .03 for SB_1. The Kleibergen-Paap F-statistics for SUP and SB_1 show that the heteroskedasticity-based instrument is strong enough to identify the effects of the TUP program participation; the Kleibergen-Paap F-statistic is 35.43 for SUP and 33.98 for SB_1, much higher than the Stock et al. rule-of-thumb value of 10 for one endogenous variable.[28]

A. Effects of the TUP Program on Treatment Group "Selected Ultrapoor"

We first consider the effects of the TUP program participation on BRAC's own treatment group SUP. Despite the fact that it includes relatively better-off households, the effects of the program on the SUP treatment group can be interesting; a comparison with the effects on the treatment group SB_1 that excludes the relatively better-off households can be particularly illuminating. Table 5 reports the results for the SUP treatment group; the standard errors are clustered at the village level (there are 27 villages in the data set).

The benchmark difference-in-difference (table 5, col. 1) estimates suggest that participation in the TUP program for an SUP household had positive effects on a number of outcomes including per capita income, cash savings, food

[25] In addition, the sample is trimmed around the 2% tails of the propensity score to minimize the bias arising from the failure of the conditional independence assumption.

[26] Because K-V is not suitable to use on binary outcomes, our preferred estimates in that case are the MB-IPW set.

[27] It is now widely appreciated in the literature that the IV estimates may be more biased than the ordinary least squares if the instrument is weak.

[28] Note that we do not report the estimates for the binary outcomes from the Klein and Vella (2009) estimator, as it not appropriate in such cases.

security, land ownership, quality of housing (with tin roof), livestock (cows and bulls, goats and sheep, and ducks and hens), other productive assets (e.g., number of big trees and fishing nets), and household durables (with the exception of radios/TVs).[29] However, there seems to be no significant effect on the indicators of female empowerment and child labor. Interestingly, the results indicate that TUP participation leads to improvements in women's clothing (number of saris), but there is no significant effect on men's clothing (lungis). Participation in the TUP program also improves the probability that all the members of a households have shoes/sandals. The evidence on health outcomes is conflicting—while there is improvement compared to the health status in the last year, we do not find evidence of an improvement in overall health status that can be attributed to the TUP program participation.

The estimates from DIDM (table 5, col. 2) and MB-IPW (col. 3) are very close to each other, with the point estimates numerically identical in many cases. While for most of the household outcomes columns 2 and 3 show similar results, the DIDM and the MB-IPW estimators reveal a few notable differences compared to the difference-in-difference estimates. Probably the most important is the estimated effect on the total land endowment of a household; the effect is negative and insignificant according to both DIDM and MB-IPW, while it is positive, numerically substantial (1.41), and statistically significant at the 5% level in the difference-in-difference specification in column 1. Moreover, the effects on the ownership of homestead land and number of big trees are much smaller according to the DIDM and the MB-IPW estimators when compared to the difference-in-difference estimate. In contrast, the effect on male clothing is more than three times larger and also statistically significant at the 1% level according to both the DIDM and the MB-IPW estimators. But it is numerically small and not statistically significant according to the difference-in-difference estimate.[30]

Column 4 in table 5 reports the estimates from the Klein and Vella (2009) approach, implemented with household fixed effects. The first thing to note is that the effect of TUP membership on per capita income of an SUP household is numerically much larger according to the heteroskedasticity-based IV estimate. But the standard error is also large, and the effect is not significant at the 10% level. A conservative interpretation of the results on income in table 5 is that the evidence is not very strong that TUP participation has a posi-

[29] The effect on fishing nets, rickshaw vans, and bicycles is numerically small.

[30] We again emphasize here that both the DIDM and the MB-IPW estimates are built on household fixed effects to take care of the time-invariant heterogeneity such as innate entrepreneurial ability.

tive effect on the income of an average SUP household. An alternative interpretation, which in our view is more plausible, is that, if anything, the evidence suggests that TUP membership increases income because all four estimates in table 5 are numerically large, and the large standard error of the K-V estimate probably reflects, at least partly, the inefficiency of the heteroskedasticity-based estimator noted earlier (see Lewbel 2012). This interpretation is also consistent with the robust evidence from the matching estimators of a positive effect on cash savings, ownership of homestead land, and quality of housing (tin roof) for a SUP household. The K-V estimate also strengthens the conclusion that TUP improves significantly the overall food availability (the effect is about 50% larger according to K-V). The TUP program effect is much larger for male clothing according to the K-V estimate; it increases from 0.22 (DIDM) to 0.70 (K-V) and is significant at the 10% level. The estimated effect for female clothing (saris) is somewhat smaller; it declines from 0.28 (DIDM and MB-IPW) to 0.22 (K-V), and it is no longer significant at the 10% level.

The effect on the number of cows/bulls a household owns seems to be very robust, but the evidence in favor of a substantial positive effect is much weaker according to the K-V estimates for the other livestock such as goats, sheep, ducks, and hens. The estimated effect for other livestock is numerically smaller compared to the estimates from DIDM and MB-IPW and statistically not significant at the 10% level. The K-V IV estimates for productive assets (fishing nets, big trees, rickshaw vans, and bicycles), a number of household durables (chairs, tables, and beds), self-reported health outcomes, child labor, and women's empowerment indicators also fail to show any significant effect of TUP membership. One should, however, be careful in interpreting the lack of a program effect on some of these outcomes. For example, the TUP program may not increase the number of fishing nets owned by a household, as fishing is not the main economic activity for most of the households in Bangladesh. Interestingly, the K-V estimates suggest that there is significant program impact on a set of household durables: radios, TVs, quilts, blankets, and tube-wells. While the effects on radios, TVs, and quilts/blankets are positive, the effect on tube-wells is negative according to the K-V estimate. The negative effect may appear puzzling. It might, however, be useful to recall that the asset measures (including tube-wells) in 2005 do not include the transfers from the TUP program. Thus, even though the TUP program might have installed many new tube-wells as part of the health intervention to ensure safe drinking water, our data do not capture this effect. This implies that if the TUP replaced some existing tube-wells (presumably not working properly), it would be measured as a negative program effect.

B. Effects of the TUP Program on the Poorest of the Poor: SB_1 Treatment Group

We begin with the benchmark difference-in-difference estimates reported in column 1 of table 6. Similar to the results on the SUP treatment group, the benchmark estimates suggest beneficial effects of TUP membership on a number of important household outcomes, including per capita income, ownership of homestead land, better quality of housing (tin roof), food security, livestock, and most of the household durables. Consistent with the effects on the SUP treatment group in table 5, the difference-in-difference results in table 6 also suggest that there is no significant effect of TUP membership on the SB_1 households regarding women's empowerment and child labor. However, there are two important differences between the results for SUP and SB_1. First, the effects on rickshaw vans and fishing nets are not significant at the 10% level for SB_1, although they were found significant for the SUP. Second, the program effect on health status is significant at the 5% level for SB_1, but it is numerically smaller and not significant at the 10% level for SUP.

The DIDM and the MB-IPW estimates for SB_1 are reported in columns 2 and 3, respectively, in table 6. The results from DIDM and MB-IPW estimators are, in general, close to the difference-in-difference estimates reported in column 1. Among the few exceptions are the number of big trees and two indicators of health. The effect on male clothing is much larger according to the matching and the IPW estimators. In contrast, the positive effect on the two health outcomes found in difference-in-difference results becomes smaller and statistically not significant according to both DIDM and MB-IPW.

Column 4 in table 6 reports the estimated program effects from the heteroskedasticity-based IV approach of Klein and Vella (2009) with household fixed effects for the SB_1 households. A comparison with the estimates from the DIDM and the MB-IPW estimators in columns 2 and 3 shows some interesting differences. The K-V estimate for per capita income is about 33% larger than the DIDM and the MB-IPW estimates and significant at the 10% level. The K-V estimate for food availability is consistent with the conclusion that participation in the TUP program enhances food security of an average SB_1 household. The magnitude of the program effect on "food availability" is about 20% larger, and it is significant at the 5% level.

The K-V estimates when combined with the estimates from the DIDM and the MB-IPW estimators provide strong evidence of a substantial and significant positive effect of TUP participation on livestock (cows, bulls, goats, and sheep). However, the positive and significant program impact indicated by the DIDM and the MB-IPW estimators is not supported by K-V results for a number of household outcomes such as male clothing, ducks and hens, and some of the durables (chairs, tables, quilts/blankets, tube-wells).

The K-V estimate of the effect on female clothing (saris) is numerically much larger than the estimates from the DIDM and the MB-IPW estimators, and it is also significant at the 5% level. The overall evidence thus is very robust that the TUP has a substantial positive effect on female clothing. But there is no evidence, according to the K-V estimates, that male clothing (lungis) or the ratio of female to male clothing (saris to lungis) is positively affected by the program participation in any appreciable manner.[31] Interestingly, the K-V IV estimates suggest that the TUP program participation increases productive investment such as livestock and big trees but reduces the expenditure on luxury items such as radios/TVs.[32] The results on female clothing and radios/TVs thus indicate that, in some cases, the matching estimators may underestimate the program effect. Applying the sensitivity analysis within a matching approach that arbitrarily assumes a positive selection effect (upward omitted-variables bias) would clearly be counterproductive in such cases.

C. *Comparison between the Treatment Groups and Broader Implications of the Empirical Results*

The evidence presented in tables 5 and 6 is interesting for two reasons: (i) the TUP program seems to have robust beneficial effects on food security, cash savings, and household assets such as livestock and housing for both the treatment groups, and (ii) the TUP program effect may be different for different treatment groups (e.g., on income and expenditure and on luxury goods). The evidence in tables 5 and 6 shows that, in many cases, the estimates of the program effects from difference-in-difference and matching estimators are consistent with those from alternatives such as heteroskedasticity-based identification. However, a comparison with the Klein and Vella (2009) data also suggests that the estimates from the difference-in-difference and matching estimators can potentially under- or overestimate the causal effects of a microfinance program. This is consistent with other recent evidence on the performance of the matching estimators (see, in particular, the evidence on the effects of a school breakfast program in Millimet and Tchernis [2013]).

The evidence strongly suggests that TUP participation confers significant benefits to a participant household, irrespective of the treatment group considered. A comparison of tables 5 and 6 shows that, for many outcomes, the estimated effects are similar between the two treatment groups, but this apparent similarity hides important differences. This is because of the fact that

[31] The ratio of saris to lungis is an indicator of women's bargaining power in household expenditure choices.

[32] The negative significant effect on radios/TVs is in contrast to the DIDM and the MB-IPW estimates that do not show any perceptible effect.

the households in two treatment groups start from very different initial conditions in 2002. Since households in the SB_1 treatment group are poorer with lower initial income and asset positions, the gains from participation when normalized by the initial conditions are, in general, much higher for the SB_1 households. For example, the mean stock of cows/bulls was 0.01 for SB_1 and 0.04 for SUP in 2002. The estimated program effect is 1.57 for SB_1 and 1.31 for SUP. If we express as proportion of the initial stock to get a measure of "normalized program effects," the program effects on cows/bulls become 157 times those of the initial stock for SB_1 and 32.75 times those of the initial stock for SUP. This dramatically illustrates the importance of taking into account the fact that the poorest of the poor start from much worse initial asset positions. We report the normalized program effects in tables 7 (for the SUP treatment group) and 8 (for the SB_1 treatment group). Had the program concentrated on the poorest of the poor, the program effects would have been larger than those found.

While all the participant households seem to invest in livestock and housing improvements, the poorest-of-the-poor households who are the target group of the TUP program (i.e., the SB_1 treatment group) do not spend their money

TABLE 7
NORMALIZED PROGRAM EFFECTS ON TREATMENT GROUP "SELECTED ULTRAPOOR" (SUP)

	DID (1)	DIDM (2)	MB-IPW (3)	K-V (4)
Income and savings:				
Per capita income	.45	.43	.37	.55
Cash savings (dummy)	10.21	9.96	9.93	
Food security:				
Food availability	.47	.47	.48	.71
Meals twice a day (dummy)	.73	.73	.73	
Land and housing:				
Total land owned	.64	−.05	−.03	4.96
Own homestead land (dummy)	.19	.11	.10	
Roof made of tin (dummy)	.30	.30	.30	
Clothing:				
Number of saris	.17	.16	.16	.13
Number of lungis	.04	.13	.12	.42
Shoes (dummy)	.27	.25	.24	
Livestock:				
Number of cows/bulls	49.15	48.31	48.36	37.01
Number of goats/sheep	4.94	4.83	4.78	3.87
Number of ducks/hens	.76	.67	.67	.40
Productive assets:				
Number of fishing nets	11.86	19.76	21.74	−7.91
Number of big trees	1.11	.48	.51	−2.30
Number of rickshaw vans	2.31	2.31	2.31	3.47
Number of bicycles	4.74	4.74	3.32	.00

TABLE 7 (*Continued*)

	DID (1)	DIDM (2)	MB-IPW (3)	K-V (4)
Household durables:				
Number of chairs/tables	.49	.54	.59	.39
Number of beds	.23	.22	.22	.28
Number of radios/TVs	.00	1.25	.75	17.50
Number of quilts/blankets	10.00	10.00	10.06	27.50
Number of tube-wells	6.12	9.52	9.93	−36.05
Indicators of health:				
Health status	.03	.03	.03	−.07
Health improvement	.05	.04	.04	−.01
Indicators of female empowerment:				
Ratio of saris to lungis	.03	.03	.02	−.19
Presence of female children working (dummy)	.00	.12	.10	
Ability of female children to read and write a letter (dummy)	.00	.14	.14	
Years of schooling of female children	.06	.06	.04	−.37
Child labor:				
Presence of child labor (dummy)	.00	.19	.17	

Note. DID = difference-in-difference; DIDM = difference-in-difference matching; MB-IPW = minimum biased inverse probability weighted; K-V = Klein and Vella.

TABLE 8

NORMALIZED PROGRAM EFFECTS ON THE POOREST OF THE POOR SB_1 TREATMENT GROUP

	DID (1)	DIDM (2)	MB-IPW (3)	K-V (4)
Income and savings:				
Per capita income	.54	.54	.54	.73
Cash savings (dummy)	15.58	14.49	14.49	
Food security:				
Food availability	.50	.50	.52	.62
Meals twice a day (dummy)	.75	.73	.75	
Land and housing:				
Total land owned	.02	−.04	−.04	1.49
Own homestead land (dummy)	.25	.18	.18	
Roof made of tin (dummy)	.29	.29	.29	
Clothing:				
Number of saris	.16	.15	.16	.41
Number of lungis	.05	.18	.10	−.77
Shoes (dummy)	.18	.18	.16	
Livestock:				
Number of cows/bulls	167.33	166.34	167.33	155.45
Number of goats/sheep	7.64	7.81	7.64	10.02
Number of ducks/hens	.90	1.00	.93	.19
Productive assets:				
Number of fishing nets	NA	NA	NA	NA
Number of big trees	.68	.63	.73	4.10
Number of rickshaw vans	.72	1.45	.72	5.80
Number of bicycles	.00	.00	.00	−21.74
Household durables:				
Number of chairs/tables	.95	1.02	1.09	.07
Number of beds	.23	.24	.24	.84
Number of radios/TVs	2.17	2.17	2.17	−19.57

TABLE 8 (*Continued*)

	DID (1)	DIDM (2)	MB-IPW (3)	K-V (4)
Number of quilts/blankets	19.09	20.00	19.09	10.00
Number of tube-wells	8.15	8.70	8.70	−4.35
Indicators of health:				
Health status	.05	.04	.04	.03
Health improvement	.04	.04	.04	.37
Indicators of female empowerment:				
Ratio of saris to lungis	−.01	−.03	−.02	.51
Presence of female children working (dummy)	.16	.08	.08	
Ability of female children to read and write a letter (dummy)	−.15	.00	.00	
Years of schooling of female children	−.07	−.07	−.03	.43
Child labor:				
Presence of child labor (dummy)	.08	.04	−.04	

Note. DID = difference-in-difference; DIDM = difference-in-difference matching; MB-IPW = minimum biased inverse probability weighted; K-V = Klein and Vella.

on luxury goods such as radios/TVs. The relatively better-off participants (SUP), in contrast, seem to use microcredit as a substitute for the missing markets for consumer credit; their expenditure on radios/TVs goes up significantly.

However, the evidence also suggests that the TUP participation does not have any significant effect on our indicators of women's empowerment, health, and child labor. The absence of an effect on child labor is consistent with a situation in which the income effect approximately cancels out the labor demand effect of microcredit through an increase in the marginal productivity of child labor.

VI. Conclusion

Using a two-period household-level panel data set, this article provides robust evidence on the effects of BRAC's Targeting Ultra-poor Program (TUP phase 1) on a set of important household outcomes for the ultrapoor. We use a battery of recent econometric approaches and alternative treatment-comparison groups to identify and estimate the effects of TUP program participation. In addition to BRAC's own treatment-comparison groups, we use the type 1 errors in assignment (mistargeting) in BRAC's screening to create an alternative treatment-comparison pair. This allows us to identify a treatment group composed of the poorest of the poor (i.e., ultrapoor) among the sample households and also an appropriate comparison group for this treatment group.

To estimate the effects of the TUP program, we use recently developed matching and propensity score reweighting estimators and a heteroskedasticity-based identification approach that takes into account selection on both observables and unobservables. The results show that there is significant impact of TUP

program participation on food security, cash savings, and livestock of the ultrapoor households. The evidence also indicates that the TUP program may not have any significant effects on health-related outcomes, women's empowerment, and some of the productive assets examined. When the differences in the initial conditions are taken into account, we find that the normalized program effects are significantly larger for the treatment group consisting of the poorest-of-the-poor households (i.e., SB_1). Had the program concentrated on the poorest of the poor, its effects would have been larger than those found.

An interesting finding from our analysis with potentially broad policy implications is that the effects of an ultrapoverty program on the poorest of the poor may be different from the effects on an average participant in the program when there is mistargeting. Compared to an average program participant, the poorest participants (i.e., SB_1) gain more in per capita income, invest more in productive assets such as cows/bulls and goats/sheep, and do not increase expenditure on luxury goods such as radios/TVs. This implies that the researchers should carefully define the treatment and comparison groups using program criteria, especially when there is significant mistargeting in a program. The substantive conclusions can differ depending on the treatment group under focus in an analysis. The evidence also indicates that the effectiveness of the TUP program would improve substantially with better targeting.

Appendix

Creating Variables for the Errors in Assignment Analysis

Initial eligibility for people living in poverty to join the program is based on selection at a meeting of the village, which designates households in the lower two socioeconomic strata, but among those selected as potentially eligible ultrapoor by the village, the NGO then selects participants according to three exclusion criteria and the presence of at least three out of five inclusion criteria (Noor et al. 2004, ix). The exclusion criteria are EC1 (the household is not a member of another NGO), EC2 (the household is not a recipient of a government welfare food distribution program), and EC3 (there is no female able to work in the household.)

We created our own designation of those eligible, using the survey data. To do so for the case of NGO membership, we used the responses to (i) whether the household had NGO savings (variable "ngos"; selected 340 observations), (ii) whether the household had a loan from an NGO (variable "ngoln"; selected 64 observations), (iii) whether the materials for the house wall and roof were provided by an NGO (variable tins1 = 3; selected 32 observations), (iv) whether the source of a loan was from an NGO (variable srln; selected 1 observation), and (v) whether the household was indicated as a member of more than one NGO (selected 23 observations). This classification selected

444 observations for 2002, of which 49 had been selected as SUP members for the program despite apparent ineligibility.

EC2 was composed of the following variables: (i) whether the household had government benefits (gprben1 = 2), which selected 28 observations; (ii) whether the main source of income was government benefits, for three primary sources (variables msoi1, msoi2, msoi3), which selected 3, 11, and 7 observations, respectively. This classification selected 127 observations, of which 35 had been selected as SUP members for the program.

To create EC3 we used the variable disab1, those women who presented a disability. This selected 48 observations, of which 22 previously had been selected as SUP members. Overall, according to the exclusion criteria, we identified 103 participants who were selected despite being ineligible.

With respect to the inclusion criteria, the household had to meet at least three out of five conditions in order to be considered for the TUP program. They were IC1, owning less than 10 decimals of land (a tenth of an acre), including homestead; IC2, no male income earner at home; IC3, children of school-age working; IC4, adult women of household selling labor outside homestead; and IC5, household having no productive assets.

With respect to IC1, we created a dummy variable for whether the household owns self-cultivated land, owns land that others cultivate, owns homestead land, or owns land that is uncultivated. This criterion selects (as eligible) 4,624 out of the 5,067 for 2002, of which 2,279 had been selected for SUP.

For IC2, we first created a dummy variable for the presence of no male income earner at home, as the intersection of males of working age (more than 14 years old) who are not working. There are 66 observations that fulfill this criterion, of which 27 already had been selected as SUP. The second auxiliary variable constructed was a dummy for the presence of no male at home (additional to the previous one, no male earner). This variable selects 1,893 observations, of which 1,085 had been selected for SUP participation.

For IC3, we used questionnaire data, which selected 740 observations, of which 372 had been selected as an SUP. For IC4, we selected those observations for which the main source of income (for the first three primary occupations) was 5 = day labor (agriculture), 6 = day labor (nonagriculture), 7 = small business/trading, 9 = begging, 10 = servant, 11 = professional. This selected 1,627 observations, of which 994 had been selected already as SUP.

For IC5, we used the dummy variable "prodasst," which selected 2,791 observations, of which 1,520 were already SUP members. Finally, to construct the inclusion criteria, we considered those observations that fulfilled at least three out of the five conditions. According to these data, there were 1,727 observations that should have been classified as SUP, of which 641 were not.

According to the exclusion and inclusion criteria, we have created the following groups: SB_1 (selected as SUP and fulfilling both inclusion and exclusion criteria), composed of 1,086 observations; SNB_1 (selected as SUP but not fulfilling the criteria), composed of 1,289 observations; SNB_0 (correctly not selected as SUP, criteria not met), composed of 2,051 observations; and SB_0 (not selected as SUP but fulfilling the criteria), with 641 observations.

TABLE A1

PROBIT REGRESSIONS FOR SELECTION INTO THE TARGETING THE ULTRA-POOR (TUP) PROGRAM

	SUP/NSUP	SB_1/SB_0
Household size	.03	.04
	(.01)**	(.02)
Dummy if day labor activities	.14	.04
	(.04)***	(.07)
Total land owned	−.02	−.01
	(.00)***	(.01)
Sex of household head (female = 1)	.04	.23
	(.20)	(.26)
IC1: <10 decimals of land (a tenth of an acre) including homestead	.27	.29
	(.11)**	(.30)
IC2: No male income earner at home	−.07	−.36
	(.16)	(.23)
IC3: Presence of child labor	.05	.02
	(.06)	(.09)
IC4: Female working outside the household	.57	.52
	(.11)***	(.16)***
IC5: Lack of productive assets	.34	.21
	(.04)***	(.07)***
Constant	−.81	−.49
	(.13)***	(.37)
Number of observations	4,854	1,657

Note. Standard errors in parentheses. Covariates are 2002 preintervention characteristics. SUP = selected ultrapoor; NSUP = not selected ultrapoor; SB_1 = should be, one; SB_0 = should be, zero; IC = inclusion criterion; IC1–IC5 are the five Building Resources across Countries criteria for inclusion into the TUP program.

** $p < .05$.

*** $p < .01$.

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