HEIDELBERG UNIVERSITY
DEPARTMENT OF ECONOMICS

UNIVERSITÄT HEIDELBERG
ZUKUNFT SEIT 1386

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Michael Hillebrecht

Stefan Klonner

Noraogo A. Pacere

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Dynamic Properties of Poverty Targeting*

Michael Hillebrecht[a], Stefan Klonner[b] [1], Noraogo A. Pacere[c]

Abstract:

A body of recent studies has compared the ability of proxy-means testing (PMT), a data-driven poverty targeting procedure, and community-based targeting (CBT), a participatory method, to identify consumption-poor households. Motivated by the facts that targeted benefits typically reach beneficiaries with a substantial time lag and that transitions into and out of poverty are frequent, we are first to assess PMT's and CBT's performance one and two years subsequent to the targeting exercise. With data from Burkina Faso, we replicate the finding that PMT targets more accurately than CBT with respect to poverty at baseline, by 14 percent. We find that this pattern is reversed for households' poverty status twelve months later, while both methods perform identically with respect to poverty data collected 30 months after the baseline. We investigate how communities process different kinds of information and identify three properties of CBT that make it forward-looking: implicit weights put on PMT variables that predict future rather than current consumption, accounting for additional household characteristics not included in typical PMTs and processing of additional information unobserved by the researcher.

Keywords: Poverty targeting; targeting performance; proxy-means tests; community-based targeting

JEL classification: I13, I38, O15

[1] Corresponding author, email: klonner@uni-heidelberg.de

[a] Institute of Global Health, Heidelberg University, Im Neuenheimer Feld 324, 69120 Heidelberg, Germany

[b] South Asia Institute and Department of Economics, Heidelberg University, Vossstrasse. 2, 69115 Heidelberg, Germany

[c] Centre de Recherche en Santé de Nouna, Ministère de la Santé, BP 02 Nouna, Rue Namory Keita, Sect. 6 Nouna, Burkina Faso

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1 Introduction

Targeted poverty reduction programs have been an important policy tool for achieving the global community's development targets and inclusive growth in numerous low- and middle-income countries (World Bank 2018). When poverty reduction under a limited budget is the purpose of such an intervention, its success crucially depends on the ability of the targeting process to identify the poor (Ravallion 1993). When a government lacks data on households' incomes ('means'), poverty targeting most often involves proxy-means tests (PMTs), where easily verifiable household characteristics are aggregated into an index. A fundamentally different but also common approach is to delegate the identification of beneficiaries to local communities, which is known as community-based targeting (CBT) (Ravallion 2003).

Previous work mentions lower costs and higher satisfaction rates as advantages of CBT because target populations are given agency in the targeting process, thereby improving ownership and sustainability of the underlying program at the local level. On the other hand, CBT may be more susceptible to capture by local elites (Basurto, Dupas, and Robinson 2020) and its outcomes may depend on network structures in the communities (Alatas et al. 2016a). PMTs, in contrast, are less vulnerable to principal-agent problems and have the advantage of being easy to replicate (Ravallion 1993).

Within the vast literature on targeting of anti-poverty programs, there is a rapidly growing branch on the topic of targeting performance, which is assessed in terms of the overlap of the set of beneficiary households selected by a particular targeting method with the set of consumption-poor households. The latter are typically identified through a household survey which is administered alongside the targeting exercise. Regarding targeting performance comparisons of CBT and PMT, the pioneering study by Alatas et al. (2012) as well as a number of more recent papers (Sabates-Wheeler, Hurrell, and Devereux 2015; Karlan and Thuysbaert 2016; Stoeffler, Mills, and del Ninno 2016; Hillebrecht et al. 2020) all find modest to large advantages of proxy-means testing over participatory targeting methods.

Our research on the targeting performance of community-based targeting and proxy-means testing presented in this paper is motivated by two important stylized facts applying to the bulk of targeted anti-poverty programs. First, intentionally or unintentionally, program benefits typically reach eligible households with a substantial time lag, often a year or more after the targeting exercise is accomplished.[2] Moreover, benefits are often granted over an extended time period, sometimes several successive years after the beneficiary status has been awarded.[3] Second, even in the absence of social assistance programs, poverty has been

[2] The time lag has been a moderate six months in Indonesia's BLT program studied by Alatas et al. (2012), about one year for the consumption support programs studied by Karlan and Thysbaert (2016), and two years for Cameroon's SSNPP studied by Stoeffler, Mills, and del Ninno (2016).

[3] Indonesia's original BLT program paid four installments over 15 months (World Bank 2012). In the graduation programs covered by Banerjee et al. (2015), which include the two covered by Karlan and Thysbaert (2016), as well as in Cameroon's SSNPP (World Bank 2013) beneficiary households received consumption support for up to two years.

shown to be a highly dynamic rather than a static phenomenon in various country contexts, with rapid and frequent transitions in and out of poverty (Lee, Ridder, and Strauss 2017).

In our view, the poverty impact of a targeted welfare program is maximized when benefits accrue to households which are most deserving at the time benefits reach, rather than when the targeting exercise takes place. In all existing studies of targeting performance, however, household surveys for identifying consumption-poor households are fielded well before program benefits reach the targeted households, typically around the time of the targeting exercise.[4] Moreover, Alatas et al. (2012) use contemporaneous consumption data to simulate poverty impacts of the different targeting methods, which is misleading if the sets of poor households at the time of the consumption survey and the accrual of benefits differ systematically across targeting methods. That such a pattern is not unlikely is supported by the observation that community targeting choices incorporate household characteristics beyond those typically included in a PMT (Alatas et al. 2012).

In this paper, we carry out the first *dynamic* analysis of poverty targeting. We investigate the overlap of sets of targeted households with sets of consumption-poor households where consumption is measured in several consecutive years following the targeting exercise – the time when welfare benefits typically accrue in practice. Our focus is on the trajectory of PMT's and CBT's comparative targeting performance over time. The empirical setting of our study is a participatory targeting exercise aimed at identifying recipients of discounted micro-health insurance vouchers in 41 villages and 22 urban neighborhoods in Burkina Faso in 2007. In parallel, we fielded a panel living standards survey in 2007, 2008 and 2009, which serves two purposes. First, we carry out a hypothetical PMT for the same year in which the actual participatory targeting exercise took place. Second, we calculate the overlap of the sets of beneficiaries of each of the two targeting methods with the sets of consumption-poor households at baseline as well as 12 and 30 months later for both an absolute and a relative poverty concept. While program benefits accruing to targeted households during the years following the baseline fundamentally challenge targeting performance assessments subsequent to the baseline – because they may help households to escape poverty – we provide evidence that the rather indirect and small economic benefit of our intervention is unlikely to affect consumption and a household's poverty trajectory, making our setting particularly suited to learn about the dynamics of targeting performance.

Consistent with previous studies, we find that PMT targets households which are consumption-poor at the time of the targeting exercise more accurately than CBT. On the other hand, this advantage of PMT is reversed in consumption data collected twelve months later: according to our preferred poverty concept, PMT's targeting error rate exceeds CBT's by twelve percent. Another 18 months later, neither method is at an advantage with differences in error rates of less than two percent.

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[4] In Alatas, Banerjee, and Hanna (2012), Karlan and Thysbaert (2016), and Sabates-Wheeler, Hurrell, and Devereux (2015), the targeting exercises and the consumption survey, which the researchers use for identifying poor households, are simultaneous; in Stoeffler, Mills, and del Ninno (2016) the CBT exercise precedes the consumption survey by twelve months.

An important implication of this finding is that communities' poverty assessments are more forward-looking than conventional proxy-means tests – which by construction maximize targeting performance at baseline. In additional, novel analyses, we explore several potential sources of CBT's dynamic advantage. We find evidence for the following three. First, communities put different implicit weights on the variables included in a typical PMT than a proxy-means test calibrated by baseline consumption, and these weights are better suited to predict future rather than current consumption. Second, in their targeting choices, communities factor in additional household characteristics which are typically not included in PMTs, such as the possession of movable assets. Finally, we provide evidence that communities process additional information not easily observed by the researcher: relative to 26 household characteristics recorded at baseline, the community targeting decision improves poverty predictions one year subsequent to the baseline by 25 percent, while there is no such improvement for poverty at baseline.

These findings qualify in an important way the main conclusions of the extant literature on what we shall call comparative *static* targeting performance, where the targeting exercise and the collection of consumption data take place concurrently. Like us, all these authors find significant advantages of PMTs over CBT at baseline, often of around 20 percent in magnitude (Alatas et al. 2012; Sabates-Wheeler, Hurrell, and Devereux 2015; Karlan and Thuysbaert 2016; Hillebrecht et al. 2020). Our results show, on the other hand, that this advantage might be practically irrelevant and in fact misleading as the performance ranking of the two methods may be reversed at the time program benefits typically reach.

Our findings also carry important practical implications. Relative to participatory methods, proxy-means testing is costly and requires the collection and processing of large amounts of data. Nonetheless, Alatas et al. (2012) argue that PMT's superior targeting performance compensates for the higher targeting costs. When the performance advantage of PMT quickly melts away as time passes, as we document here, participatory targeting methods become more attractive than previously thought. Indeed, our findings entail no advantage of proxy-means testing at all when program benefits reach with a time lag of one year and more.

The remainder of this paper is structured as follows. Section 2 describes the context and the CBT intervention in some detail. In section 3 we introduce our data. Section 4 sets out our empirical approach. Section 5 contains the results and the last section concludes.

2 The Targeting Intervention

2.1 Background

The setting of our study is the administrative department of Nouna in the northwest of Burkina Faso. At the time of the national census preceding our field campaign, in 2006, it was inhabited by a population of about 70,000 of which 72 percent resided in 59 villages and the rest in Nouna Town, the department's only urban settlement. With this makeup, the study area is similar to the country as a whole, where 22 percent of the population have resided in urban settlements in 2014 (UN-DESA 2015).[5]

According to the country's national statistical office, consumption poverty evaluated at the national poverty line, which is close to the World Bank's dollar a day (in 1990), stood at 47 percent in the department of Nouna in 2009, which equals precisely the national figure for that year (INSD 2015). A continuous collection of vital statistics and later also of sample survey data by a local, government-funded health research center, the *Centre de Recherche en Santé de Nouna*, *CRSN*, and Heidelberg University has been ongoing in the Nouna department since 1993 (DeAllegri 2006).

With the objective of developing a nation-wide voluntary health insurance scheme, the Burkinabé Ministry of Health decided to explore the potential of voluntary community-based health insurance during the early 2000s (Fink et al. 2013). The Nouna area was chosen as the pilot site because of the statistical monitoring systems already in place. The roll-out of the insurance scheme commenced in 2004 and since 2006 all households in the study area have had the opportunity to purchase community-based health insurance from a formal not-for-profit provider, the *Assurance Maladie à Base Communautaire*, *AMBC*, sponsored by the central government.

Despite the seemingly affordable insurance premium, health insurance enrollment rates had remained far below expected levels. As enrollment rates were especially low among poor households (Souares et al. 2010), in 2007 a fifty percent discount on the premium, essentially a voucher, was offered to the poorest 30 percent of households in each village and urban neighborhood.[6]

2.2 Community-Based Targeting

For the identification of beneficiary households, a CBT exercise was carried out in 39 villages as well as in Nouna Town, which was partitioned into 23 neighborhoods of similar size for this purpose. In what follows, we shall refer to both villages and urban neighborhoods as 'communities'. In each community, the procedure started with a publicly convened community meeting, where the facilitators first informed about the purpose of the meeting.

[5] Some of the material in this section draws on our previous study of targeting performance of several statistical targeting procedures and CBT in Nouna (Hillebrecht et al. 2020). While the geographical context is the same, our previous paper is entirely in the tradition of the literature on static targeting performance. Moreover, Hillebrecht et al. (2020) consider a CBT carried out in 2009 with different rules, and their PMT builds on an economic census fielded in the same year. In the present paper, the focus is on the first CBT in Nouna, in 2007, because the panel household survey, which serves to evaluate the dynamic targeting performance (see below), was collected in an uninterrupted fashion only until 2009. In terms of data, the only overlap between Hillebrecht et al. (2020) and the present paper is the 2009 wave of the household survey. We repeat some of the description of the geographical and institutional background here rather than referring to Hillebrecht et al. (2020) for the sake of providing a self-contained exposition.

[6] The original objective was to make the health insurance vouchers available to the 20 percent poorest households in each community. However, after discussion with community leaders and key informants, who opined that 30 percent is a more realistic absolute poverty rate, the program was expanded to reach the 30 percent poorest households of each community.

The facilitators initiated a focus group discussion to elicit criteria regarding poverty and wealth. The community was then instructed to use these criteria for defining three or four wealth brackets. In a third step, the community assembly elected three local key informants by acclamation. Physically separated from the assembly and each other, each key informant first assigned each household from a comprehensive household listing that had been compiled in advance by CRSN staff to one of the previously defined wealth brackets and, second, ordered all households in the two lowest brackets. This was implemented by giving each key informant a deck of cards, with one household on each card. The number of households eligible in a community, *m* say, was fixed in advance by us and set equal to 0.3 times the number of ranked households. This number was communicated to each key informant after completion of the sorting task, who would then deliver a deck of *m* cards as his choice of beneficiary households.

The set of beneficiary households was then determined by the following algorithm: First, all households nominated by all three informants are automatically eligible. Second, all households nominated by exactly two informants are included if the resulting number of beneficiaries does not exceed *m*, and, in a consultation among the key informants, the remaining beneficiary households are chosen from the set of households nominated by exactly one informant previously. Otherwise, only a subset of the households nominated by exactly two informants is chosen in a consultation among the key informants. On average, the entire exercise took half a day per community.

To assess how well CBT identifies consumption-poor households at the time when program benefits typically reach, in an ideal experiment the researcher would observe targeted and not-targeted households' poverty status in the absence of program benefits during each of several years following the targeting exercise. Since we considered withholding program benefits from targeted households unethical, our research design instead features the following elements that make us confident that our repeated survey consumption measurements are largely unaffected by the health insurance vouchers, which were given to CBT-targeted households after the baseline and remained valid for two years, until mid-2009. First, while we formulated the community targeting exercise's objective explicitly "to identify the poorest *m* households", the program benefit is not a consumption support through income or asset transfers (as in Alatas et al. (2012); Banerjee et al. (2015); or Stoeffler, Mills, and del Ninno (2016)), which have been shown to have sizable immediate effects on consumption poverty. Instead, beneficiary households obtain a voucher for discounted health insurance, where the insurance product is also available to households which are not targeted, albeit at an unsubsidized rate. Second, an actual economic benefit accrues to only a small subset of targeted households because enrollment in the insurance program is voluntary and insurance demand among targeted households increased only marginally in response to the subsidization campaign, by merely 1.1 percentage points among eligible households, relative to an average enrolment rate of 7.3 percent in 2007 (Parmar et al. 2012). Third, for targeted households that chose to enroll, the implied transfer benefit is relatively minor, around CFA 600 per person and year (roughly $1.20, not purchasing-power-parity adjusted), which compares to average annual consumption expenditures of around CFA 60,000. Finally, previous work on Nouna's AMBC has found significant overall effects on neither health nor health-care-related out-of-pocket expenditures (Fink et al. 2013).

3 Data

Our first source of data is the CBT data set, which contains the beneficiary status of 6,148 households. The second data source is a demographic census, which is updated annually. It contains demographic characteristics of households including educational attainments, which we employ for the proxy-means tests. We use only data recorded for 2007, our baseline year. The third source of data is a representative, annual panel household survey, which contains information on dwelling characteristics, asset possessions, health and consumption. The survey waves we use in our analyses were fielded during the second quarter of 2007 and 2008, and during the last quarter of 2009, the last wave of this survey with the same households interviewed in 2007. We detail the calculation of monthly per capita consumption from this survey, the key variable to determine the poverty status of households in each of the three years, in the online appendix. Since we will use all 833 households covered by this survey in 2007 to calibrate the proxy-means test, which we will describe in detail below, we will refer to this sample as *calibration sample* in the sequel.

While the household survey and demographic census build on the same household roster from 2003, a complication in our data is that the CBT does not consistently identify households by their 2003 roster ID and therefore we cannot locate all survey households in the CBT data set, especially in the urban neighborhoods. Our assessments of targeting performance, however, will be based on the survey households that we were able to locate in the CBT data set, 675 in 2007, to which we will refer as the *evaluation sample*. Over the two subsequent years, there is some attrition in this sample, on the order of five percent, which is mainly due to some households' reluctance to respond to the annual survey, which had started in 2003, for a sixth and seventh time.

To assess whether the evaluation sample is representative of Nouna's population at large, we set out sample means for consumption and poverty, whose calculation we detail in section 4, separately for the calibration and evaluation samples in table 1. While average monthly per capita consumption expenditures (MPCE) among urban households are consistently larger in the calibration sample, by 8 to 17 percent, none of the respective differences is statistically significant – due to large variation in this measure. On the other hand, all poverty figures, which are calculated from the same poverty lines for the two samples, are very similar and statistically indistinguishable at conventional levels. In addition, table A1 contains sample means for fifteen baseline covariates for both the calibration and the evaluation sample. As for consumption and poverty, the respective means are virtually indistinguishable in the two samples. In panel 2 of table 1 we also report the share of households targeted by CBT, which stands at precisely 30 percent in the CBT data set. While such households are slightly over-represented in the urban evaluation sample, the difference to 0.3 is less than one standard error for all three years. Taken together, these findings give us confidence that our evaluation sample is sufficiently representative of Nouna's population at large.

4 Methodological Approach

4.1 The Proxy-Means Test

A proxy-means test typically employs a large set of household characteristics, often collected in a census-like field campaign, which we shall call poverty census here, and maps them into a single index. Weights are obtained from a regression of per capita consumption, usually in logarithmic form, on these proxy-means variables with an additional sample survey data set that contains consumption as well as the proxy variables, which is typically collected previously (Filmer and Scott 2012).[7] The regression coefficients are then used as weights to calculate a PMT score for each household from the poverty census. Hence, for a given household, the resulting score essentially equals (a linear transformation of) its predicted logarithmic consumption in a linear regression sense (Brown, Ravallion, and van de Walle 2018).

In our study context, no previous independent survey is available for estimating (or "calibrating") PMT weights for the department of Nouna. For our hypothetical PMT, we therefore adopt the approach of Brown, Ravallion, and van de Walle (2018) and use the same data set for calibrating the PMT, all 823 households included in the 2007 wave of our household survey, and for evaluating targeting performance in 2007, 675 households which we were able to identify in the CBT data (see section 3 for details). We carry out all steps for the PMT exercise separately for rural and urban households.

Building on the "basic PMT" of Brown, Ravallion, and van de Walle (2018), which comprises 23 proxy variables covering dwelling characteristics, demographics, education, employment and religion, we select 15 proxy variables, five dwelling characteristics, seven demographic, two educational and occupational characteristics, as well as the household's religion. Our choice of this relatively small number of indicators is driven by the limited degrees of freedom, especially in our urban subsample, and the specific context of our study area. Descriptive statistics for these variables and the regression output of the calibration exercise are set out in tables A1 and A2 of the online appendix. While most variables have the expected sign, only a few variables are individually significant predictors of consumption, the type of roof, a high dependency ratio as well as age and education of the household head.

In a second step, for all households in the evaluation sample, we calculate the PMT's index value, essentially predicted logarithmic consumption. In a third step, all households are sorted by this index value. Finally, we classify the *n* households with the smallest index values as PMT-eligible, where we set *n* equal to 146 and 64 for the rural and the urban sector, respectively, the number of (CBT-)beneficiary households in the evaluation sample. As discussed in section 3, these numbers come very close to the population targeting rate of 30 percent. We choose the actual numbers of CBT-targeted households in our sample rather than 0.3 times the

[7] For example, in Alatas, Banerjee, and Hanna (2012), district-wise PMT weights are calculated for 49 indicators from the 2007 wave of the SUSENAS national household survey (collected in February and July 2007) and these are used for calculating PMT scores for all households in communities assigned to the PMT treatment of their RCT, where the poverty census was conducted during the last two months of 2008.

number of evaluation-sample households (which would be 144 and 59, respectively) to eliminate any mechanical differences in targeting errors between the two methods, CBT and PMT.

4.2 Identifying the Poor

The main objective of our empirical analysis is to compare the extents to which households that are actually poor are targeted by CBT and by PMT. To identify the poor in each of the years 2007, 2008 and 2009, we consider both an absolute and a relative poverty concept. We calculate absolute poverty lines by calibrating headcount ratios in our sample to the official ones reported for Burkina Faso's 2009 living standards survey.[8] The headcount ratios for the country as a whole, at its national poverty line, were 52.6% in rural and 27.9% in urban areas in that year. Moreover, for the same data, the country's national statistical office reports an aggregate headcount ratio of 47.0 percent for the department of Nouna, which closely matches the national figure of 46.7 percent (INSD 2015).[9] Given, in addition, that Nouna's rural-urban population distribution closely corresponds to the national one (see section 2.1), we make the assumption that Nouna is broadly representative of the country as a whole regarding rural and urban poverty and calibrate rural and urban absolute poverty lines for our 2009 household survey data such that the rural and urban head count ratios in the sample match the national ones. Since neither national nor regional poverty figures are available for 2007 and 2008, we calculate the absolute poverty lines for these two years by deflating our 2009 poverty lines using Burkina Faso's official consumer price index.

According to panel 1 of table 1, in the calibration sample the share of households whose consumption falls short of the poverty line, *poverty rate* for short, stands at 62.4, 50.3 and 53.6 percent for 2007, 2008 and 2009, while the corresponding figures are 33.0, 50.5 and 30.6 for the urban sector. Notice that the poverty rate is for households while the headcount ratios used to calibrate absolute poverty lines are for individuals. Panel 2 of the same table demonstrates that all poverty rates are very similar in the evaluation sample.

For the relative poverty concept, we take the stated objective of the targeting intervention discussed earlier on, to identify the poorest 30 percent of households, and flag the 30 percent consumption-poorest households in the calibration sample in each year, separately for rural and urban sectors. The tiny deviations of the relative poverty rates from 0.30 in the calibration sample (panel 1 of table 1) are entirely due to the finite number of sample households.

4.3 Targeting Performance

We focus on the most common measure of targeting performance, the mean targeting error, which is the proportion of households erroneously classified by the targeting method under consideration relative to their poverty status (Alatas et al. 2012). To fix ideas, we first calculate the targeting error for each targeting method, poverty concept, household and year as

[8] Official poverty figures for Burkina Faso are available only for the years 2003, 2009 and 2013, when national consumption surveys were fielded.

[9] To the best of our knowledge, official sector-wise poverty figures are not available for the department of Nouna.

$$TE_{h,t}^{TM,PC} = 1\{h \text{ is poor by poverty concept } PC \text{ in year } t \text{ and not targeted by method } TM\} + 1\{h \text{ is targeted by method } TM \text{ and not poor by poverty concept } PC \text{ in year } t\}, \quad (1)$$

where *TE* denotes targeting error, *h* is a household identifier, *t* denotes the year (2007, 2008 or 2009), *TM* the targeting method (CBT or PMT), and *PC* the poverty concept (absolute or relative); $1\{\,\}$ denotes the indicator function, which takes a value of one (zero) if the expression in parentheses is true (false). The corresponding mean targeting error (MTE) is the sample mean of *TE* with respect to *h* for given values of *t*, *TM* and *PC*. Notice that, given the targeting method, the targeting status of a household is the same in all three years while its poverty status may change from year to year.

Equation (1) illustrates that the mean targeting error is the sum of two types of errors. First, an exclusion error (false negative) occurs when a consumption-poor household is not targeted by the targeting method under consideration. Conversely, non-poor households which are targeted by the method under consideration contribute to an inclusion error (false positive).

Since the mean targeting error depends on the targeting rate as well as the poverty rate, we will also report the mean targeting error when households are targeted at random for reference. We calculate the random targeting error as

$$RTE_{h,t}^{TM,PC} = Sample\_Poverty\_Rate_t^{PC}\left(1 - Sample\_Targeting\_Rate_t^{TM}\right) + \left(1 - Sample\_Poverty\_Rate_t^{PC}\right) Sample\_Targeting\_Rate_t^{TM},$$

where $Sample\_Poverty\_Rate_t^{PC}$ is the relative frequency of consumption-poor households according to poverty concept *PC* in year *t* and $Sample\_Targeting\_Rate_t^{TM}$ denotes the relative frequency of households targeted by method *TM* in the year *t* sample. Due to some attrition in the household survey, there are minor fluctuations in the targeting rates, both by year and targeting method, and therefore *RTE* also fluctuates accordingly between CBT and PMT in 2008 and 2009. Between the two targeting methods, however, this difference is smaller than half a percent (or 0.2 percentage points) on average and hence negligible.

To conduct statistical inference regarding the difference in mean targeting errors across the two targeting methods in a given year, we estimate the following regression equation with each household in the year *t* sample appearing twice, once with its CBT targeting error (*TM=CBT*) and once with its PMT targeting error (*TM=PMT*),

$$TE_{h,t}^{TM,PC} = \gamma + \beta\, 1\{TM = PMT\} + u_{h,t}^{TM,PC}, \quad (2)$$

where u is a stochastic error term, which we cluster at the household level. For a given poverty concept and year, the OLS estimate of the coefficient β gives the difference between PMT and CBT mean targeting errors.

To conduct statistical inference regarding the dynamics of targeting performance we will consider double differences in mean targeting errors, across the two targeting methods and years. Toward this, we estimate the following regression equation with each household appearing four times, with its CBT targeting error (*TM=CBT*) in 2008 (or 2009) and 2007, and with its PMT targeting error (*TM=PMT*) in the same two years. For a comparison of targeting performance in 2008 relative to the baseline in 2007, the estimating equation is

$$TE_{h,t}^{TM,PC} = \gamma_t + \delta\, 1\{TM = PMT\} + \beta\, 1\{t = 2008\}\, 1\{TM = PMT\} + u_{h,t}^{TM,PC}, \quad (3)$$

where we cluster error terms at the household level. For a given poverty concept, the OLS estimate of β gives the change in the difference between PMT and CBT mean targeting errors from 2007 to 2008.

5 Results

5.1 Poverty Transitions

A necessary condition for the main hypothesis under investigation in this paper – that the dynamic targeting performance of CBT and PMT differ fundamentally – is that poverty is a dynamic phenomenon, more precisely that households transition into and out of poverty from year to year with a positive probability. To see this, suppose that the poverty status of households were unchanged over the three years covered by our consumption survey. Then the mean targeting errors of both methods would be the same in the baseline and the two following years. On the other hand, if the sets of poor households differ between 2007 and 2008 and CBT and PMT were to come with the same mean targeting errors in 2007, the overlap of consumption-poor households in 2008 with the set of CBT-eligible (PMT-eligible) households may well be larger (smaller) than in 2007, giving the CBT an advantage regarding its relative targeting performance in 2008.

Therefore, in this subsection, we assess the poverty dynamics in our sample. Poverty transition matrices for the calibration sample, that is all households in our consumption data set, are set out in table 2. Panel 1 contains figures for the absolute and panel 2 for the relative poverty concept. For both sectors as well as the pooled sample, we consider two transitions, from 2007 to 2008 and from 2007 to 2009. Hence the table includes twelve transition matrices in total. Since absolute poverty rates differ over the years, we first turn to panel 2, where the underlying poverty rate equals 0.3 in each of the three years. According to columns 3 to 6, the probabilities for poverty transitions, that is out of poverty and into poverty, from 2007 to 2008 and from 2007 to 2009 are large and statistically significant. According to columns 1 and 2 of panel 2.A, households which are poor in 2007 are more likely to be non-poor (53.4 percent) than poor (46.6 percent) in 2008 as well as in 2009 (57.6 versus 42.4 percent according to panel 2.B). As for all transition matrices set out in this table, the off-diagonal elements of this matrix are significantly different from zero at the 1 percent level. Consistent with these figures, the percentage of chronically poor households according to the relative poverty concept, those that are poor in all three years, equals merely 11.1 and 4.2 percent in the rural and urban sector, respectively, and not more than 22 percent of households are poor in at least two years.

The order of magnitude of the transition probabilities reported in table 2 for the relative poverty concept is well in line with the figures in Baulch and Hoddinott (2000), who report probabilities of 51 to 60 percent for households in the poorest consumption quintile of various low- and middle-income countries to exit this quintile within one period. In sum, we record that poverty as measured by per-capita consumption is a highly dynamic phenomenon in Nouna – as in other previously studied contexts.

5.2 Targeting Performance

Table 3 contains the mean targeting errors for the pooled sample as well as separately for rural and urban households for the three years 2007, 2008 and 2009. The underlying poverty concept is the absolute one. For each MTE, the table also contains the respective random targeting error as well as the sample targeting and poverty rates. Columns 3, 6 and 9 contain the differences in targeting errors between CBT and PMT, which are obtained from the OLS estimate of β in equation (2). At the bottom of panels 2 and 3, in the same columns, we report double differences in MTEs for 2008 and 2007, and 2009 and 2007, respectively, which are obtained from the OLS estimate of β in equation (3). Table 4 is structured analogous to table 3 for the relative poverty concept.

According to the first panel of tables 3 and 4, at baseline in 2007, PMT comes with eight to thirteen percent smaller mean targeting errors than CBT, and PMT outperforms CBT in rural as well as in urban areas. For the relative poverty concept, the advantage of PMT over CBT in the pooled sample of 4.7 percentage points is statistically significant, with a p-value of 0.057 (column 3). These MTE differences are of a similar magnitude as the ten percent reported in Alatas et al. (2012) for Indonesia.

According to panel 2 of tables 3 and 4, twelve months later this pattern is radically reversed. While PMT continues to be slightly more accurate than CBT for urban households according to the absolute poverty concept, CBT is considerably more accurate in both sectors according to the relative poverty concept. For the pooled sample, CBT's advantage equals twelve percent, which is borderline significant (p-value of 0.104). The statistical test for an equal difference between the two methods in 2007 and 2008 confirms the superior dynamic targeting properties of CBT: the three double differences between the entries in columns 3, 6 and 9 of panels 2 and 1 in table 4 are all positive, large and statistically significant at the 1 percent level for the rural sector as well as the pooled sample, implying that CBT performs substantially better relative to PMT one year after the targeting exercise than at baseline, at least according to the relative poverty concept. For the absolute poverty concept, we also obtain positive double differences, albeit they do not attain statistical significance. We attribute this pattern to the fact that absolute poverty rates fluctuate considerably over the years and are not well synchronized with the CBT targeting rates.

According to panel 3 of both tables, CBT and PMT perform very similarly 30 months post baseline: while neither of the two targeting methods improves substantially over random targeting in the villages, CBT does considerably better than PMT in the urban neighborhoods, resulting in large and significant double differences of fourteen and twelve percentage points, respectively.

To summarize, PMT outperforms CBT only when consumption data is collected concurrent to the targeting exercise while this advantage is reversed one year later – when program benefits usually reach the targeted households. While our "static" results are fully in line with several previous studies, including our own (Hillebrecht et al. 2020), the findings presented here qualify the extant static targeting performance analyses in an important way. Several authors have shown that local communities implement a different concept of poverty than proxy-means tests (Alderman 2002; Van Campenhout 2007; Kebede 2009; Alatas et al. 2012). Our results imply that their concept is indeed forward-looking and that they predict consumption rankings one year ahead more accurately than the most common data-based targeting method, even though the community members in our CBT exercise were not prompted to base their assessments on contemporaneous or future per-capita consumption.

5.3 What Makes Community-Based Targeting Forward-Looking?

In this section, we explore three distinct channels that are candidates for explaining the main finding of our dynamic targeting performance analysis, that PMT's advantage over CBT at baseline vanishes quickly as time passes. The first one is static and maintains that communities can identify severe poverty at baseline better than a statistical procedure.[10] If the depth of poverty correlates negatively with the probability of exiting poverty one or two years later, an advantage in identifying severely poor households will result in better targeting accuracy in future time periods.

To assess this possibility, we calculate mean targeting errors along the consumption distribution at baseline, in 2007. For each poverty concept, we define four consumption classes such that the shares of extremely and moderately poor households are roughly equal and sum up to the 2007 sample poverty rates, which are included in tables 3 and 4. The other two classes, which we call "around median" and "affluent", are defined similarly and their shares sum up to one minus the sample poverty rate. By this construction, the targeting error is equal to the exclusion error for extremely and moderately poor households, while for around-median and affluent households the targeting error equals the inclusion error.

According to the results, which are set out in table 5, PMT targets extremely as well as moderately poor households more accurately than CBT. PMT's advantage as captured by the exclusion error for both poverty concepts is large, albeit, due to small samples, not in a statistically significant fashion. For the relative poverty concept, this advantage of PMT at baseline extends to the entire consumption distribution: in columns 3, 6 and 9 of panel 2 all twelve differences are negative. From these figures we conclude that CBT has no relative advantage in identifying households that are extremely (or ultra-) poor at the time of the targeting exercise.

The second possibility we explore is that CBT may identify chronically poor households more accurately. Support for such a pattern comes, e.g., from Scoones (1995), who come to the conclusion that communities pay more attention to household characteristics which predict chronic poverty. To assess this possibility, we focus on households which are part of our consumption panel survey in each of the years 2007, 2008 and 2009, and create three binary variables for alternative definitions of chronic poverty, poor in at least one, at least two, or all three years. For each of these definitions, we calculate mean targeting errors, which are set

[10] For example, Alatas et al. (2012) find some, albeit statistically insignificant evidence for this claim.

out in table 6. According to the first row of both panels, which captures the two targeting methods' ability to identify chronically poor households, PMT maintains its static advantage over CBT. For both poverty concepts and across rural and urban sectors, PMT has slightly smaller targeting errors than CBT. If anything, CBT performs a little better in identifying households that are poor according to the relative poverty concept in at least two of the three years, albeit not in a statistically significant fashion. From these figures we conclude that CBT has no advantage in identifying chronically poor households.

As a third channel, we examine the way observable and unobservable (to the researcher) household characteristics at the time of the targeting exercise are processed by the communities. Our focus is, first, on how communities weigh characteristics evident to the researcher and whether the implied weights reflect the relationship between these characteristics and future rather than current consumption. Second, we assess to what extent community assessments are based on household characteristics which are not easily evident, at least to the researcher.

Turning to the former, in a first step we assess whether community targeting outcomes give implicit weights to household characteristics incorporated in the PMT that are more suited to predict future rather than current consumption. Toward this we regress the community targeting outcomes and MPCE from all three survey waves on the 15 proxy variables of our PMT, which are all recorded in 2007, the baseline year. To make the coefficients of the CBT outcome, which is captured by a dummy variable, and MPCE comparable, we take the negative of the dummy variable capturing eligibility based on CBT and standardize all four dependent variables by dividing by their respective standard deviations. The results are set out by sector in table 7. Columns 1 and 5 display the coefficients for the CBT outcome, while columns 2 to 4 and 6 to 8 are regressions of standardized logarithmic per capita consumption on the same set of variables. Notice that columns 2 and 6 essentially correspond to our PMT calibration regressions, except that the sample used now is the evaluation sample. The F-statistic toward the bottom of this table in the consumption columns is for the joint hypothesis that the coefficients in the respective consumption regression are equal to those in the corresponding CBT regression (column 1 for columns 2, 3 and 4; column 5 for columns 6, 7 and 8). Since these cross-equation restrictions require simultaneous estimation of two equations at a time, we restrict the set of observations for this exercise to households in the evaluation sample for which consumption is recorded in all three years.

For the rural and urban CBT regressions in columns 1 and 5, most of the coefficients have the expected signs, positive for dwelling characteristics and household head's education, negative for the share of elderly household members and female headship. For less than a handful of these covariates, individually significant coefficients obtain, which is owed to the relatively small size of our survey data set and non-negligible correlations among some of the covariates. The perhaps most unexpected estimate obtains for household size, which is positive and significant for rural households, implying that communities deem a large household less poor – conditional on other observable characteristics. Columns 2 and 6 display the coefficients of regressions of rural and urban logarithmic, standardized MPCE recorded around the time of the CBT exercise on the same covariates. As expected, household size has a significant negative coefficient, while most of the other coefficients are in line with those in the CBT regressions, albeit they differ in magnitude. From the R-squared statistics in columns 1, 2, 5 and 6, it is evident that CBT outcomes and consumption poverty are predicted by the PMT variables to very similar extents. Nonetheless, according to the F-statistics in columns 2 and 6, the hypothesis that CBT implicitly factors in baseline covariates in the same way the PMT does to optimally predict baseline consumption is rejected at the one percent level for both sectors.

While columns 3, 4, 7 and 8 feature different linear combinations of baselines covariates to optimally predict consumption in the two years following the CBT, an F-test for identical linear combinations to predict rural (urban) consumption in 2008 and 2007 (7 and 6), and 2009 and 2007 (8 and 6) fails to reject at the ten percent level for both years (p-values of 0.375 and 0.101 for the rural, and 0.556 and 0.508 for the urban sample; not displayed in the table). In other words, the relationship between baseline covariates and consumption in different years is similar. Despite this, the F-statistic for equal linear combinations predicting the 2007 CBT outcome and MPCE drops sharply for 2008 and 2009 consumption (columns 3 and 4, and 7 and 8) relative to 2007 consumption (columns 2 and 6) and the corresponding p-values imply that the hypothesis of equal coefficients fails to be rejected at the five percent level in three of four instances (columns 4, 7 and 8). In sum, part of the favorable dynamic targeting performance of CBT relative to PMT is due to how household characteristics at baseline which are included in a PMT are implicitly weighted by the communities.

In a second step, we explore to what extent community assessments take into account additional household baseline characteristics which are not included in typical PMTs, in particular possession of moveable assets and health. Both of them are usually omitted from PMTs because they are difficult to verify (Alatas et al. 2012; Brown, Ravallion, and van de Walle 2018). For example, moveable assets can be hidden by the household on the occasion of an enumerator's visit – unlike the building condition. We implement this analysis by augmenting the regressions reported in table 7 by ten indicator variables capturing the possession of various moveable assets and a count variable capturing occurrences of illness, both chronic and acute, during the four weeks preceding the survey interview.

The results are set out in table 8, which is structured like table 7 except for the test statistics reported toward the end. The first of the two F-statistics is for the hypothesis that the eleven additional regressors are jointly significant, while the second one is for the hypothesis that the coefficients of the same set of variables in the respective column is equal to the ones reported for the corresponding CBT (set out in columns 1 and 5, respectively). For considerations of space, the coefficient estimates for the fifteen PMT covariates reported in the previous table are not set out in table 8. They are similar in sign and magnitude to the ones reported in table 7.

According to columns 1 and 5, community assessments correlate significantly with the possession of movable assets, in particular cart and plow in the villages and television sets in the urban neighborhoods. Both respective F-statistics are significant at the five percent level. Regarding the linear combinations by which these covariates best predict MPCE, a pattern similar to the previous table emerges: the F-test of the null that the eleven slope coefficients for MPCE (columns 2, 3 and 4, and 6, 7 and 8) are the same as the ones in the corresponding CBT (columns 1 and 5) clearly rejects for baseline consumption (last row of columns 2 and 6). For consumption in 2008 and 2009, on the other hand, the same test fails to reject in three of four instances. In sum, we conclude that a second contributor to the favorable dynamic targeting performance of CBT relative to PMT is that the communities implicitly weigh additional information on households available at the time of the targeting exercise such that future rather than current consumption is predicted.

We finally explore to what extent CBT includes information relevant for a household's contemporaneous and future consumption-poverty status beyond easily observable household characteristics at baseline. Toward this we regress a household's poverty status in different years on all 26 baseline covariates included in table 8 as well as the household's eligibility status according to the CBT. The results are set out in table 9. For considerations of space, the coefficient estimates for the 15 PMT and the 11 additional baseline covariates are not included in this table. Results for the absolute poverty concept are set out in panel 1 and for the relative concept in panel 2. Our focus here is primarily on the latter since the dependent variable, a dummy for belonging to the 30 percent consumption-poorest households, has the same variation in each year and corresponds more closely to the communities' task, to identify the 30 percent poorest households. For the absolute poverty concept, in contrast, the poverty rate fluctuates considerably, exceeding 60 percent in one instance (rural sector in 2007; see table 1).

For both the rural and the urban sector and both poverty concepts (columns 1 and 4), a household's consumption-poverty status at the time of the community targeting exercise is conditionally positively correlated with the community's assessment, indicating that communities process some information relevant for a household's contemporary poverty status which is not observed by the researcher. On the other hand, all four respective coefficients fail to be statistically significant by far, implying that the extra information implied in the communities' assessments is of only a minor extent. This pattern is radically reversed for households' poverty status in the two subsequent years. For the relative poverty concept, for three of the four instances, the CBT variable has a large and significant positive coefficient. According to the point estimate in column 6, for example, an urban household deemed eligible by its community in 2007 is 20.6 percentage points more likely to be consumption-poor in 2009 – conditional on 26 baseline covariates. For the urban sector, where relative and absolute poverty rates are more similar, the same picture arises also for the absolute poverty concept.

How much additional information does the communities' assessment contain for future poverty relative to (relatively easily observable) baseline characteristics? To come up with quantitative figures to answer this question, we conduct two analyses. First, we calculate the contributions to the R-squared statistic included in table 9 for three groups of regressors, PMT variables (15), additional baseline covariates (11) as well as the CBT outcome (a dummy variable). We use a technique building on the concept of the Shapley value recently suggested by Shorrocks (2013) and applied to general regression settings by Huettner and Sunder (2012). The results of this analysis are plotted for the relative poverty concept in figure 1.

In addition, to obtain insights beyond the restrictive functional forms of linear regression, we formulate the problem of classifying a household as poor or non-poor in alternative years based on a set of covariates as machine learning problems employing random forests (Breiman 2001). For each problem, we calculate variable importance metrics, which are obtained by adding up the improvement in the objective function given in the splitting criterion over all internal nodes of a tree and across all trees in the forest, separately for each predictor variable (Schonlau and Zou 2020). For the relative poverty concept, figure 2 depicts the relative variable importance of the twelve most important predictors of poverty among the set of the 27 predictors included in the regressions set out in table 9. The figures are normalized such that the most important predictor is given an importance score of one while the other predictors' importance is scaled in relation to that one.

The results from both approaches are fully consistent and match the pattern obtaining in panel 2 of table 9: community judgements convey almost no additional information about consumption poverty at baseline, while the opposite holds one year later in both sectors and also for urban poverty two years later. According to the Shapley decompositions depicted in figure 1, PMT variables and the additional covariates are similarly important for predicting baseline as well as 2008 poverty, while the PMT variables clearly take the lead for poverty thirty months later. Averaged across both sectors and the two years subsequent to the baseline (2008 and 2009), the contribution of CBT outcomes stands at around 15 percent. In comparison to other individual predictors, CBT eligibility status is the most important predictor of 2009 urban poverty and the second-most important one for 2008 poverty in both sectors (not reported in the charts).

While qualitatively in line with the Shapley decompositions, the corresponding variable importance analyses with the machine-learning algorithm yield less stark contributions of the CBT status (figure 2): while, with relative importance scores of 70 percent and more, the CBT eligibility variable is most important for poverty predictions in both sectors in 2008 and for 2009 urban poverty, it is always dominated by at least three other predictors, of which two are consistently demographic variables.

We conclude that an important contributor to the favorable dynamic targeting performance of CBT relative to PMT is that community assessments contain information which is particularly important for a household's future poverty status but included neither in conventional PMTs nor in usual living standard surveys. Our variable importance analyses show that the extent of this additional information is sizable, albeit perhaps not critical in relation to other baseline characteristics, especially when we go beyond restrictive linear functional forms.

It is beyond the scope of our research design to identify econometrically the individual factors which are unobserved by the researcher while taken into account by the communities. In our view, plausible candidates are past consumption volatility experienced by a household, lagged values of baseline covariates, more precise information on health, which our survey captures only roughly, or a household's social and economic network connections, including connectedness to local elites. In addition, qualitative evidence from focus group discussions preceding the community wealth rankings highlights the importance communities give to various capabilities of a household. According to Savadogo et al. (2015), of the seven criteria most often mentioned in these discussions, four are capabilities which fail to be captured – at least directly – by the baseline covariates included in our analysis: "is not in good health preventing to work", "is unable to solve his own problems by himself", "is unable to fill his medical prescriptions", "has no social network". Albeit we are not aware of quantitative research on this topic, we strongly suspect that these capabilities are important predictors of a household's poverty trajectory, in particular in the medium term, the focus of our analyses.

6 Discussion

In this paper we have conducted the first dynamic analysis of poverty targeting. Comparing the targeting accuracy of PMT and CBT relative to consumption poverty during the year where the targeting exercise takes place as well as 12 and 30 months later, we find that the advantage of PMT documented by many previous studies and replicated here is reversed one year later, the time welfare program benefits often start reaching beneficiary households in practice. We have been first to proof that communities exhibit a forward-looking concept of poverty, a property that has been conjectured by several previous writers (e.g. Scoones (1995); Stoeffler, Mills, and del Ninno 2016) but thus far not been established quantitatively. We have also elicited the sources of this forward-orientation and shown that communities process household characteristics observable at baseline differently than a PMT and, in addition, consider information which is not usually observed by a researcher or bureaucrat.

We view our analysis as an important, albeit imperfect first step to learn about the dynamics of targeting accuracy. The fundamental challenge in this connection is that either the targeting exercise is followed by welfare benefits, which in turn will affect the poverty trajectories of households, or the targeting exercise is purely hypothetical. While it is straightforward to assess the dynamic properties of hypothetical PMTs, by combining a fictitious PMT (as, for example, in Brown et al. (2018)) with a multi-year panel survey on consumption, such an approach is not feasible for a CBT. A targeting exercise with benefits promised but not accruing would deceive the communities involved, while a CBT with no stakes at the outset would be cheap talk. Our research design has resolved this challenge by combining a hypothetical PMT with a relatively small and indirect economic benefit allocated through a CBT for which we have provided evidence that it is unlikely to affect households' poverty trajectories. While we have requested the communities to nominate the poorest 30 percent of households, we are aware that the targeting outcome might look different if stakes were higher, for example if a generous income transfer scheme were promised (as e.g. in Alatas et al. 2012).

Our research carries important practical implications. First, CBTs, perhaps preceded by a geographical targeting step, might have a greater poverty impact than PMTs than previously thought. In our context, PMT has a satisfactory targeting performance only at baseline, while CBT performs well one year later in villages and during two years following the baseline in urban neighborhoods. Given the cost advantages of CBT, these findings clearly speak in favor of CBT, at least for welfare programs that reach with a lag and/or pay revolving benefits. Second, deviating from the current best practice for calibrating PMTs, where proxy variables and consumption data are recorded contemporaneously, researchers as well as practitioners should assess the dynamic targeting performance of PMTs calibrated with lagged values of proxy variables. In consequence, proxy-means tests should be explored that are tailored to the timing of the accrual of program benefits. While our explorative analysis of this topic has not shown statistically different PMT calibrating weights for different lags of baseline proxy variables, our data is not ideally suited for such an endeavor because of the relatively small number of observations available for the PMT calibration. Finally, our analysis can serve as a point of departure for investigating how frequently a targeting exercise should be repeated to ensure an enduring poverty impact. In our context, it appears that PMTs would have to be repeated virtually every year if consumption-poor households are to be reached. For CBT, instead, a rhythm of two in rural and three years in urban areas seems sufficient.

We view these suggestions as contributions to a very recent literature on the optimal design of targeting exercises. For Indonesia, Alatas et al. (2016a) advocate considering community networks more carefully in the context of CBT, while, for PMTs, Alatas et al. (2016b) demonstrate the advantage of self-targeting and Bah et al. (2019) draw attention to the importance of updating targeting household registries more often. Complementary to these, we advocate reconsidering CBTs more seriously and we highlight the importance of the timeline of targeting and program benefits as well as the need to repeat targeting exercises periodically.

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Figures and Tables

Figure 1. Contributions to predicting baseline and future poverty: Shapley decompositions

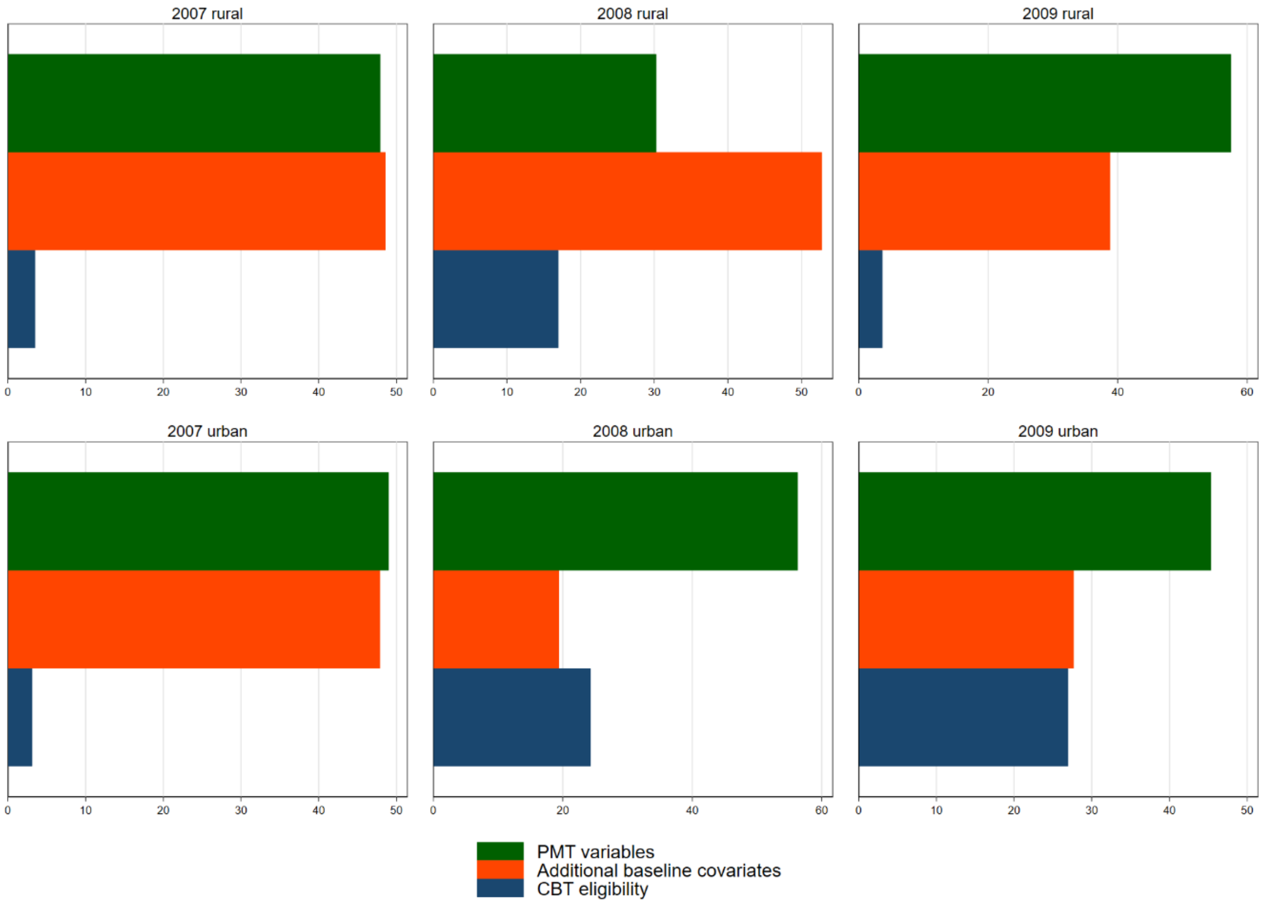

Figure 2. Contributions to predicting baseline and future poverty: random forests

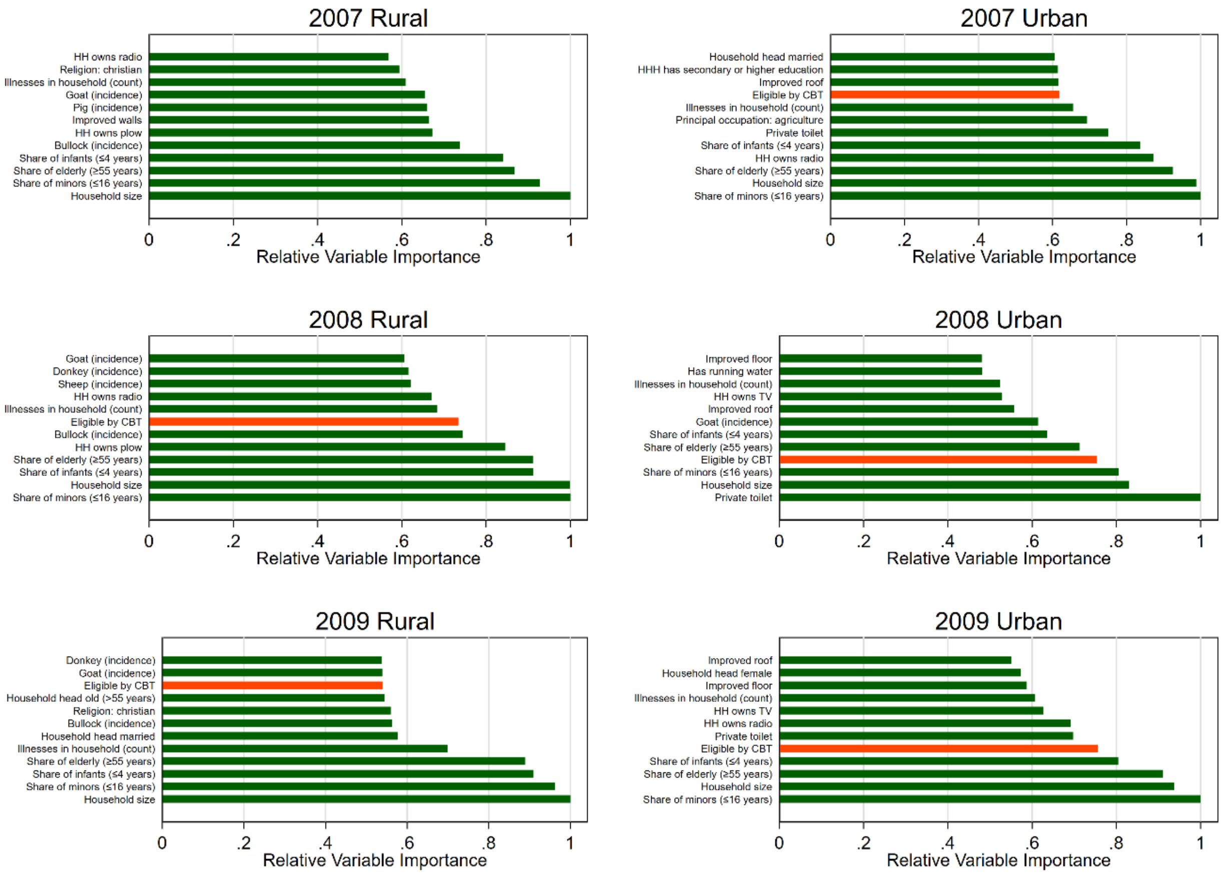

Table 1. Descriptive statistics: consumption, poverty and targeting

	2007		2008		2009	
	Rural	Urban	Rural	Urban	Rural	Urban
	(1)	(2)	(3)	(4)	(5)	(6)
Panel I. Calibration sample (all households in survey)						
MPCE	2975	6587	4753	7985	3544	7934
	(3743)	(9672)	(7925)	(16296)	(4208)	(12147)
Poverty rate (absolute)	0.624	0.330	0.503	0.505	0.536	0.306
	(0.485)	(0.471)	(0.500)	(0.501)	(0.499)	(0.462)
Poverty rate (relative)	0.301	0.298	0.298	0.299	0.298	0.299
	(0.459)	(0.458)	(0.458)	(0.459)	(0.458)	(0.459)
Households	518	315	493	291	483	281
Communities	39	23	39	23	39	23
Panel II. Evaluation sample (intersection of survey and CBT households)						
MPCE	3024	5520	4862	7409	3590	6954
	(3835)	(5707)	(8110)	(12334)	(4312)	(6983)
Poverty rate (absolute)	0.614	0.342	0.491	0.500	0.533	0.281
	(0.487)	(0.476)	(0.500)	(0.501)	(0.499)	(0.451)
Poverty rate (relative)	0.296	0.316	0.290	0.328	0.296	0.281
	(0.457)	(0.466)	(0.454)	(0.471)	(0.457)	(0.451)
Targeted by CBT	0.305	0.327	0.303	0.328	0.300	0.330
	(0.461)	(0.470)	(0.460)	(0.471)	(0.459)	(0.471)
Households	479	196	462	192	456	185
Communities	39	23	39	23	39	23

Notes: means, standard deviations in parentheses.

Table 2. Poverty transitions

	Pooled		Rural		Urban	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel I. Absolute poverty concept					
	A. Poverty transitions 2007-2008					
	Poor 08	Not Poor 08	Poor 08	Not Poor 08	Poor 08	Not Poor 08
Poor 07	0.637	0.363	0.638	0.362	0.634	0.366
	(0.024)	(0.024)	(0.027)	(0.027)	(0.048)	(0.048)
Not Poor 07	0.359	0.641	0.280	0.720	0.437	0.563
	(0.025)	(0.025)	(0.033)	(0.033)	(0.036)	(0.036)
	B. Poverty transitions 2007-2009					
	Poor 09	Not Poor 09	Poor 09	Not Poor 09	Poor 09	Not Poor 09
Poor 07	0.592	0.408	0.642	0.358	0.438	0.563
	(0.025)	(0.025)	(0.028)	(0.028)	(0.051)	(0.051)
Not Poor 07	0.301	0.699	0.364	0.636	0.238	0.762
	(0.024)	(0.024)	(0.036)	(0.036)	(0.031)	(0.031)
	Panel II. Relative poverty concept					
	A. Poverty transitions 2007-2008					
	Poor 08	Not Poor 08	Poor 08	Not Poor 08	Poor 08	Not Poor 08
Poor 07	0.466	0.534	0.510	0.490	0.396	0.604
	(0.033)	(0.033)	(0.042)	(0.042)	(0.052)	(0.052)
Not Poor 07	0.226	0.774	0.210	0.790	0.255	0.745
	(0.018)	(0.018)	(0.022)	(0.022)	(0.031)	(0.031)
	B. Poverty transitions 2007-2009					
	Poor 09	Not Poor 09	Poor 09	Not Poor 09	Poor 09	Not Poor 09
Poor 07	0.424	0.576	0.406	0.594	0.453	0.547
	(0.033)	(0.033)	(0.042)	(0.042)	(0.054)	(0.054)
Not Poor 07	0.246	0.754	0.255	0.745	0.231	0.769
	(0.019)	(0.019)	(0.024)	(0.024)	(0.030)	(0.030)

Notes: The table reports transition probabilities. Standard errors are in parentheses.

Table 3. Targeting performance, absolute poverty concept

	Pooled			Rural			Urban		
	CBT (1)	PMT (2)	Difference (3)	CBT (4)	PMT (5)	Difference (6)	CBT (7)	PMT (8)	Difference (9)
	Panel I. Poverty status in 2007 (Baseline)								
Mean targeting error	0.458	0.422	-0.036	0.484	0.463	-0.021	0.393	0.321	-0.071
	(0.019)	(0.019)	(0.025)	(0.023)	(0.023)	(0.030)	(0.035)	(0.033)	(0.047)
Random targeting error	0.513	0.513		0.544	0.544		0.445	0.445	
Targeting Share	0.311	0.311		0.305	0.305		0.327	0.327	
Poverty rate	0.535			0.614			0.342		
Households	675			479			196		
	Panel II. Poverty status in 2008 (12 months later)								
Mean targeting error	0.413	0.417	0.005	0.422	0.435	0.013	0.391	0.375	-0.016
	(0.019)	(0.019)	(0.025)	(0.023)	(0.023)	(0.030)	(0.035)	(0.035)	(0.048)
Random targeting error	0.498	0.498		0.497	0.497		0.500	0.500	
Targeting Share	0.310	0.315		0.303	0.307		0.328	0.333	
Poverty rate	0.491			0.494			0.500		
Households	654			462			192		
Double Difference 2008-2007			0.04			0.03			0.06
			(0.03)			(0.03)			(0.07)
	Panel III. Poverty status in 2009 (30 months later)								
Mean targeting error	0.448	0.448	0.000	0.500	0.474	-0.026	0.319	0.384	0.065
	(0.020)	(0.020)	(0.026)	(0.023)	(0.023)	(0.030)	(0.034)	(0.036)	(0.049)
Random targeting error	0.485	0.485		0.513	0.513		0.425	0.425	
Targeting Share	0.309	0.312		0.300	0.305		0.330	0.330	
Poverty rate	0.460			0.531			0.281		
Households	641			456			185		
Double Difference 2009-2007			0.04			-0.01			0.14**
			(0.03)			(0.03)			(0.06)

Notes: Table reports mean targeting errors, standard errors are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
Columns 3, 6 and 9 report the difference between the PMT and the CBT targeting errors in the preceding two columns.

Table 4. Targeting performance, relative poverty concept

	Pooled			Rural			Urban		
	CBT (1)	PMT (2)	Difference (3)	CBT (4)	PMT (5)	Difference (6)	CBT (7)	PMT (8)	Difference (9)
Panel I. Poverty status in 2007 (Baseline)									
Mean targeting error	0.370	0.323	-0.047*	0.363	0.317	-0.046	0.388	0.337	-0.051
	(0.019)	(0.018)	(0.025)	(0.022)	(0.021)	(0.029)	(0.035)	(0.034)	(0.047)
Random targeting error	0.425	0.425		0.421	0.421		0.436	0.436	
Targeting Share	0.311	0.311		0.305	0.305		0.327	0.327	
Poverty rate	0.302			0.296			0.316		
Households	675			479			196		
Panel II. Poverty status in 2008 (12 months later)									
Mean targeting error	0.324	0.365	0.041	0.333	0.381	0.048	0.302	0.328	0.026
	(0.018)	(0.019)	(0.025)	(0.022)	(0.023)	(0.030)	(0.033)	(0.034)	(0.048)
Random targeting error	0.425	0.426		0.417	0.419		0.441	0.443	
Targeting Share	0.310	0.315		0.303	0.307		0.328	0.333	
Poverty rate	0.301			0.290			0.328		
Households	654			462			192		
Double Difference 2008-2007			0.09***			0.09***			0.08
			(0.03)			(0.03)			(0.06)
Panel III. Poverty status in 2009 (30 months later)									
Mean targeting error	0.388	0.382	-0.006	0.417	0.382	-0.035	0.319	0.384	0.065
	(0.019)	(0.019)	(0.026)	(0.023)	(0.023)	(0.030)	(0.034)	(0.036)	(0.049)
Random targeting error	0.420	0.422		0.419	0.420		0.425	0.425	
Targeting Share	0.309	0.312		0.300	0.305		0.330	0.330	
Poverty rate	0.292			0.296			0.281		
Households	641			456			185		
Double Difference 2009-2007			0.04			0.01			0.12**
			(0.03)			(0.04)			(0.06)

Notes: See table 3.

Table 5. Targeting performance by consumption expenditure class

	Pooled			Rural			Urban		
	CBT (1)	PMT (2)	Difference (3)	CBT (4)	PMT (5)	Difference (6)	CBT (7)	PMT (8)	Difference (9)
				Panel I. Absolute poverty concept					
Extremely poor	0.589	0.506	-0.083	0.605	0.524	-0.082	0.515	0.424	-0.091
	(0.037)	(0.037)	(0.055)	(0.040)	(0.041)	(0.060)	(0.088)	(0.087)	(0.134)
Households	180	180	360	147	147	294	33	33	66
Moderately Poor	0.685	0.702	0.017	0.687	0.735	0.048	0.676	0.559	-0.118
	(0.035)	(0.034)	(0.047)	(0.038)	(0.037)	(0.051)	(0.081)	(0.086)	(0.119)
Households	181	181	362	147	147	294	34	34	68
Around median	0.269	0.237	-0.032	0.217	0.185	-0.033	0.344	0.313	-0.031
	(0.036)	(0.034)	(0.052)	(0.043)	(0.041)	(0.067)	(0.060)	(0.058)	(0.084)
Households	156	156	312	92	92	184	64	64	128
Affluent	0.234	0.190	-0.044	0.237	0.215	-0.022	0.231	0.154	-0.077
	(0.034)	(0.031)	(0.044)	(0.044)	(0.043)	(0.057)	(0.053)	(0.045)	(0.071)
Households	158	158	316	93	93	186	65	65	130
				Panel II. Relative poverty concept					
Extremely poor	0.490	0.431	-0.059	0.479	0.423	-0.056	0.516	0.452	-0.065
	(0.050)	(0.049)	(0.075)	(0.060)	(0.059)	(0.090)	(0.091)	(0.091)	(0.140)
Households	102	102	204	71	71	142	31	31	62
Moderately Poor	0.706	0.608	-0.098	0.718	0.620	-0.099	0.677	0.581	-0.097
	(0.045)	(0.049)	(0.069)	(0.054)	(0.058)	(0.083)	(0.085)	(0.090)	(0.127)
Households	102	102	204	71	71	142	31	31	62
Around median	0.315	0.272	-0.043	0.298	0.250	-0.048	0.358	0.328	-0.030
	(0.030)	(0.029)	(0.041)	(0.035)	(0.034)	(0.048)	(0.059)	(0.058)	(0.080)
Households	235	235	470	168	168	336	67	67	134
Affluent	0.229	0.203	-0.025	0.231	0.213	-0.018	0.224	0.179	-0.045
	(0.027)	(0.026)	(0.039)	(0.033)	(0.032)	(0.046)	(0.051)	(0.047)	(0.072)
Households	236	236	472	169	169	338	67	67	134

Notes: Table reports mean targeting errors by consumption class for 2007. See also the notes to table 3.

Table 6. Targeting performance by chronic-poverty status

	Pooled			Rural			Urban		
	CBT (1)	PMT (2)	Difference (3)	CBT (4)	PMT (5)	Difference (6)	CBT (7)	PMT (8)	Difference (9)
				Panel I. Absolute poverty concept					
Poor in all 3 years	0.352	0.347	-0.004	0.360	0.358	-0.002	0.330	0.319	-0.007
	(0.019)	(0.019)	(0.022)	(0.023)	(0.023)	(0.029)	(0.035)	(0.034)	(0.032)
Households	640	640	1526	455	455	964	185	185	562
At least 2 years poor	0.420	0.428	0.007	0.455	0.448	-0.006	0.335	0.378	0.028
	(0.020)	(0.020)	(0.022)	(0.023)	(0.023)	(0.029)	(0.035)	(0.036)	(0.032)
Households	640	640	1526	455	455	964	185	185	562
At least 1 year poor	0.553	0.523	-0.025	0.598	0.574	-0.023	0.443	0.400	-0.028
	(0.020)	(0.020)	(0.022)	(0.023)	(0.023)	(0.029)	(0.037)	(0.036)	(0.032)
Households	640	640	1526	455	455	964	185	185	562
				Panel II. Relative poverty concept					
Poor in all 3 years	0.319	0.311	-0.007	0.316	0.314	-0.002	0.324	0.303	-0.014
	(0.018)	(0.018)	(0.022)	(0.022)	(0.022)	(0.029)	(0.035)	(0.034)	(0.032)
Households	640	640	1526	455	455	964	185	185	562
At least 2 years poor	0.325	0.352	0.022	0.336	0.343	0.006	0.297	0.373	0.050
	(0.019)	(0.019)	(0.022)	(0.022)	(0.022)	(0.029)	(0.034)	(0.036)	(0.032)
Households	640	640	1526	455	455	964	185	185	562
At least 1 year poor	0.445	0.416	-0.025	0.464	0.426	-0.035	0.400	0.389	-0.007
	(0.020)	(0.019)	(0.022)	(0.023)	(0.023)	(0.029)	(0.036)	(0.036)	(0.032)
Households	640	640	1526	455	455	964	185	185	562

Notes: Table reports mean targeting errors by chronic-poverty status. See also the notes to table 3.

Table 7. How CBT and PMT predict consumption: PMT variables

	Rural				Urban			
Dependent variable:	Non-poor by CBT	MPCE 2007	MPCE 2008	MPCE 2009	Non-poor by CBT	MPCE 2007	MPCE 2008	MPCE 2009
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Improved floor	0.237	0.290	0.327*	0.224	0.008	0.052	0.210	0.405**
	(0.187)	(0.189)	(0.195)	(0.196)	(0.185)	(0.181)	(0.176)	(0.179)
Improved walls	0.016	-0.230*	-0.138	0.216	0.134	0.148	0.195	0.150
	(0.130)	(0.131)	(0.135)	(0.136)	(0.206)	(0.202)	(0.197)	(0.199)
Improved roof	0.265*	0.272*	0.314*	0.087	0.070	0.087	0.002	-0.144
	(0.158)	(0.160)	(0.165)	(0.166)	(0.184)	(0.181)	(0.176)	(0.178)
Has running water	0.388	0.288	-0.080	0.502**	-0.092	0.155	-0.049	-0.175
	(0.238)	(0.240)	(0.248)	(0.249)	(0.171)	(0.167)	(0.163)	(0.165)
Private toilet	0.053	0.307**	-0.009	0.044	-0.134	0.440**	0.645***	0.503**
	(0.129)	(0.130)	(0.134)	(0.135)	(0.208)	(0.204)	(0.198)	(0.201)
Household size	0.021***	-0.017***	-0.003	0.001	0.023	-0.030**	0.009	0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.015)	(0.015)	(0.014)	(0.015)
Share of infants (≤4 years)	0.040	-0.240	-0.089	-0.895*	-0.851	-0.649	-0.061	-2.117***
	(0.495)	(0.501)	(0.516)	(0.519)	(0.836)	(0.819)	(0.797)	(0.808)
Share of minors (≤16 years)	0.365	-1.000***	-0.610*	-0.416	-0.256	-0.132	-0.190	-0.766*
	(0.301)	(0.305)	(0.314)	(0.316)	(0.438)	(0.430)	(0.418)	(0.424)
Share of elderly (≥55 years)	-0.743**	-0.959***	-0.637*	-1.008***	-0.487	-0.252	-0.492	-1.000**
	(0.336)	(0.340)	(0.350)	(0.352)	(0.429)	(0.420)	(0.409)	(0.415)
Household head female	-0.205	-0.077	-0.225	-0.003	-0.542***	-0.145	-0.194	-0.271
	(0.153)	(0.155)	(0.160)	(0.161)	(0.197)	(0.193)	(0.188)	(0.191)
Household head married	-0.016	-0.149	-0.023	-0.213**	0.172	-0.153	0.169	-0.199
	(0.102)	(0.103)	(0.107)	(0.107)	(0.155)	(0.152)	(0.148)	(0.150)
HHH has secondary or higher education	0.280	0.429*	0.212	0.134	0.269	0.261	0.145	0.283*
	(0.222)	(0.224)	(0.231)	(0.233)	(0.175)	(0.172)	(0.167)	(0.169)
Principal occupation: agriculture	-0.171	-0.109	0.398**	0.001	-0.126	-0.333*	-0.426**	-0.118
	(0.181)	(0.183)	(0.189)	(0.190)	(0.178)	(0.174)	(0.170)	(0.172)
Household head old (≥55 years)	0.032	0.297***	0.126	0.213*	-0.121	0.082	0.108	0.011
	(0.112)	(0.113)	(0.117)	(0.117)	(0.177)	(0.173)	(0.169)	(0.171)
Religion: christian	0.219**	0.159	-0.035	0.269***	-0.341**	0.134	-0.053	0.186
	(0.098)	(0.100)	(0.103)	(0.103)	(0.168)	(0.165)	(0.161)	(0.163)
Constant	1.180***	9.157***	8.035***	11.538***	1.604***	9.920***	6.910***	8.642***
	(0.258)	(0.262)	(0.270)	(0.271)	(0.351)	(0.344)	(0.335)	(0.339)
Observations	451	451	451	451	184	184	184	184
R-squared	0.155	0.120	0.068	0.081	0.175	0.173	0.229	0.230
F-statistic: all coefficients equal to CBT		3.795	2.382	1.415		2.186	1.319	1.635
p-value		0.000	0.002	0.133		0.007	0.188	0.063

Notes: All dependent variables are standardized. Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

The dependent variable in columns 1 and 5 is the negative of a dummy variable capturing CBT eligibility, divided by its standard deviation.

The F-statistic is for the hypothesis that all 15 slope coefficients are the same as in column 1 (5) for columns 2, 3 and 4 (6, 7 and 8).

Table 8. How CBT and PMT predict consumption: additional baseline covariates

	Rural				Urban			
Dependent variable:	Non-poor by CBT	MPCE 2007	MPCE 2008	MPCE 2009	Non-poor by CBT	MPCE 2007	MPCE 2008	MPCE 2009
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HH owns bullock	-0.001	0.005	0.012	0.036***	0.026	0.013	-0.043	0.050
	(0.011)	(0.010)	(0.011)	(0.011)	(0.040)	(0.037)	(0.038)	(0.040)
HH owns donkey	-0.040	0.036	-0.015	-0.075	-0.004	0.217**	0.018	-0.026
	(0.053)	(0.048)	(0.054)	(0.053)	(0.092)	(0.086)	(0.089)	(0.091)
HH owns goat	0.017	0.029**	0.006	0.010	-0.021	0.014	0.015	0.003
	(0.013)	(0.012)	(0.013)	(0.013)	(0.027)	(0.025)	(0.026)	(0.027)
HH owns pig	-0.040*	0.055***	0.041*	0.029	-0.005	-0.111***	0.017	-0.040
	(0.021)	(0.019)	(0.022)	(0.021)	(0.032)	(0.030)	(0.031)	(0.032)
HH owns sheep	0.022	0.009	0.002	-0.018	0.051*	0.006	0.033	-0.038
	(0.014)	(0.013)	(0.014)	(0.014)	(0.029)	(0.027)	(0.028)	(0.029)
HH owns cart	0.279**	0.339***	0.140	0.038	0.230	0.028	0.015	0.026
	(0.135)	(0.122)	(0.137)	(0.136)	(0.228)	(0.213)	(0.220)	(0.227)
HH owns plow	0.264**	0.290**	0.254**	0.041	-0.207	-0.100	0.327	-0.034
	(0.124)	(0.112)	(0.126)	(0.125)	(0.225)	(0.210)	(0.217)	(0.224)
HH owns radio	0.132	0.142	0.351***	-0.008	0.055	0.232	-0.145	0.103
	(0.109)	(0.099)	(0.111)	(0.110)	(0.208)	(0.194)	(0.201)	(0.207)
HH owns TV	0.072	0.409**	0.006	0.453**	0.627***	0.298	0.411**	0.246
	(0.194)	(0.176)	(0.198)	(0.195)	(0.206)	(0.193)	(0.199)	(0.205)
Illness accumulated at HH-level	-0.001	0.128***	0.061	0.103**	-0.060	0.181**	0.031	0.099
	(0.046)	(0.042)	(0.047)	(0.046)	(0.096)	(0.090)	(0.093)	(0.096)
Constant	0.990***	8.857***	7.708***	11.423***	1.709***	9.544***	7.069***	8.617***
	(0.281)	(0.255)	(0.287)	(0.284)	(0.377)	(0.352)	(0.364)	(0.376)
Observations	413	413	413	413	177	177	177	177
R-squared	0.220	0.304	0.149	0.142	0.286	0.326	0.279	0.265
F-statistic extra variables	3.415	6.704	3.640	3.305	2.080	2.834	1.582	1.402
p-value	0.000	0.000	0.000	0.000	0.029	0.000	0.056	0.119
F: CBT has same coefficients		2.378	1.228	2.611		1.949	0.660	1.317
p-value		0.009	0.269	0.004		0.039	0.761	0.220

Notes: All dependent variables are standardized. Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
The dependent variable in columns 1 and 5 is the negative of a dummy variable capturing CBT eligibility, divided by its standard deviation.
All 15 PMT variables are included as additional explanatory variables but not displayed in the table.
The first F-statistic is for the hypothesis that the 11 extra baseline covariates reported in the table jointly have a coefficient of zero.

Table 9. How CBT predicts poverty beyond baseline covariates

	Rural			Urban		
	2007	2008	2009	2007	2008	2009
	(1)	(2)	(3)	(4)	(5)	(6)
				Panel I. Absolute poverty concept		
Eligible by CBT (dummy)	0.009	0.050	-0.093	0.086	0.145*	0.203**
	(0.051)	(0.056)	(0.059)	(0.084)	(0.082)	(0.086)
Observations	439	424	418	189	185	178
R-squared	0.196	0.188	0.101	0.238	0.251	0.163
				Panel II. Relative poverty concept		
Eligible by CBT (dummy)	0.027	0.127**	-0.091*	0.072	0.277***	0.203**
	(0.052)	(0.055)	(0.053)	(0.081)	(0.078)	(0.086)
Observations	439	424	418	189	185	178
R-squared	0.219	0.142	0.095	0.215	0.300	0.163

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
The dependent variable is a dummy indicating a household's poverty status.
Additional explanatory variables not displayed in the table: all 15 PMT variables and 11 additional baseline covariates; see table 8.

Online Appendix

I. Consumption

Our household survey includes a short questionnaire on consumption expenditures on regular as well as lumpy consumption items, which is administered to all household members aged 15 and older. It records market purchases with two recall periods, one and six months, for each of sixteen expenditure categories. We partition these into five high and eleven low-frequency items according to the questionnaire of the 2014 Burkina Living Standard Measurement Survey (LSMS) and calculate monthly household market purchases from the one-month and six-months recalls, respectively.[1]

Non-market consumption in the form of self-produced food items is common in our study area and, according to our own calculations with household-level data from Burkina's 2014 LSMS, accounts for 15 and 37 percent of the value of food consumption and 8 and 23 percent of total consumption among urban and rural households in Kossi province, of which the department of Nouna is a part. As our survey questionnaire does not include home-produced consumption, we approximate home-produced food consumption by combining a detailed questionnaire on the household's last harvest with basic household demographics, which are both part of the household survey. For each household, we calculate the value of annual home-produced food consumption as the market value of all food crops in the household's preceding harvest (that is around the month of December of the years 2006, 2007 and 2008) and reduce the resulting figure whenever the calories contained in the harvest of food crops exceed the household's annual calorie requirement. We determine these calorie thresholds endogenously through a calibration exercise, in which we match the resulting average rural and urban food consumption shares in our data with the ones in the LSMS data from Kossi province, which are 66.0 and 49.5 percent.[2] For more details on calculating consumption figures from the Nouna household survey, see Hillebrecht et al. (2020).

II. Tables

[1] In the 2014 Burkina LSMS, the recalls for the two categories are one week and three months, respectively.

[2] Allowing for different daily thresholds for individuals younger than 16 years of age and for rural and urban inhabitants, we obtain implied daily 'autoconsumption' calorie thresholds of 1800 (1400) for adult and 900 (700) for adolescent rural (urban) inhabitants. These thresholds very likely understate actual levels of home-produced consumption by 30 to 40 percent because of understated market consumption in our survey (see the next paragraph) and our calibration strategy, which matches food expenditure *shares* with those in the LSMS. While we cannot assess how well our measure of MPC approximates actual MPC in our data, applying the same methodology to the LSMS data from Kossi province, which also includes a harvest module, gives very high rank correlations of actual survey MPC (survey market consumption plus survey autoconsumption) and the resulting proxy measure of MPC (survey market consumption plus imputed autoconsumption), 0.94 and 0.99 in rural and urban primary sampling units, respectively.

Table A1. Descriptive statistics: PMT variables

	Rural		Urban	
	Calibration sample (1)	Evaluation sample (2)	Calibration sample (3)	Evaluation sample (4)
Improved floor	0.109	0.107	0.472	0.451
	(0.312)	(0.309)	(0.500)	(0.499)
Improved walls	0.146	0.143	0.205	0.202
	(0.354)	(0.350)	(0.405)	(0.403)
Improved roof	0.150	0.151	0.436	0.440
	(0.358)	(0.359)	(0.497)	(0.498)
Has running water	0.034	0.036	0.283	0.249
	(0.180)	(0.187)	(0.451)	(0.433)
Private toilet	0.176	0.177	0.873	0.834
	(0.381)	(0.382)	(0.334)	(0.373)
Household size	14.138	14.237	10.805	11.145
	(10.093)	(10.228)	(6.221)	(5.676)
Share of infants (≤4 years)	0.110	0.112	0.066	0.066
	(0.109)	(0.110)	(0.090)	(0.090)
Share of minors (≤16 years)	0.370	0.372	0.333	0.343
	(0.197)	(0.192)	(0.206)	(0.197)
Share of elderly (≥55 years)	0.118	0.123	0.141	0.139
	(0.204)	(0.209)	(0.219)	(0.219)
Household head female	0.103	0.100	0.166	0.192
	(0.304)	(0.301)	(0.373)	(0.395)
Household head married	0.652	0.672	0.495	0.601
	(0.477)	(0.470)	(0.501)	(0.491)
HHH has secondary or higher education	0.038	0.038	0.349	0.378
	(0.190)	(0.192)	(0.477)	(0.486)
Principal occupation: agriculture	0.891	0.908	0.625	0.689
	(0.312)	(0.289)	(0.485)	(0.464)
Household head old (≥55 years)	0.385	0.401	0.384	0.404
	(0.487)	(0.491)	(0.487)	(0.492)
Religion: christian	0.308	0.324	0.267	0.306
	(0.462)	(0.469)	(0.443)	(0.462)
Households	506	469	307	193
Communities	39	39	23	23

Notes: The table reports means, standard deviations are in parentheses.
The calibration samples are trimmed at a rate of 2.5 percent, 1.25 percent on either side and the evaluation samples reported here are obtained from the trimmed calibration samples analogously to Table 1. All variables are from the 2007 household survey.

Table A2. PMT calibration regressions

	Rural	Urban
	(1)	(2)
Improved floor	0.236	0.177
	(0.163)	(0.108)
Improved walls	-0.255**	-0.0442
	(0.108)	(0.125)
Improved roof	0.262*	0.277**
	(0.139)	(0.113)
Has running water	0.322	0.0985
	(0.202)	(0.0976)
Private toilet	0.174	0.196
	(0.107)	(0.138)
Household size	-0.0117***	-0.0267***
	(0.00413)	(0.00877)
Share of infants (≤4 years)	0.374	-0.141
	(0.367)	(0.510)
Share of minors (≤16 years)	-0.471**	-0.198
	(0.218)	(0.232)
Share of elderly (≥55 years)	-0.674***	-0.302
	(0.240)	(0.245)
Household head female	-0.0918	-0.107
	(0.124)	(0.123)
Household head married	-0.0852	-0.0818
	(0.0833)	(0.0915)
HHH has secondary or higher education	0.264	0.255**
	(0.193)	(0.103)
Principal occupation: agriculture	0.0475	-0.168
	(0.124)	(0.104)
Household head old (≥55 years)	0.248***	0.166
	(0.0920)	(0.107)
Religion: christian	0.114	0.136
	(0.0826)	(0.107)
Constant	7.757***	8.310***
	(0.152)	(0.184)
Observations	506	307
R-squared	0.096	0.199

Notes: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.
The dependent variable is logarithmic MPCE in 2007.
The samples used in the estimations are the calibration samples trimmed at a rate of 2.5 percent, 1.25 percent on either side.