NBER WORKING PAPER SERIES

# A POOR MEANS TEST? ECONOMETRIC TARGETING IN AFRICA

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Working Paper 22919
http://www.nber.org/papers/w22919

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
December 2016

For their comments the authors thank Arthur Alik-Lagrange, Kathleen Beegle, Mary Ann Bronson, Raphael Calel, Phillippe Leite, Mead Over, Adam Wagstaff and seminar participants at Georgetown University. The authors are grateful to the World Bank's Strategic Research Program for funding assistance for this research. These are the views of the authors, and need not reflect those of their employers, including the World Bank, its member countries, or the National Bureau of Economic Research.

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NBER Working Paper No. 22919
December 2016
JEL No. I32,I38,O15

## ABSTRACT

Proxy-means testing is a popular method of poverty targeting with imperfect information. In a now widely-used version, a regression for log consumption calibrates a proxy-means test score based on chosen covariates, which is then implemented for targeting out-of-sample. In this paper, the performance of various proxy-means testing methods is assessed using data for nine African countries. Standard proxy-means testing helps filter out the nonpoor, but excludes many poor people, thus diminishing the impact on poverty. Some methodological changes perform better, with a poverty-quantile method dominating in most cases. Even so, either a basic-income scheme or transfers using a simple demographic scorecard are found to do as well, or almost as well, in reducing poverty. However, even with a budget sufficient to eliminate poverty with full information, none of these targeting methods brings the poverty rate below about three-quarters of its initial value. The prevailing methods are particularly deficient in reaching the poorest.

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## 1. Introduction

While universal social programs—whereby everyone is covered—are excellent at reaching the poorest, the beneficiaries can include many people who do not need this form of public help. Governments have tried many ways of assuring better "targeting," with the explicit aim of concentrating the benefits of a social policy on poor people. The means used vary in their data requirements, methodological sophistication and costs (both administrative and broader social costs).

Readily measurable proxies for consumption or income are often used in efforts to reduce poverty in settings in which the means-testing of benefits is not an administratively feasible option, as in most low-income countries (and many middle-income countries). Efficiency considerations point to the need for indicators that are not easily manipulated by actual or potential beneficiaries. Proxy variables, such as gender and education, family size and housing conditions, have been common.[2] A score based on these variables is used in validating other targeting methods, such as those based on community-level subjective assessments of who is "poor." The scores are also entering many social-protection registries—national data bases that are used in various ways including to flag ineligible households in future schemes.

The main challenge has been in setting the score's weights. Various "poverty scorecards" or "basic needs indicators" have been used. Some versions use *ad hoc* weights, such as taking a simple average of the scores across components.[3] Practitioners have turned to more sophisticated statistical methods in an effort to further improve targeting accuracy. These methods have come to be known as *proxy means testing* (PMT).[4]

This paper assesses an increasingly popular solution in which the weights in the PMT are identified from regression coefficients for household consumption or income as a function of readily observed covariates. The regression is calibrated to survey data and then used to make the out-of-sample predictions for the relevant population. This has the intuitive attraction that the dependent variable is a

[2] This idea appears to have emerged in social policy making in Chile in the 1980s (Grosh, 1994, Ch.5). Grosh et al. (2008) provides a useful overview of PMT and other targeting methods found in practice in developing countries, with details on many examples.
[3] A popular example of the poverty scorecard was proposed by Schreiner (2010); the Progress out of Poverty Index uses Schreiner's (2015) method. The scorecard includes 10 easily measured correlates of poverty which are used to form a composite index. Diamond et al. (2016) argue that the predictive ability of such scorecards can be improved by calibrating the variables and their weights to local (sub-national) conditions, for which purpose they advocate econometric methods.
[4] This term appears to be due to Grosh and Baker (1995, p. ix), who define PMT as "a situation where information on household or individual characteristics correlated with welfare levels is used in a formal algorithm to proxy household income, welfare or need."

well-established measure of household economic welfare and, indeed, the same variable is typically used in measuring poverty.[5] To distinguish it from other methods of means testing, we will use the term "econometric targeting" to refer to any PMT based on a regression model. An influential early contribution by Grosh (1994) compared numerous social programs in Latin America and concluded that this class of methods produced the best targeting outcomes, measured in terms of reducing inclusion errors, whereby a nonpoor person is counted as poor. Various versions of econometric targeting have since been proposed, and the method has been widely implemented in developing countries.[6]

Econometric targeting has also been criticized for its seemingly poor predictions about who is poor and who is not. For example, Kidd and Wylde (2011, p.ii) refer to the method's "considerable inaccuracy at low levels of coverage." Transparency has also been a concern. Sometimes the score variables and weights are deliberately kept secret for incentive reasons. In other cases, the method and formula are too complicated, or too poorly explained, for public consumption. Either way, observers on the ground do not always understand why some people are selected and some are not based on these targeting methods. With reference to a conditional cash transfer scheme in Nicaragua using PMT, field work by Adato and Roopnaraine (2004, p.15) led them to write that:

> "…the targeting process as a whole is poorly understood at the community level in both geographical- and household-targeted communities. When asked why some households were beneficiaries and others not, informants offered a range of explanations, from divine intervention to a random lottery. For example, one informant from a geographically-targeted community noted: '*Well, some people wonder why they weren't targeted even though they live in this same area. So we tell them that the Bible says that many are called but few are chosen.*'"

In the context of a PMT in Indonesia, Cameron and Shah (2014) argue that considerable local social unrest was generated by this lack of transparency in why some people were deemed beneficiaries and some not. This came with an erosion of local social capital and greater distrust of local administrators.

Another critique relates to the goals of social protection policies, which can be thought of as involving both protection from uninsured risks as well as promotion from poverty over the longer-term.[7] Some observers have questioned the effectiveness of PMT in responding to shocks or targeting insurance. Instead, it is argued that, because it is largely based on long-term assets, PMT is suitable "…for identifying the chronic poor and determining eligibility for programs that provide long-term support" (Del Ninno and Mills, p.22). Nonetheless, PMT is widely used in implementing policies that

[5] For a critical review of the methods used see Ravallion (2016, Part 2).
[6] Useful overviews can be found in Mills et al. (2015) and USAID's website on Poverty Assessment Tools.
[7] On this distinction and the implications for assessing social protection policies see Ravallion et al. (1995).

offer short-lived benefits and are seen to be justified by their provision of insurance or emergency relief such as public works and cash transfer schemes.

The criticisms of econometric targeting could reflect either methodological inadequacies or informational/data limitations. On the former, standard regression-based calibration of the PMT score will tend to work less well toward the extremes of the distribution of household consumption. By its design, a standard regression line passes through the means of the data. The residuals will be positively correlated with the dependent variable (more so the higher the variance of the residuals given exogenous regressors).[8] One can expect the method to have a tendency to overestimate living standards for the poorest and underestimate them for the richest, though the degree to which this is problematic for targeting accuracy is unclear. Indeed, it is theoretically possible that the PMT method predicts that nobody lives below a poverty line for which even a sizable share of the population is deemed to be poor based on observed consumptions. Another possibility is that the variables used are not sufficiently good proxies for household consumption. In other words, that there is an information problem.

The paper aims to provide a systematic assessment of the reliability of econometric targeting as a tool for social policies aiming to reduce poverty. We assess what appears to be the most common form of what we call "Basic PMT," as well as some alternative methods using extra covariates and methods that are arguably more appropriate when it is recognized explicitly that the PMT is for antipoverty policy making. A natural counterfactual for assessing any form of PMT is a uniform allocation—the same for everyone. Various other counterfactuals of interest to policy makers are examined. The study also considers less finely-targeted options to econometric targeting, which are uniform only within stipulated categories.

While Latin America has attracted the bulk of the past research on PMT, we study the method using survey data for the world's poorest region, Sub-Saharan Africa (SSA). This is also the region where existing social spending has been least effective in reaching the poorest.[9] The specific countries studied are Burkina Faso, Ethiopia, Ghana, Malawi, Mali, Niger, Nigeria, Tanzania and Uganda, being all those countries in SSA with recent and reasonably comparable surveys in the World Bank's Living Standards Measurement Study (LSMS).[10] For a subset of these countries we also have panel data.

[8] If the regression model is $y = \beta x + \varepsilon$ with $Cov(x,\varepsilon) = 0$ then $Cov(y,\hat{\varepsilon}) = Var(\hat{\varepsilon}) > 0$ (in obvious notation).

[9] For evidence on the point see Ravallion (2016, Chapter 10).

[10] Existing government safety net programs in Ghana, Malawi, Nigeria, and Tanzania, and World Bank projects in Burkina Faso, Ethiopia, Ghana, Malawi and Niger use PMT, often in combination with geographic and community-based targeting. At the time of writing, PMT is also being considered for Mali.

In advocating and assessing PMT, social policy making in developing countries has often emphasized the need to avoid the "leakage" of benefits to the non-poor, and to assure broad coverage of the poor. Following the literature, one can term failures with regard to these two aspects of targeting as the aforementioned "errors of inclusion" (i.e., counting someone as poor who is not) and "errors of exclusion" (i.e., counting someone as non-poor who is in fact poor).[11] The difference is important when deciding how much to spend on a program. Inclusion errors are generally costly to the public budget while exclusion errors save public money. Governments and international financial institutions concerned about the fiscal cost of social policies have thus put greater emphasis on avoiding inclusion errors as a means of cutting the cost to the government without hurting poor people.[12] Some observers have questioned this prioritization, arguing that exclusion errors should get higher weight when the policy objective is to minimize poverty.[13] In this paper we consider various measures of both targeting performance and impacts on poverty.

Some assessments of econometric targeting are already available in the literature.[14] The methods appear to vary considerably across the studies to date, such as in how many variables are used in the PMT, how targeting performance is assessed, and what poverty cutoff point is used. However, documentation is rarely ideal, often leaving the reader to guess what has been done. This makes it difficult to compare results. We provide similar tests on a consistent basis.

We go further than past work in a number of other respects. We consider alternative econometric methods for calibrating the PMT scores. These include methods that recognize explicitly that the goal of PMT is poverty reduction rather than obtaining unbiased estimates of conditional means. We also simulate stylized policies to see how well econometric targeting works. Here we consider simpler alternatives to PMT that have a long history, going back to the state-contingent transfers that were introduced under England's Poor Laws, and the various proposals that have been made over the last 200 years for a "basic-income scheme."[15] Additionally, we compare PMT to optimally differentiated

[11] The distinction between these two targeting errors goes back to Weisbrod (1970) who called them "vertical" and "horizontal targeting efficiency." Smolensky et al. (1995) called them "errors of inclusion" and "errors of exclusion." Some authors refer to exclusion as "under-coverage" and refer to inclusion as "leakage." In development contexts, influential early contributions were made by Cornia and Stewart (1995) and Grosh and Baker (1995).
[12] This emphasis on reducing inclusion errors appears to have emerged during macroeconomic adjustment efforts, notably in Latin America in the 1980s (Smolensky et al., 1995).
[13] See Cornia and Stewart (1995), Smolensky et al. (1995) and Ravallion (2009).
[14] See Grosh and Baker (1995) (Jamaica, Bolivia, Peru), Ahmed and Bouis (2002) (Arab Republic of Egypt), Narayan and Yoshida (2005) (Sri Lanka), Sharif (2009) (Bangladesh), Stoeffler et al. (2015) (Cameroon), Pop (2015) (Ghana) and Cnobloch et al. (2015) (Malawi).
[15] On the history of these policy options see Ravallion (2016, Part 1).

transfers based on the same information set. In considering these options, we focus directly on the impacts on poverty rather than looking solely at measures of targeting performance. Here we take the view that "better targeting" should not be seen as an end in itself but rather as a possible means of assuring a greater impact on poverty.

The panel data that are available for a subset of countries help us address concerns about measurement errors in consumption. To some degree, what are called "targeting errors" are likely to be measurement errors (Ravallion 2008). By using the panel data to calculate time-mean consumption we can at least partly reduce the effect of measurement error, as a robustness test of our main findings.

Another departure from past work is that we allow for likely lags in implementation; past assessments have ignored the fact that PMT invariably entails such lags, given that the score must be set in advance of implementation. There are lags between the survey year and the release of the PMT formula, and further lags to implementation.[16] We can expect a degree of churning, with households moving in and out of poverty.[17] So implementation lags are likely to constrain the performance of econometric targeting in identifying the currently poor. We exploit the panel nature of our data for a subset of countries to explicitly introduce lags.

There are a number of issues that we do not take up. One of these is whether household consumption obtained from a survey is an adequate welfare indicator. The methods of econometric targeting studied here make that assumption, and we accept it for the purpose of evaluating the performance of these methods. Another issue not taken up here is how well a low level of household consumption identifies deprived individuals; Brown et al. (2016) take up this issue in the context of attempts to reach undernourished women and children. While we do address the performance of econometric targeting for stylized cash transfer programs we do not consider alternatives such as self-targeting using work requirements ("workfare") or community-based targeting in which local communities are engaged directly in deciding who is poor and who is not.[18] Nor do we consider the (economic, social and political) costs of targeting, which have received some attention in the literature.[19]

[16] For example, even for a relatively simple PMT such as the Progress out of Poverty Index, we find that across the 59 countries for which the index is currently available, the number of years between the survey year and the release date of the index ranges from 1 to 9, with a mean of 3.9 years and a median of 3.5.
[17] The implications of such churning for assessing the performance of social protection policies are examined further in Ravallion et al. (1995).
[18] On workfare see, for example, Murgai et al. (2016) and on community-based targeting see Alatas et al. (2012), Karlan and Thuysbaert (2013) and Stoeffler et al. (2016). Barrientos (2013) provides a useful overview of the whole class of social assistance policies in developing countries.
[19] See the discussions in van de Walle (1998), Gelbach and Pritchett (2000) and Ravallion (2016, Ch. 10).

For example, we do not discuss behavioral responses, social stigmas, or implications for social cohesion and political support for poverty programs.[20]

The paper finds that when the counterfactual is a uniform allocation of the same budget, even our Basic PMT allows a substantial reduction in the rate of inclusion errors; in this setting it should be possible to roughly halve the rate of inclusion errors using econometric targeting. When judged against a fixed poverty line, this success at avoiding leakage to the nonpoor comes with seemingly weak coverage of poor people—a high rate of exclusion errors. In other words, the method helps exclude the poor as well as the non-poor. The paper finds that econometric targeting typically provides at most modest gains in the poverty impacts over other policy-relevant alternatives. Indeed, in a number of cases and depending on the country and the nature of its poverty profile, simpler state-contingent targeting methods or even a "basic-income scheme" (in which everyone is covered) dominate in certain policy-relevant cases, such as when one allows for plausible lags in PMT implementation. However, none of these methods can be considered to perform especially well. Prevailing methods do not reliably reach the poorest. The costs of each method in practice may then be decisive in the choice.

The following section describes the PMT method that we assess, while Section 3 describes the measures we use in assessing econometric targeting. Section 4 studies the basic version of PMT, while Section 5 turns to various extensions and revisions to that version. For stylized transfer programs, Section 6 compares the poverty impacts of econometric targeting to those of less methodologically sophisticated methods, including un-targeted (universal) transfers and simple demographic "scorecard" methods. Section 7 presents our results for (informationally-feasible) differentiated transfers, including optimal transfer schemes for poverty reduction with a given budget but limited information. Section 8 uses the panel surveys to introduce lags in implementation. Section 9 offers some cross-country comparisons of the performance of econometric targeting; here we ask how much the impacts of PMT on poverty for a given budget are explicable in terms of the alternative targeting measures and the predictive ability of the PMT regressions. Section 10 concludes.

## 2. Econometric targeting

Quite generally, we can think of any PMT as some weighted function of a vector of covariates $x_{ijt}$. The specific form of this function that has become popular and that we focus on uses household-

[20] Smolensky et al. (1995) conclude that none of these issues is likely to be decisive for or against targeting. Atkinson (1995) argues that broader objectives of social policy (including social solidarity) warn against targeting.

consumption regression coefficients as the weights. We can write the following empirical regression function for the consumption of household $i$ in country $j$ at date $t$ on a vector of covariates $x_{ijt}$ using a survey sample of size $N_{jt}$:

$$y_{ijt} = \alpha_{jt} + \beta_{jt} x_{ijt} + \varepsilon_{ijt} \ (i=1,\ldots,\ N_{jt}) \tag{1}$$

The PMT is then based on:

$$\hat{y}_{ijt} = \hat{\alpha}_{jt} + \hat{\beta}_{jt} x_{ijt} \tag{2}$$

The most common method in practice for estimating $\alpha_{jt}$ and $\beta_{jt}$ in (1) appears to be Ordinary Least Squares (OLS) using log consumption per capita as the dependent variable. As usual, OLS chooses the parameter estimates to minimize the sum of squared errors with no difference in the weights attached to poor versus non-poor households (i.e., choosing $\hat{\alpha}_{jt}$ and $\hat{\beta}_{jt}$ to minimize $\sum \hat{\varepsilon}^2_{ijt}$ for each $j$, $t$). We also considered the option of using a binary indicator for whether a household's actual consumption falls below the poverty line as the dependent variable (equal to one if a household is poor, and zero otherwise). We tried this for both OLS (giving a linear probability model) and a Probit. However, we found that targeting errors were substantially higher with a binary dependent variable in all cases. So we confine attention to the continuous dependent variable in the rest of this paper.

Another option to OLS in estimating equation (1) is to try to better tailor the estimator to the specific policy problem, in this case poverty reduction. Two ways of doing this can be suggested. The first is the quantile regression method of Koenker and Bassett (1978). This is more robust to outliers than OLS, and (importantly) the method can be tailored to the problem at hand in that the quantile can be set at the overall poverty rate.[21] In other words, we calibrate the PMT score to how that specific quantile in the distribution of log consumption, given the covariates, changes with those covariates. The second method entails placing higher weight on the squared errors of poorer people, giving "poverty-weighted least-squares" (PLS). Among the various weighting schemes that might be used, we choose the method proposed by Mapa and Albis (2013), which weights equally all observations below the poverty line but gives zero weight to those above the line. In other words, we run the regression on poor households only. We extend this method by including households somewhat above the line. Once we have the PLS parameter estimates we calculate the revised PMT scores using the actual values of $x_{ijt}$.

[21] For example, this is one of the methods used by USAID (2011) in calibrating a PMT for Peru. This method is also discussed in Mills et al. (2015).

Any PMT method is likely to be quite constrained in practice in the choice of covariates. Practitioners are restricted to using $x_{ijt}$ variables that are considered easy to observe or verify in the field. There are feasibility constraints associated with the number and nature of the variables used in practice; administrative costs almost certainly rise with the number of variables. There are also incentive constraints, stemming from the scope for manipulation by local agents when there are many variables in the PMT (Niehaus et al. 2013).

The variables used in practice typically cover readily observed living conditions of the household, such as basic consumer durables or assets, demographic variables (size and composition) and attributes of the head.[22] Two important exclusions are notable. First, prices are rarely used and assets are identified in broad categories; clearly, two households can each own a "fridge" but in one case it is 30 years old and works poorly while in the other case it is a fancy new model. Second, an important exclusion is that one cannot use fine geographic effects, such as at the level of the village, since one is constrained to estimating on a sample survey that will typically only cover a sample of villages (typically determined by the first stage of a two-stage sampling design). One does not know the geographic effect for the population, as required for implementing the PMT.[23] However, in one version we include community-level variables that go some way toward addressing this concern.

There is a degree of judgement required in selecting covariates. Here we consider various options, starting with a "Basic PMT" that seems to capture well the set of variables found in practice. We also consider "Extended PMT" methods that include variables that have extra explanatory power; while this provides a useful indication of the gains from more data, it is acknowledged that this version may not be easily implemented in the field. The Statistical Addendum provides descriptive statistics.

## 3. Measures of targeting and poverty

An early strand of the literature formulated the targeting problem as that of choosing a schedule of transfer payments across types of households to minimize a measure of poverty subject to a budget constraint.[24] The subsequent literature has instead emphasized "targeting efficiency," defined in terms of reducing targeting errors as defined below. Here we shall study both types of measures. We start with targeting measures.

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[22] See, for example, the various studies in the compilation by Del Ninno and Mills (2015).
[23] The same limitation is shared by small-area estimation methods ("poverty mapping") as in Elbers et al. (2003).
[24] The idea was developed in theoretical terms by Kanbur (1987) and the problem was formulated and solved numerically in Ravallion and Chao (1989) for the squared poverty gap index of Foster et al. (1984). Glewwe (1992) generalized this approach to allow for continuous variables.

***Measures of targeting performance:*** The relevant counts corresponding to the joint distribution of $y_{ijt}$ and $\hat{y}_{ijt}$ are shown in Table 1, which helps clarify our notation and some of the properties of our measures.

We focus on three main measures of targeting performance. The first is the Inclusion Error Rate (*IER*), defined by the proportion of those identified as poor who are not. This can be written as:[25]

$$IER_{jt} = \frac{\sum_{i=1}^{N_{jt}} w_{ijt} 1(y_{ijt} > z_{jt} | \hat{y}_{ijt} \le z_{jt})}{\sum_{i=1}^{N_{jt}} w_{ijt} 1(\hat{y}_{ijt} \le z_{jt})} \quad (3)$$

Here the poverty line (in consumption space) is $z_{jt}$ and the sample size is $N_{jt}$ with households indexed $i$=1,…, $N_{jt}$ and $w_{ijt}$ denotes the appropriate sample weights (to deal with differences in household size and sample design); $\sum_{i=1}^{N_{jt}} w_{ijt} = 1$.

Inclusion errors have received much attention in efforts to reduce the budgetary cost of social policies aiming to use transfer payments (in cash or kind) to reduce poverty. Inclusion errors imply a fiscal cost without any direct impact on poverty. For a uniform transfer paid to all those who are deemed to be poor, the *IER* gives the share of the transfers going to the non-poor.[26] If everyone is deemed "poor," so the transfer payment is universal, then *IER* is simply one minus the poverty rate.

The *IER* is often normalized by the poverty rate when the latter varies, which we will also do in some cases. The resulting measure has been used extensively—clearly more than any other targeting measure—in comparing the targeting performance of social programs across developing countries.[27] Critics of the focus on reducing inclusion errors have pointed to a number of issues, including measurement errors and the need for more inclusive policies in the interest of social coherence/stability.[28]

The second measure is the Exclusion Error Rate (*EER*), given by the proportion of the poor who are not identified as poor. (Sometimes the term "coverage rate" is used instead, which is simply one minus the *EER*.) For a social program providing a uniform transfer payment to all—variously called a "basic income guarantee" or "citizenship income"—the *EER* is of course zero, since everyone is covered. One might expect measures based on the *EER* to be better predictors of a social program's

[25] The indicator function 1(.) takes the value unity when the condition in parentheses is true and zero otherwise.
[26] This is what Weisbrod (1970) dubbed "vertical efficiency."
[27] This normalized share of transfers going to the poor was used by Coady et al. (2004a, b) to compare 85 programs across many countries.
[28] Weisbrod (1970) raised concerns about focusing solely on reducing inclusion errors (vertical efficiency in his terms). On measurement errors in targeting see the discussion in Ravallion (2008).

impact on poverty.[29] While that is intuitive—the more the poor are covered, the greater their expected gain—it does not necessarily hold as it will depend on the measure of poverty used, the distribution of coverage and the budget.[30] The Exclusion Error Rate can be written as:

$$EER_{jt} = \frac{\sum_{i=1}^{N_{jt}} w_{ijt} 1(\hat{y}_{ijt} > z_{jt} | y_{ijt} \leq z_{jt})}{\sum_{i=1}^{N_{jt}} w_{ijt} 1(y_{ijt} \leq z_{jt})} \tag{4}$$

To better understand the properties of these measures it helps to also think of *IER* and *EER* in probabilistic terms as:

$$IER_{jt} = \Pr(y_{ijt} > z_{jt} | \hat{y}_{ijt} \leq z_{jt}) = \Pr(y_{ijt} > z_{jt}, \hat{y}_{ijt} \leq z_{jt}) \Pr(\hat{y}_{ijt} \leq z_{jt}) \tag{5.1}$$

$$EER_{jt} = \Pr(\hat{y}_{ijt} > z_{jt} | y_{ijt} \leq z_{jt}) = \Pr(\hat{y}_{ijt} > z_{jt}, y_{ijt} \leq z_{jt}) \Pr(y_{ijt} \leq z_{jt}) \tag{5.2}$$

Plainly, when the predictions are perfect ( $y_{ijt} = \hat{y}_{ijt}$ for all *i,j,t*) $IER_{jt} = EER_{jt} = 0$ for all *j,t*. Note that:

$$\Pr(y_{ijt} > z_{jt}, \hat{y}_{ijt} \leq z_{jt}) + \Pr(y_{ijt} \leq z_{jt}, \hat{y}_{ijt} \leq z_{jt}) = \Pr(\hat{y}_{ijt} \leq z_{jt})$$

$$\Pr(y_{ijt} \leq z_{jt}, \hat{y}_{ijt} > z_{jt}) + \Pr(y_{ijt} \leq z_{jt}, \hat{y}_{ijt} \leq z_{jt}) = \Pr(y_{ijt} \leq z_{jt})$$

Also note that $\Pr(\hat{y}_{ijt} \leq z_{jt}) = \Pr(y_{ijt} \leq z_{jt})$ implies $\Pr(y_{ijt} > z_{jt}, \hat{y}_{ijt} \leq z_{jt}) = \Pr(y_{ijt} \leq z_{jt}, \hat{y}_{ijt} > z_{jt})$. Then, from (5.1) and (5.2), we see that $IER_{jt} = EER_{jt}$. Thus the two error rates are equalized (though not at zero unless all levels are predicted correctly) when the poverty rates are equal ( $\hat{H}_{jt} = H_{jt}$), i.e., $\Pr(\hat{y}_{ijt} \leq z_{jt}) = \Pr(y_{ijt} \leq z_{jt})$ implies that $IER_{jt} = EER_{jt}$. Intuitively, this is because each time a person who is in fact poor (based on the survey-based consumption) is incorrectly identified as non-poor, that person has to be replaced by someone who is in fact non-poor, so as to keep the total count of the poor constant. In other words, every exclusion error must generate an inclusion error once the poverty rate is identical when comparing actual and predicted values. Of course, we do not expect the actual and predicted poverty rates to be equal in general. However, in the methodology of PMT there is the option of fixing the poverty rate for predicted values according to the survey-based measure using actuals. For example, if the survey indicates that 20% of the population is poor then one targets the poorest 20% based on the PMT scores. When the poverty rate is fixed this way we will simply refer to the "Targeting Error Rate" (*TER*).

The third measure is the Normalized Targeting Differential (*NTD*). In the context of a transfer program, the (ordinary) Targeting Differential (*TD*) is defined as the mean transfer made to the poor less

[29] See Ravallion (2009) who finds supportive evidence using data for a large cash transfer program in China.
[30] For example, for the headcount index of poverty one focuses on whether there is exclusion at the poverty line.

that made to the non-poor.[31] For a uniform transfer paid to all those who are deemed eligible, the *TD* becomes the difference between the proportion of the poor who are predicted to be poor and the proportion of the non-poor who are predicted to be poor. (In the case of a specific antipoverty program it is the difference between the program's coverage rate for the poor and that for the non-poor.) The *NTD* divides this measure by the mean transfer receipt, to make the resulting measure more comparable across countries and programs. For a basic income guarantee, *NTD*=0. When only the poor get help from the program and all of them are covered, the *NTD* reaches its upper bound of 1; when only the non-poor get the program and all of them do, the *NTD* is at its lower bound of -1. For a uniform transfer to all recipients in the amount $\tau_{jt}$ we have:

$$NTD_{jt} = \frac{TD_{jt}}{\tau_{jt}} = 1 - EER_{jt} - \frac{\sum_{i=1}^{N_{jt}} w_{ijt} 1(\hat{y}_{ijt} \le z_{jt} | y_{ijt} > z_{jt})}{\sum_{i=1}^{N_{jt}} w_{ijt} 1(y_{ijt} > z_{jt})} = \frac{1 - \hat{H}_{jt} - EER_{jt}}{1 - H_{jt}} \tag{6}$$

where $H_{jt}$ is the <u>headcount index</u> of poverty (or poverty rate), defined as the proportion of the relevant population living in households with consumption per person below the poverty line, and $\hat{H}_{jt}$ is the headcount index obtained based on predicted consumptions.

Another concept of targeting errors occasionally found in the literature makes the distinction between "Type 1" (*T*1) and "Type 2" (*T*2) errors of targeting (borrowing the terms from statistics).[32] The former is defined as the proportion of the (ineligible) non-poor who are assigned a program targeted to the poor; thus, in this context:[33]

$$T1_{jt} = \frac{\sum_{i=1}^{N_{jt}} w_{ijt} 1(\hat{y}_{ijt} \le z_{jt} | y_{ijt} > z_{jt})}{\sum_{i=1}^{N_{jt}} w_{ijt} 1(y_{ijt} > z_{jt})} = \frac{\hat{H}_{jt} - (1 - EER_{jt}) H_{jt}}{(1 - H_{jt})} \tag{7}$$

When the poverty rate is fixed ($\hat{H}_{jt} = H_{jt} = H$ for all (*j*, *t*)), $T1_{jt}$ is directly proportional to $EER_{jt}$; specifically, $T1_{jt} = EER_{jt} H / (1 - H)$. On the other hand, the Type 2 error rate is $T2_{jt} = EER_{jt}$. This yields another interpretation of the *NTD* as (one minus) the aggregate of Type 1 and 2 errors:

[31] This measure was proposed by Ravallion (2000). Also see Galasso and Ravallion (2005) and Ravallion (2009) on the properties of this measure and the discussions in Stifel and Alderman (2005) and Stoeffler et al. (2016).

[32] The designation of which is Type 1 and which Type 2 is arbitrary, and usage has varied. For example, Wodon (1997) and Ravallion (2009) define them our way but Grosh and Baker (1995) and Barrientos (2013) swap the two labels while Van Domelen (2007) has both usages. Appeals to statistics (whereby a Type 1 error is the incorrect rejection of a true null hypothesis while Type 2 is the failure to reject a false null) cannot resolve the matter since one can define the relevant null hypotheses consistently with either interpretation. (For our interpretation the hypothesis being tested is that a specific person is poor; the null is that she is not poor.) Readers are free to swap the labels and nothing substantive changes in our argument.

[33] Note that $\sum_{i=1}^{N_{jt}} w_{ijt} 1(\hat{y}_{ijt} \le z_{jt} | y_{ijt} > z_{jt}) = H_{jt} N_{jt} - \sum_{i=1}^{N_{jt}} w_{ijt} 1(\hat{y}_{ijt} \le z_{jt} | y_{ijt} \le z_{jt}) = H_{jt} N_{jt} (1 - EER_{jt})$.

$$NTD_{jt} = 1 - (T1_{jt} + T2_{jt}) \tag{8}$$

When the poverty rate is fixed the *NTD* is also a simple linear transform of the exclusion rate; i.e. $NTD_{jt} = 1 - EER_{jt} / (1 - H)$). We will not use *T*1 and *T*2 given that they are so closely related to *EER* and *NTD*.

***Poverty measures:*** Given that poverty reduction is typically the primary (or even sole) objective of this class of policies it is appropriate that we also study impacts on poverty measures. The first measure we use is the popular headcount index, defined already. We denote the empirical cumulative distribution function (CDF) of consumption as $p = F_{jt}(y) \in [0,1]$, which gives the proportion of the population of country (or group) *j* at date *t* consuming less than the amount $y \in [y^{\min}, y^{\max}]$. Then the headcount index can be written as:

$$H_{jt} = F_{jt}(z_{jt}) = \sum_{i=1}^{N_{jt}} w_{ijt} 1(y_{ijt} \leq z_{jt}) \tag{9}$$

The calculated poverty rate when based on the empirical distribution of $\hat{y}_{ijt}$ ($i$=1,…, $N_{jt}$) is $\hat{H}_{jt}$.

While *H* is (by far) the most popular measure in practice, its limitations are widely appreciated, notably that the measure does not reflect changes in living standards below the poverty line. We also consider two "higher-order" measures. The first is the poverty gap index, as given by the mean distance below the poverty line as a proportion of the line where the mean is taken over the whole population, counting those above the line as having zero gap.[34] The poverty gap index can be written as:

$$PG_{jt} = \sum_{y_{ijt} \leq z_{jt}} w_{ijt} (1 - y_{ijt} / z_{jt}) \tag{10}$$

We also make use of a distribution-sensitive measure, namely the Watts index proposed by Watts (1968) given by the mean proportionate poverty gap (counting the non-poor as having zero gap). This measure penalizes inequality among the poor, by putting higher weight on poorer people.[35] The Watts index can be written as:

$$W_{jt} = \sum_{y_{ijt} \leq z_{jt}} w_{ijt} \ln( z_{jt} / (y_{ijt})] \tag{11}$$

***Optimal transfers for a given budget:*** It appears to be a near-universal practice to provide a uniform transfer payment to all those who are identified as poor by the PMT. Transfer size may vary according to the number or age of children in the household (as in some conditional cash transfer

[34] The Statistical Addendum gives selected results for the squared poverty gap index of Foster et al. (1984).
[35] The Watts index is known to have a number of other desirable theoretical properties, as detailed in Zheng (1993).

schemes) but not with respect to predicted poverty levels based on the PMT. The popularity of such uniform transfers to those predicted to be poor can be thought of as a feasibility constraint on PMT; in the field it is likely to be difficult to make finely differentiated transfers. However, it is still of interest to see how much this constraint is limiting the impact on poverty.

We explore the effect of this constraint in two ways. The first is to vary the size of transfers based on the PMT scores. The second is to reformulate the problem as one of optimizing the transfers as a function of the variables going into the PMT. Quite generally one can think of the informationally-feasible transfers as a function of *m* observed *x*'s. The policy maker only observes the covariates *x* for each person; it is not known who is poor and who is not. However, the policy maker has a survey with much more information available for a sample. The problem is to choose the parameters of a score for assigning the real-world transfers based on the *x*'s, as given by:

$$\tau_{ijt} = \sum_{k=0}^{m} (\gamma_{jt}^k x_{ijt}^k)^{\vartheta} \geq 0 \tag{12}$$

(Here $x_{ijt}^0 = 1$ so that $\gamma_{jt}^0$ is the intercept—the transfer received by someone with $x_{ijt}^k = 0$ for $k$=1,…,$m$.) In one version transfers are linear in the *x*'s, i.e., $\vartheta = 1$. We call this the linear optimization. We also estimate a nonlinear version with $\vartheta = 2$, which introduces squared terms and interaction effects among the *x*'s.[36] The choice of the score parameters $\gamma_{jt}^k$ is made to minimize the Watts index in the sample survey data:

$$W_{jt} = \sum_{y_{ijt} \leq z_{jt}} w_{ijt} \ln[z_{jt} / (y_{ijt} + \tau_{ijt})] \tag{13}$$

The choice is constrained by the budget:

$$B_{jt} = \sum_{i=1}^{N_{jt}} w_{ijt} \tau_{ijt} \tag{14}$$

We solve this problem numerically.[37] One start value we use for the optimization is the uniform case obtained by setting $\gamma_{jt}^0 = B_{jt}$ and $\gamma_{ijt}^k = 0$ for $k$=1,…,$m$. Other start values are tested. When there are multiple local optima the solution for the parameters $\gamma_{jt}^k$ that gives the lowest value of the poverty measure is chosen.

[36] Glewwe (1992) recommended this in his formulation of the optimal targeting problem.
[37] We use the "fmincon" program in Matlab.

***Data:*** The data for implementing these measures come from the World Bank's well-known LSMS.[38] Table 2 lists the countries, years of survey, and numbers of households surveyed. In keeping with the bulk of the literature, our dependent variable is log total consumption per capita.[39] Consumption is measured in local currency units. Spatially deflated consumption values are available for all countries except Burkina Faso.[40] We use two poverty lines, corresponding to $H_{jt} = 0.2$ and 0.4 for all ($j$, $t$). The 40% figure coincides fairly closely with the overall poverty rate found for the Africa region using the World Bank's international line.[41] The 20% rate allows us to focus on how well the method does at identifying those who can be considered extremely poor. When comparing the actual values and the PMT scores one can chose to either fix the poverty rate (at 0.2 or 0.4) or fix the poverty line in the consumption space (i.e., fixing $z_{jt} \equiv F_{jt}^{-1}(0.2)$ or $F_{jt}^{-1}(0.4)$). This choice makes a difference and practice varies so we present results for both options.

## 4. Results for Basic PMT

The "Basic PMT" follows our reading of prevailing practice. Variables used include the type of toilet a household has; floor, wall and roofing material; type of fuel used for cooking; certain characteristics of the head, including gender, education and occupation; the household's religion and demographic size and composition. All regressions have dummy variables for categories of household size, age of head, month of survey and region of residence; the latter is measured at an aggregate level (typically a state or province) for which the surveys can be considered representative. The Statistical Addendum gives the OLS regression results for the Basic PMT. The simple average $R^2$ is 0.53, with a range from 0.32 (for Ethiopia) to 0.64 (Burkina Faso); Table 3 provides summary statistics for the Basic PMT (as well as for the extended version discussed below). This explanatory power is typical of past studies.[42] We did not try to prune this model, by either *ad hoc* or more systematic methods (such as

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[38] The LSMS has designed and implemented household surveys across many countries since the 1980s. These are nationally representative multi-purpose surveys spanning a quite wide range of topics. Further information can be found at the LSMS website. All surveys except for Ghana are LSMS-ISA surveys.

[39] We also considered the option of using log consumption per equivalent single adult using the scales provided by the LSMS. We focus on the "per capita" case in this paper although the Addendum also gives regressions and key results using scales.

[40] We use nominal consumption for Burkina Faso.

[41] Using the World Bank's international line of $1.90 a day at 2011 purchasing power parity, 43% of the population of Sub-Saharan Africa are found to be poor in 2013 (based on PovcalNet).

[42] A seemingly representative set of studies is Grosh and Baker (1995) ($R^2$ from 0.3 to 0.4), Ahmed and Bouis (2002) ($R^2$=0.43), Narayan and Yoshida (2005) ($R^2$=0.59), Sharif (2009) ($R^2$=0.57), Stoeffler et al. (2015) ($R^2$=0.62), Pop (2015) ($R^2$=0.54) and Cnobloch et al. (2015) ($R^2$=0.5 to 0.7). The simple average is 0.52.

stepwise regression). This would reduce the number of predictors but (of course) also reduce $R^2$ and probably increase targeting errors. However, we do consider stepwise regression as an option in Section 5 when using a much larger set of explanatory variables.

Using the lower poverty line, fixed across the comparisons between distributions of $y_{ijt}$ and $\hat{y}_{ijt}$, the Basic PMT substantially under-predicts the poverty rate in all countries. This is not unexpected given the properties of OLS, as noted in the Introduction. The extent of the problem can be seen in Table 4. While the poverty rate based on the data is 20%, that based on the predicted values ranges from 0 (for Mali) to 12% (Nigeria); the simple (population weighted) average is 8%. This improves considerably when one switches to the higher poverty line ($F_{jt}^{-1}(0.4)$), for which the average $\hat{H}_{jt}$ is 37% with a range from 29% to 44%.

***OLS results for Basic PMT:*** We turn now to the targeting measures. Table 5 gives the results. Let us focus first on the fixed poverty line case with $H$=0.2. On average, the rate of inclusion errors implies that 48% of those identified as poor by the Basic PMT method are in fact non-poor, i.e., just over half of those identified as poor using Basic PMT are in the poorest 20% when measured using the survey-based consumption. For a poverty rate of 20% and a fixed line, the PMT method has nearly halved the rate of inclusion errors that would be obtained with a uniform transfer payment. However, this has come at the expense of exclusion. The average exclusion error is sizeable, with 81% of those who are in the poorest 20% in terms of survey-based consumption being incorrectly identified as non-poor by the PMT method.

There is considerable variation across countries, with *IER* ranging from 33% to 100%, and *EER* from 55% to 100%. In the country with the lowest coverage rate of the poor implied by PMT, Mali, all poor families are incorrectly identified as non-poor. Unsurprisingly, we also find a tendency for the PMT to do better at correctly identifying poor households when the $R^2$ in the PMT regression is higher (comparing Tables 5 and 3). However, the proportion of households correctly included is less than half of those who are poor under a poverty line corresponding to $H$=0.2.

Both inclusion and exclusion errors are lower for $H$=0.4. Taking a (population weighted) average of our estimates of *IER* and *EER* for $H$=0.4, we find that 36% of those who are poor are excluded on average, while 31% of those who are deemed poor are actually not poor. So we again find that econometric targeting halves the inclusion error rate of 0.6 that would be implied by uniform transfers. There is also less spread in the values across countries with *IER* ranging from 25% to 40% and *EER* from 24% to 56%.

The finding that the errors tend to be higher using the lower poverty line again suggests that econometric targeting may have difficulty in identifying those who are very poor. This is supported by Figure 1, which plots actual consumption against predicted consumption. The lines represent the poverty lines at $H$=0.2. The bottom left quadrant represents households that are correctly identified as poor by the Basic PMT. The top left quadrant is the inclusion error, and the bottom right quadrant is exclusion. In one case, Mali, there are no data points in the bottom left quadrant; only one household is (incorrectly) predicted to be poor (thus giving the result that $IER$=$EER$=1.0 from Table 5). While Mali is exceptional in this respect, the point remains that PMT is missing many of the poorest households in all countries. Figure 2 gives the implied residuals. As expected, these tend to be lower (more negative) for poor people, but it is notable just how much the PMT regression is over-estimating the living standards of the poorest. For the poorest 20% in terms of actual consumption, the mean residual ranges from -0.73 to -0.37, implying that the PMT regressions yield predicted consumptions for the poor between 50% and 100% above their actual consumption.[43] (The fact that consumptions of the poor are overestimated by the PMT regressions at the poverty line, as is evident in Figure 2, echoes our finding above that Basic PMT underestimates the poverty rate.)

Looking at Figures 1 and 2, one can understand why many of those accepted or rejected might be tempted to believe that econometric targeting is something like a random lottery, or maybe even divine intervention (with reference to the quote from Adato and Roopnaraine, 2004, in the Introduction). At a given level of consumption, the predicted values generated from the PMT can vary considerably – see, for example, Ethiopia. A more encouraging finding is that households who are incorrectly included do not seem to be among the wealthiest households, that is, many of these households have actual consumption values that are relatively close to the poverty line.

So far we have focused on PMT using a fixed poverty line in consumption space. As we have seen, this tends to predict far fewer households as poor than the actual poverty rate, particularly when the poverty line corresponds to $H$=0.2 (Table 4). Table 5 also provides the results for the case where we instead fix the poverty rate. For example, we calculate the mean targeting error for the poorest 20% in the distribution of predicted consumption to be 51%, falling to 32% using $H$=0.4. Note that fixing the poverty rate instead of the poverty line will typically increase the number of predicted poor households thus resulting in higher $IER$ and lower $EER$.

[43] The mean residuals for the poorest 20% by country are -0.371 (Burkina Faso), -0.711 (Ethiopia), -0.497 (Ghana), -0.564 (Malawi), -0.725 (Mali), -0.402 (Niger), -0.401 (Nigeria), -0.555 (Tanzania), and -0.543 (Uganda).

As noted in the introduction, "targeting errors" may reflect to some extent time-varying measurement errors in the cross-sectional data. For those countries with panel data we can address this problem by assessing targeting performance using the time-mean consumption instead of current consumption. This will reduce, though probably not eliminate, any bias due to time-varying measurement errors. The lower panel of Table 5 gives the results. In the majority of cases, the measures of targeting performance improve, although this is less evident for exclusion errors than inclusion errors when using a fixed line. (Ethiopia accounts for about half of the exceptions.) Overall, the results are broadly consistent with the view that measurement errors are playing some role, but the panel data do not overturn our main conclusions about PMT. (Section 8 returns to the panel data, as a means of allowing for implementation lags in PMT.)

***Results using "poverty-focused" estimation methods:*** As noted, the OLS method used for the Basic PMT chooses the parameter estimates to minimize the unweighted sum of squared errors. Recall that we consider two "poverty-focused" options to OLS. The first is a quantile regression using the poverty rate as the quantile. For this estimator, Table 6 gives the analogous results to Table 5. This method allows a substantial reduction in the exclusion error rate using a fixed poverty line. This comes at the cost of higher inclusion errors, especially when using the lower poverty line (anchored to a quantile of 0.2). Targeting errors are similar to those for Basic PMT when using a fixed poverty rate instead.

Table 7 reports the targeting errors using our PLS method when a fixed poverty line is used to classify predicted poor households, as well as the results when a fixed poverty rate is used instead. (The Statistical Addendum gives the coefficients for our PLS regression with the Basic PMT variables.) In both cases, the weighted regressions correctly include almost all poor households. However, as with the poverty-quantile regression, inclusion errors are also high. The PMT using PLS regression is better at covering the poor but predicts that too many households are poor.

An alternative is to include some households who are above the poverty line in the PLS regression. We did this by also including in the sample all households at or below the poverty line, plus the next 20% of households, as ranked by their consumption. For example, at the poverty line for $H$=0.2, the bottom 40% of households is used in the regression. For $H$=0.4, the bottom 60% is used. Table 8 provides the inclusion and exclusion errors for this version. There is a decrease in the *IER* relative to Table 7, but with higher *EER* (though still lower than for the OLS).

***Targeting subgroups within a national population:*** So far we have used a basic PMT calibrated to national populations. However, when using PMT to target programs meant for a specific group it will typically be better to calibrate the PMT to that group. We tested this by estimating the PMT model on the samples restricted to two groups of households, namely those containing elderly and/or disabled members, and those households with children under 5. Next we compared the targeting measures based on the predicted values for each group with those predicted for the same household subgroups using a nationally calibrated PMT. We found that there is a modest improvement in targeting performance when using the sub-group-specific PMT. For example, to focus on the elderly and disabled subgroup case: for a fixed poverty rate of 0.2, average targeting errors go down from 0.50 to 0.47. Both inclusion and exclusion errors also fall using a fixed line, from 0.40 to 0.37 and 0.74 to 0.70, respectively. But again the gains are small (see Statistical Addendum for full results).

## 5. Extended PMT

We now test an extended specification with far more data, including the household's water source; more detailed information about housing materials; the number of household members per room; whether the household has a separate room for cooking; whether the household has electricity; household assets; and more details on the characteristics of the household. Regression results for the extended PMT are shown in the Addendum. The values of $R^2$ are higher but in most cases the gains are relatively small; although the number of explanatory variables has almost doubled there are clearly some strong correlations between the extra variables and those in the core set used for the Basic PMT.

As expected, the Extended PMT does better than Basic PMT with respect to targeting errors (Table 9). However, the improvement would have to be judged as modest (comparing Tables 9 and 5). For example, many more than half of the poorest 20% are still misidentified as non-poor. Table 4 also gives the average proportion of households that are predicted as poor using the poverty line methodology under the extended PMT method. At $H$=0.2, we see a slight improvement over the Basic PMT, with 11% of the sample predicted to be poor using the poverty line corresponding to $H$=0.2 (as compared to 8% using Basic PMT). The results are more similar between Basic PMT and Extended PMT for a line corresponding to $H$=0.4.

We also reran the poverty-weighted PMT regressions as in Section 3 for the extended PMT model. The Addendum gives the regression results when the extended PMT model is estimated on the bottom 20$^{th}$, 40$^{th}$ and 60$^{th}$ percentiles. The Addendum also gives the targeting errors for the poverty line and poverty rate method when the PMT is fitted using poor households only as well as the results when the poor plus the next 20 percent of the distribution are used. The key findings for Basic PMT using the poverty-weighted regression (Table 6) were confirmed using the Extended PMT.

The field implementation of a PMT formula with many variables is expensive and difficult, so some practitioners have opted for stepwise regression to obtain a more parsimonious PMT. We tested a backwards stepwise regression on the extended model to identify the key variables in the PMT. We used a cut-off of $p = 0.01$. The targeting errors for the more parsimonious regressions are given in the Addendum. We see a modest increase in the targeting errors, which are now back to approximately the same values we found for Basic PMT.

A further methodological change we considered is to include variables that are not as readily available as those in our Extended PMT regressions, but are likely to have extra explanatory power. In one case we used extra data on households' food security as well as on any shocks the household may have experienced. (Note that these variables are only available for four countries.) We augment the extended PMT model with these food security and shock variables. (The Addendum lists the variables, their means and the regression results for this model.) The $R^2$ increases slightly for all countries (Table 3). However, this version produced negligible improvement in targeting (Addendum). In another variation on the Extended PMT we included a range of community-level variables; again this was not possible for all countries. And (again) there was only a modest reduction in targeting errors, as can be seen in the Addendum.

We also tried other versions of PMT that might be of interest. In one case we used quantile regression at the median (in both the Basic and Extended PMTs). In another we used log consumption per equivalent single adult as the dependent variable. The Addendum gives the results. There was little improvement in the targeting performance of the PMT.

So far we have focused solely on the inclusion and exclusion rates as the measures of targeting performance. These appear to be the most popular measures in the literature, though others have been proposed and used in some studies. Probably the most promising example of the latter when the policy objective is poverty reduction is the targeting differential (Ravallion, 2000, 2009). Recall that the normalized *TD* is in the range [-1, 1], with zero corresponding to a uniform (un-targeted) transfer.

Table 10 gives summary statistics on the normalized targeting differential using both the Basic and Extended PMTs. The mean *NTD* for Basic PMT is 0.21, meaning that if program participation was based on the PMT scores the participation rate for the poor would be 21% points higher than that for the non-poor. Using Basic PMT, in four of the five countries with panel data, the *NTD* is higher using time-mean consumption; this rises to five out of five using the Extended PMT. Returning to the cross-section surveys, the poverty-quantile method yields the highest *NTD*, at around 0.49 on average for Basic PMT, rising to 0.53 for the Extended PMT. For all nine countries, the poverty-quantile regression method comes out best. The poverty-weighted method does almost as well provided that the 20% of households above the line are included.

## 6. Poverty impacts of stylized transfer schemes using various targeting methods

PMT is typically used to identify eligible recipients of a specific transfer scheme with the aim of reducing poverty. So we now study the poverty impacts of stylized transfers that are allocated according to various PMT specifications and selected counterfactuals.

Our comparisons are all budget neutral with the budget for each stylized scheme set at the aggregate poverty gap ($PG_{jt}z_{jt}N_{jt}$) for that country. We assume a poverty line corresponding to $H$=0.2. (The Addendum gives the average transfer amounts by country.) If the PMT worked perfectly—so that predicted consumption equaled actual consumption—then the transfers differentiated to exactly fill the poverty gaps would eliminate poverty. In this section we confine attention to uniform transfers among those deemed eligible, as is common in practice; in the next section we consider more finely differentiated transfers.

A natural benchmark is a universal ("basic income") scheme in which every person (whatever their characteristics) receives the same transfer payment. We then calculate the impacts on poverty of transfers using the various versions of PMT discussed above. We measure the impact of a uniform transfer per capita given to all households who are predicted to be below the line according to the PMT. The total transfer amount for a given country (as given by the country's aggregate poverty gap) is divided by the total number of individuals who reside in designated poor households, and distributed to households according to their size. (For example, if a poor household has two members, the transfer will be two times the per capita amount.)

We also consider counterfactual policies that use categorical targeting rather than PMT. These policies make uniform transfers within a specified category of people, as defined by a "poverty scorecard." Here we consider an especially simple form of demographic scorecard.[44] The first category is the set of persons 65 years or older. The second is any person who is a (female) widow, disabled (where disabled is defined as an illness or condition that significantly impairs a person over the age of 14 and their ability to work or study), or orphaned (defined as any child 14 or younger whose parents have both died or whose whereabouts are unknown). The third is a combination of the first two: a transfer to the elderly, widowed, disabled or orphaned. Note that if a person fits two categories, the score and (hence) transfer is doubled. The fourth transfer is a payment to households with children – whereby up to three children are each alloted a transfer. Finally, the last scheme combines all previous schemes, where children, the elderly, widowed, disabled or orphaned are eligible. (Recall that all stylized schemes considered have the same aggregate budget.)

Table 11 shows the implied headcount index for each case. (Recall that the baseline headcount index across all countries is 20%.) Most methods bring the poverty rate down to around 16%, well short of eliminating poverty; indeed, more than three-quarters of the poor remain poor. On average, Basic PMT does only slightly better than the universal basic income with the same budget, and Basic PMT does not do as well as the universal transfer in one third of the countries. Using the time-mean consumptions for the countries with panel data makes little difference on average. The quantile regression method does noticeably better on average, bringing the poverty rate down to one percentage point below the level attainable with the Basic PMT. Extended PMT does slightly better. However, it is notable how well categorical targeting does in many cases. On average, categorical targeting to households with elderly, widows, disabled and children does as well as Basic PMT. Nevertheless, categorical targeting never does as well as the poverty quantile regression method, which typically has the greatest impact on poverty. While categorical targeting does not have quite as much impact on poverty as the Basic PMT, it clearly comes close and is simpler and more transparent.

Tables 12 and 13 give the corresponding results for the poverty gap index and Watts index respectively; the pre-transfer poverty measures are shown in the first row. Aggregating across countries, the Basic PMT methods reduce the poverty gap by around 27% and the Watts index by 28%. As for the headcount index, the Extended PMT gives a larger reduction, namely 35% and 39% respectively. Simply giving a uniform transfer based on household size does as well as Basic PMT on average for both PG and the Watts index.

[44] Indeed, our method is even simpler than the "Simple Poverty Scorecard" developed by Schreiner (2010, 2015) and used for the Progress out of Poverty Index.

# 7. Allowing differentiated transfers

So far we have focused on the standard practice of giving the same transfer payment to all those predicted to be poor using PMT. While this is the most relevant case in practice, differentiating the transfers could be expected to work better if the predicted poverty gaps are quite accurate. However, we have already seen that this is not the case—that PMT works poorly in predicting the levels of living of the poorest. So it is unclear on *a priori* grounds whether differentiated transfers will have larger impacts on poverty.

How much better can PMT do using the same information if the transfers are differentiated, with more going to those who appear to be poorer? To put the question another way: how much does the constraint of relying on uniform transfers to the "predicted poor" limit the effectiveness of PMT? We address these questions in two ways. First, we simply fill the predicted poverty gaps, scaling up (or down) to attain the same budget. That is, each household predicted as poor receives the difference between the poverty line and its predicted consumption value, scaled such that the sum of all transfers equals the aggregate poverty gap. "PMT Gap" refers to this first method.

The allocation of transfers obtained this way need not be optimal in the sense of minimizing an agreed poverty index for a given budget. Following Ravallion and Chao (1989) and Glewwe (1992), we also devised a program for calculating the optimal allocation based on the set of PMT covariates. We chose the Watts index as the objective function given its desirable properties as a poverty measure (Section 2), which also provides suitable curvature to the objective function. Multiple solutions were common but we also found that the objective function tended to be quite flat in the sub-set of the parameter space corresponding to the various solutions found. Indeed, for all nine countries the minimum value of the Watts index was the same up to two decimal places whatever start value we used (though the parameter estimates themselves often differed for a given country).

Table 14 gives the results for the Watts index for $H$=0.2 using differentiated transfers that are determined by both the PMT gaps and optimization. (The Addendum gives those for the headcount index, though note that the solutions are only optimal for the Watts index.) Overall, filling the predicted gaps does little to reduce the poverty measure. As expected, the non-linear specification in the optimization routine ( $\vartheta = 2$ in equation 12) does better than the linear one in reducing poverty, and the nonlinear version does as well on average as the PMT.

## 8. Allowing for lags in PMT implementation

Lags in the implementation of a PMT are almost certainly universal. It takes some time to set up the data and the administrative apparatus for implementation. Yet there is undoubtedly some "churning" in living standards over time, even when using consumption as the welfare indicator. So the lags in implementation have bearing on the performance of PMT in reducing current poverty.

We have panel data for a subset of our study countries, namely Ethiopia, Malawi, Nigeria, Tanzania and Uganda. By exploiting the panel data, we can introduce a 1 to 2 year lag in the implementation of PMT. The precise lags are one year for Uganda, and two years for the other countries.[45] In other words, we develop the PMT on the Round 1 survey data and then apply it in Round 2. If anything, our lags appear to be less than found in practice.[46]

We consider two types of lags. In the first (Method 1), we take the regression parameters from Round 1, but use the covariates from the Round 2 data. Here there is no lag in the observations of the covariates; the lag is only due to the need to estimate the PMT scores. In the second (Method 2), we simply use the PMT score from Round 1, which we then compare to the survey data on consumptions in Round 2. The lag then applies to all aspects of the PMT method (both parameter estimates and covariate values).

The targeting errors obtained using Methods 1 and 2 are found in Tables 15 and 16, respectively. Comparing the results in Table 15 (top panel) with Table 5 we see that allowing for lags increases the targeting errors on average.[47] For the lower line, we now find that, on average, about half of those predicted to be poor are not in fact poor based on the survey data (a mean IER of 0.553, as compared to 0.481 from Table 5). Exclusion errors are also affected, though these errors rose less markedly. Using the Extended PMT we also find a substantial increase in the targeting errors, especially for inclusion, when we allow for lags using Method 1. A similar pattern is found for Method 2.

In Table 17 we give the targeting differentials. Allowing for lags, we find mixed results. Ethiopia and Nigeria have better pro-poor targeting with lags, while Malawi, Tanzania and Uganda see increases in targeting of the non-poor. Malawi in particular has negative TD's, indicating that the correction for lags now means that a uniform (un-targeted) policy would do better.

[45] The survey years are as follows: Ethiopia 2011/12 and 2013/14; Malawi 2010/11 and 2013; Nigeria 2010/11 and 2012/13; Tanzania 2010/11 and 2012/13; Uganda 2010/11 and 2011/12.

[46] For example, recall that the mean lag between the survey year and the release date of the Progress out of Poverty Index is 3.9 years (Introduction).

[47] Switching to the panel samples changes the targeting measures somewhat but the following observations still hold.

The post-transfer poverty rates allowing for lags are provided in Table 18. PMT still brings the poverty measures down, but by about two percentage points less when allowing for lags. For example, allowing for lags achieves an average post-transfer headcount index of 19% instead of 17%. The stylized categorical targeting schemes now attain similar or somewhat lower post-transfer poverty rates. On average, the simple demographic scorecards bring the poverty rate down by an extra one and in some cases two percentage points once one allows for plausible lags in PMT implementation. The Addendum gives results for other poverty measures, which follow a similar pattern.

## 9. Predictors of poverty impacts

We can bring a number of the results from previous sections together to quantify the relative importance of targeting errors and estimator fit to the impacts of PMT on poverty. With only nine country observations, cross-country comparisons should be treated with caution. Nonetheless, some strong patterns emerge even with so few degrees of freedom.

As noted in the Introduction, inclusion errors have tended to receive more attention in the policy community although it has been argued by some that exclusion errors may well be more important to the impacts on poverty. There is a clear pattern in our results whereby the exclusion error rate is generally a better predictor of the poverty impact of PMT than the inclusion error rate. This can be seen in Table 19 which gives regressions of the final "post-PMT" poverty measure on the two error rates (with controls for the initial poverty measure, which is of course a constant in the case of the headcount index). In all instances, the *EER* is the stronger predictor of impacts on poverty, and it is a strong (statistically significant) predictor in all but one case (the exception being for the method of filling the "PMT gap" described in section 7).

Another plausible predictor of both the targeting performance of PMT and its impact on poverty is the $R^2$ of the original regression used to calibrate the PMT. Indeed, it appears that this is often the main parameter that practitioners focus on in PMT applications. Table 20 gives the regressions across countries for both Basic and Extended PMT. The value of $R^2$ has only weak predictive power for the main measure used in practice, the headcount index. The $R^2$ does emerge as a strong predictor of the poverty impacts of standard PMT for the higher-order poverty measures (*PG* and the Watts index). This is not the case when we allow for differentiated transfers using either of the methods from the last section, although (as noted) the practical relevance of differentiated transfers is a moot point. For the more relevant case of uniform transfers across those predicted to be poor, $R^2$ can only be considered a useful predictor of impacts on poverty for the higher-order measures.

## 10. Conclusions

Highly imperfect information and limited administrative capabilities create challenges for implementing effective antipoverty programs in most developing-countries. Practitioners have often turned to some form of proxy means test. While these methods have an *a priori* appeal, users should have realistic expectations of what the methods can deliver.

Our results point to both strengths and weaknesses of standard econometric targeting methods. While these methods can substantially reduce inclusion errors in an antipoverty program—in most cases studied here the inclusion error rate can be at least halved—this comes at the cost of substantial exclusion errors when judged against the data on household consumption used to calibrate the test scores. Standard methods found in practice may look fine when the sole aim is to reduce inclusion errors—to prevent non-poor people receiving benefits when judged against a fixed poverty line. However, if poverty-reduction relative to a fixed line is the objective then policy makers with a given budget should be more worried about exclusion errors than inclusion errors. When attention switches to the problem of assuring broad coverage of the poor (reducing exclusion errors), better methods can be proposed, which give higher weight to performance in predicting the living standards of poor people. The method we find to generally perform best from the point of view of reducing exclusion errors is a "poverty-quantile regression." This method generates more inclusion errors than prevailing PMT methods, though still less than un-targeted transfers.

When judged in terms of the impact on poverty for a given budget (set equal to the aggregate poverty gap), we find that what appears to be the most widely-used form of PMT in practice does only slightly better on average than a universal basic income, in which everyone gets the same transfer, whatever their characteristics. One can achieve somewhat larger impacts on poverty using other PMT methods considered here, with either a richer data set or using the poverty-quantile regression method. However, even under seemingly ideal conditions, the "high-tech" solutions to the targeting problem with imperfect information do not do much better than age-old methods using state-contingent transfers or even simpler basic income schemes. We find that an especially simple demographic "scorecard" method can do almost as well as econometric targeting in terms of the impacts on poverty. Indeed, on allowing for likely lags in implementing PMT, the simpler categorical targeting methods perform better on average in bringing down the current poverty rate. This conclusion would undoubtedly be strengthened once the full costs of fine targeting are taken into account.

We were surprised that econometric targeting only allowed such small (or even negative) gains in reaching poor people compared to simpler methods. For practitioners deciding on targeting methods going forward, we suspect that other criteria besides targeting accuracy should take precedence in the choice, such as the specifics of the poverty profile, administrative capabilities and cost, the need for transparency, and the scope for fine targeting to undermine political support for social policies.

Looking at our findings as a whole, it would be fair to say that none of these methods performs particularly well when one is striving to reduce poverty. When the budget required for a set of transfer payments that would eliminate poverty (ignoring behavioral responses) is allocated by any of these methods, about three-quarters of the original (pre-intervention) count of poor people remain poor. The world's poorest should hope for something better.

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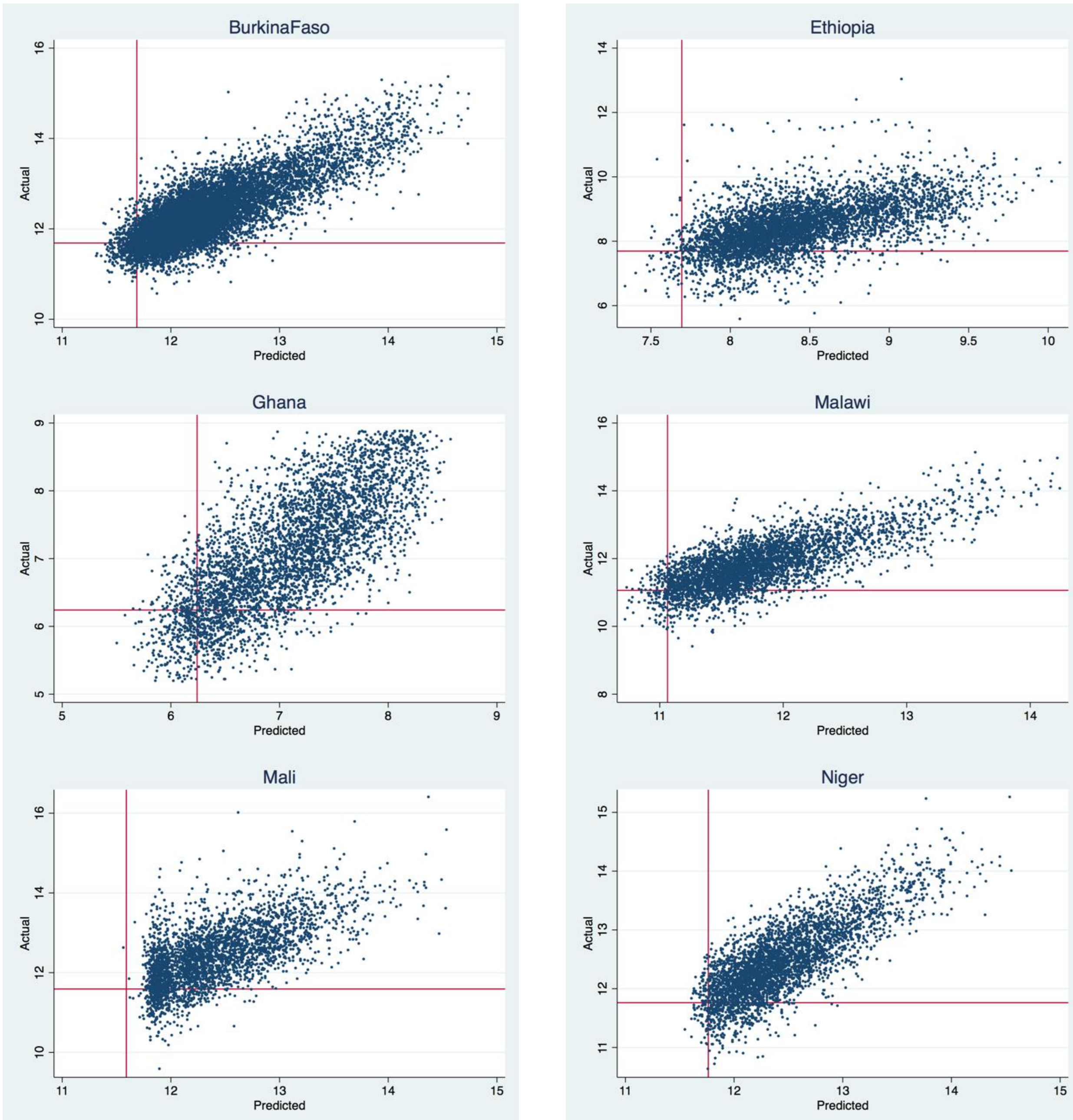
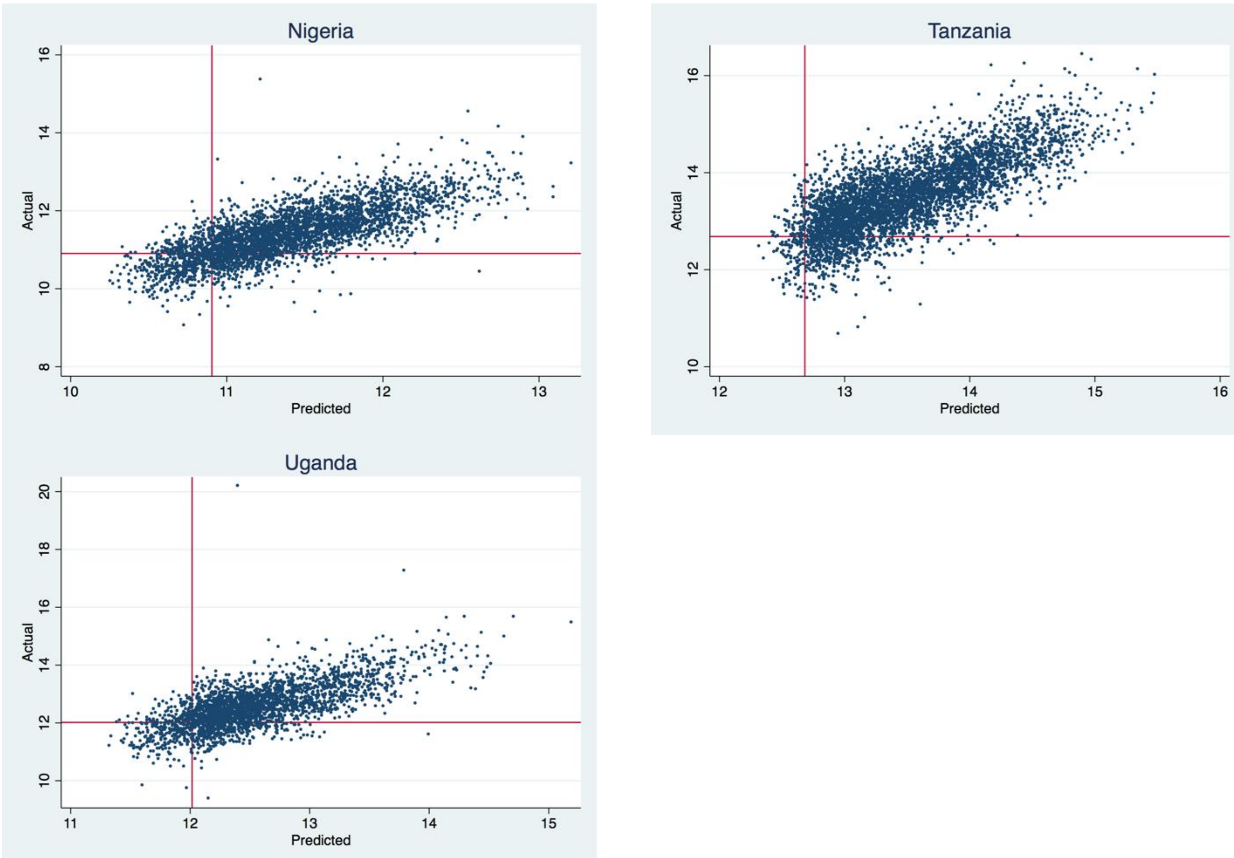
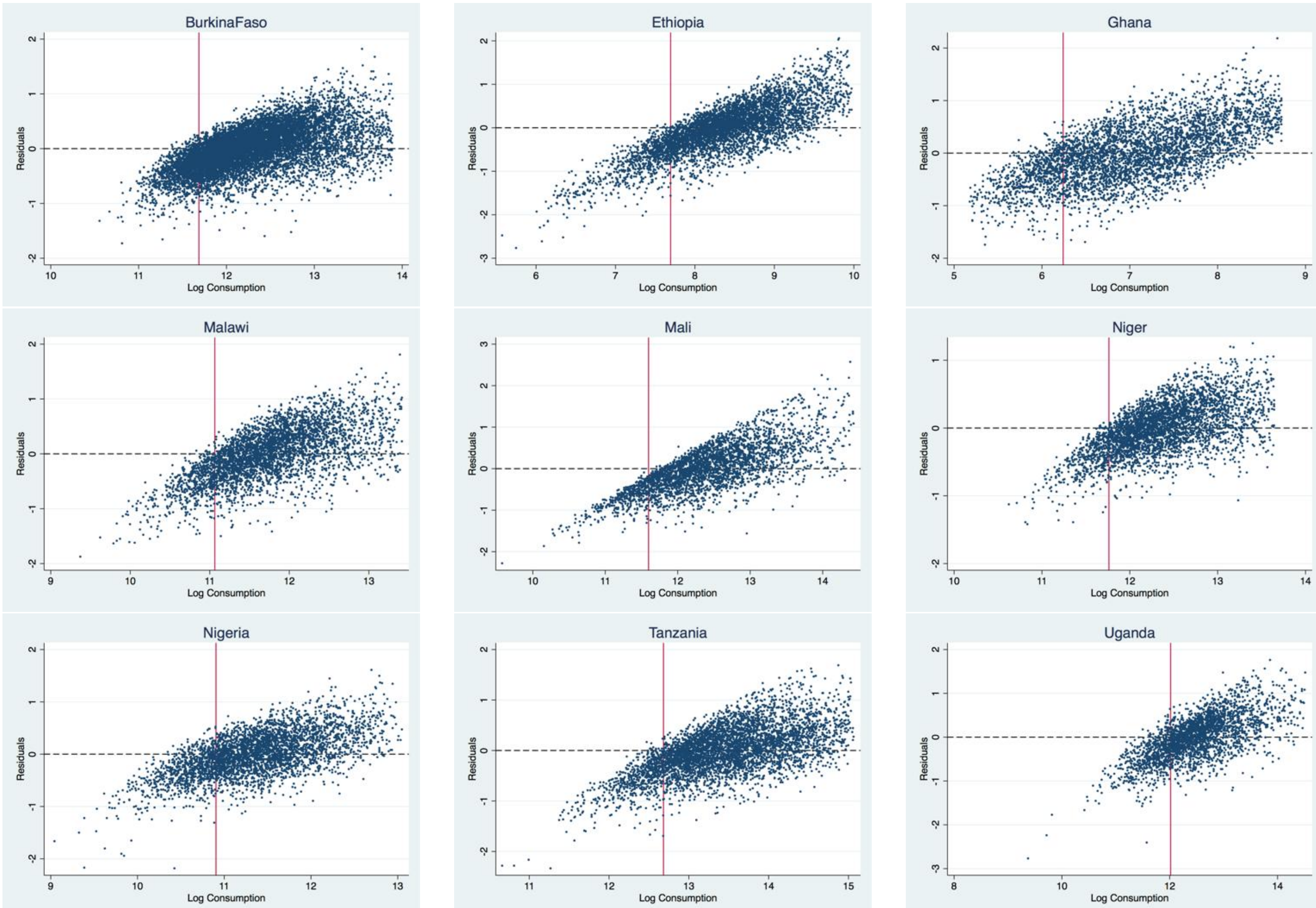
**Figure 1: Actual and predicted consumption for Basic PMT**






Note: The figure shows actual and predicted consumption in logged values using Basic PMT. The red lines represent the poverty line at the 20th percentile in logged values. Points in the top left corner are incorrectly predicted as poor (inclusion errors). Points in the bottom right corner are incorrectly predicted as non-poor (exclusion errors). Points in the bottom left and top right corners are correctly predicted as poor and non-poor respectively.

**Figure 2: Residuals for Basic PMT plotted against log real consumption per capita**




Note: The figure shows log real household consumption per capita and the residuals for the predicted consumption values. Basic PMT is used to predict consumption. The red lines represent the poverty line at the 20th percentile.

**Table 1: Counts of inclusion and exclusion**

| | | Based on proxy-means test | | |
|---|---|---|---|---|
| | | $\hat{y}_{ijt} \le z_{jt}$ | $\hat{y}_{ijt} > z_{jt}$ | |
| Based on survey data | $y_{ijt} \le z_{jt}$ | Poor either way<br>$\sum_{i=1}^{N_{jt}} w_{ijt} 1(\hat{y}_{ijt} \le z_{jt} \| y_{ijt} \le z_{jt})$<br>$= \sum_{i=1}^{N_{jt}} w_{ijt} 1(y_{ijt} \le z_{jt} \| \hat{y}_{ijt} \le z_{jt})$ | Exclusion errors<br>$\sum_{i=1}^{N_{jt}} w_{ijt} 1(\hat{y}_{ijt} > z_{jt} \| y_{ijt} \le z_{jt})$<br>$= \sum_{i=1}^{N_{jt}} w_{ijt} 1(y_{ijt} > z_{jt} \| \hat{y}_{ijt} \le z_{jt})$ | $H_{jt} N_{jt}$ |
| | $y_{ijt} > z_{jt}$ | Inclusion errors<br>$\sum_{i=1}^{N_{jt}} w_{ijt} 1(\hat{y}_{ijt} \le z_{jt} \| y_{ijt} > z_{jt})$<br>$= \sum_{i=1}^{N_{jt}} w_{ijt} 1(y_{ijt} > z_{jt} \| \hat{y}_{ijt} \le z_{jt})$ | Non-poor either way<br>$\sum_{i=1}^{N_{jt}} w_{ijt} 1(\hat{y}_{ijt} > z_{jt} \| y_{ijt} > z_{jt})$<br>$= \sum_{i=1}^{N_{jt}} w_{ijt} 1(y_{ijt} > z_{jt} \| \hat{y}_{ijt} > z_{jt})$ | $(1 - H_{jt}) N_{jt}$ |
| | | $\hat{H}_{jt} N_{jt}$ | $(1 - \hat{H}_{jt}) N_{jt}$ | $N_{jt}$ |

Note: Notation explained in the main text.

**Table 2: Countries and survey rounds**

| Country | Year | N |
|---|---|---|
| Burkina Faso | 2014 | 10,265 |
| Ethiopia | 2013/14 | 5,017 |
| Ghana | 2009 | 4,224 |
| Malawi | 2013/14 | 3,931 |
| Mali | 2014 | 3,212 |
| Niger | 2011 | 3,833 |
| Nigeria | 2012/13 | 3,720 |
| Tanzania | 2012/13 | 4,753 |
| Uganda | 2011/12 | 2,650 |

Note: All surveys except for Ghana are LSMS-ISA surveys.

**Table 3: Summary table for the regressions**

| | Burkina Faso | Ethiopia | Ghana | Malawi | Mali | Niger | Nigeria | Tanzania | Uganda |
|---|---|---|---|---|---|---|---|---|---|
| **Basic PMT** | | | | | | | | | |
| *Basic PMT* | | | | | | | | | |
| $R^2$ | 0.644 | 0.319 | 0.561 | 0.573 | 0.418 | 0.634 | 0.581 | 0.585 | 0.498 |
| N | 10265 | 5017 | 4224 | 3931 | 3212 | 3833 | 3720 | 4753 | 2650 |
| *Poor 20* | | | | | | | | | |
| $R^2$ | 0.175 | 0.136 | 0.290 | 0.151 | 0.126 | 0.156 | 0.274 | 0.176 | 0.231 |
| N | 10265 | 5017 | 4224 | 3931 | 3212 | 3833 | 3720 | 4753 | 2650 |
| *Poor 20 Probit* | | | | | | | | | |
| $R^2$ | 0.227 | 0.183 | 0.361 | 0.222 | 0.192 | 0.239 | 0.332 | 0.28 | 0.25 |
| N | 9151 | 5017 | 4224 | 3498 | 2776 | 3193 | 3491 | 4123 | 2558 |
| *Poor 40* | | | | | | | | | |
| $R^2$ | 0.285 | 0.192 | 0.393 | 0.242 | 0.228 | 0.261 | 0.392 | 0.306 | 0.299 |
| N | 10265 | 5017 | 4224 | 3931 | 3212 | 3833 | 3720 | 4753 | 2650 |
| *Poor 40 Probit* | | | | | | | | | |
| $R^2$ | 0.292 | 0.186 | 0.368 | 0.244 | 0.223 | 0.271 | 0.383 | 0.319 | 0.275 |
| N | 10265 | 5017 | 4224 | 3734 | 2922 | 3638 | 3720 | 4753 | 2647 |
| *Weighted Bottom 20* | | | | | | | | | |
| $R^2$ | 0.112 | 0.192 | 0.214 | 0.091 | 0.150 | 0.110 | 0.203 | 0.182 | 0.193 |
| N | 1395 | 762 | 755 | 558 | 422 | 456 | 871 | 618 | 628 |
| *Weighted Bottom 40* | | | | | | | | | |
| $R^2$ | 0.112 | 0.152 | 0.277 | 0.105 | 0.101 | 0.113 | 0.264 | 0.162 | 0.206 |
| N | 3024 | 1473 | 1508 | 1186 | 927 | 961 | 1685 | 1385 | 1105 |
| *Weighted Bottom 60* | | | | | | | | | |
| $R^2$ | 0.170 | 0.143 | 0.329 | 0.155 | 0.162 | 0.155 | 0.353 | 0.202 | 0.257 |
| N | 4895 | 2307 | 2325 | 1929 | 1599 | 1629 | 2427 | 2293 | 1595 |
| *Adult Equivalent Consumption* | | | | | | | | | |
| $R^2$ | | 0.287 | | 0.542 | | 0.595 | | 0.544 | 0.502 |
| N | | 5017 | | 3931 | | 3833 | | 4753 | 2650 |
| *Rural Only* | | | | | | | | | |
| $R^2$ | 0.465 | 0.201 | 0.475 | 0.462 | 0.356 | 0.355 | 0.538 | 0.449 | 0.426 |
| N | 6298 | 3148 | 2557 | 2900 | 2068 | 2326 | 2627 | 3089 | 2120 |
| *Urban Only* | | | | | | | | | |
| $R^2$ | 0.710 | 0.310 | 0.446 | 0.685 | 0.452 | 0.614 | 0.522 | 0.561 | 0.539 |
| N | 3967 | 1869 | 1667 | 1031 | 1144 | 1507 | 1093 | 1664 | 530 |
| **Extended PMT** | | | | | | | | | |
| *Extended PMT* | | | | | | | | | |
| $R^2$ | 0.724 | 0.381 | 0.587 | 0.674 | 0.520 | 0.718 | 0.666 | 0.647 | 0.596 |
| N | 10265 | 5017 | 4224 | 3931 | 3212 | 3833 | 3720 | 4753 | 2650 |
| *Weighted Bottom 20* | | | | | | | | | |
| $R^2$ | 0.191 | 0.242 | 0.259 | 0.148 | 0.226 | 0.186 | 0.292 | 0.228 | 0.288 |
| N | 1395 | 762 | 755 | 558 | 422 | 456 | 871 | 618 | 628 |
| *Weighted Bottom 40* | | | | | | | | | |
| $R^2$ | 0.193 | 0.205 | 0.321 | 0.181 | 0.173 | 0.166 | 0.355 | 0.226 | 0.292 |
| N | 3024 | 1473 | 1508 | 1186 | 927 | 961 | 1685 | 1385 | 1105 |
| *Weighted Bottom 60* | | | | | | | | | |
| $R^2$ | 0.267 | 0.192 | 0.358 | 0.256 | 0.235 | 0.225 | 0.435 | 0.274 | 0.357 |
| N | 4895 | 2307 | 2325 | 1929 | 1599 | 1629 | 2427 | 2293 | 1595 |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| *Stepwise (p=0.01)* | | | | | | | | | | |
| | $R^2$ | 0.687 | 0.344 | 0.579 | 0.676 | 0.534 | 0.701 | 0.606 | 0.632 | 0.553 |
| | N | 10265 | 5017 | 4224 | 3931 | 3212 | 3833 | 3720 | 4753 | 2650 |
| *Stepwise (p=0.05)* | | | | | | | | | | |
| | $R^2$ | 0.688 | 0.349 | 0.582 | 0.679 | 0.539 | 0.703 | 0.607 | 0.634 | 0.559 |
| | N | 10265 | 5017 | 4224 | 3931 | 3212 | 3833 | 3720 | 4753 | 2650 |
| *Household Shocks and Food Security* | | | | | | | | | | |
| | $R^2$ | 0.726 | 0.395 | | 0.690 | | 0.722 | 0.670 | 0.654 | 0.604 |
| | N | 10265 | 5017 | | 3931 | | 3833 | 3720 | 4753 | 2650 |
| *Shocks, Food Security and Community Variables* | | | | | | | | | | |
| | $R^2$ | | 0.400 | | 0.698 | | 0.725 | 0.672 | 0.655 | 0.607 |
| | N | | 5017 | | 3931 | | 3833 | 3720 | 4753 | 2650 |

Note: Values are taken from the regression tables presented in the Addendum. (Results from the quantile regressions are not included in the above table.)

**Table 4: Proportion of sample predicted to be poor using PMT regressions**

| | Fixed poverty line | | | |
|---|---|---|---|---|
| | $z = F^{-1}(0.2)$ | | $z = F^{-1}(0.4)$ | |
| | Basic PMT | Extended PMT | Basic PMT | Extended PMT |
| Burkina Faso | 0.083 | 0.112 | 0.359 | 0.358 |
| Ethiopia | 0.023 | 0.049 | 0.291 | 0.348 |
| Ghana | 0.115 | 0.126 | 0.350 | 0.360 |
| Malawi | 0.042 | 0.108 | 0.329 | 0.356 |
| Mali | 0.000 | 0.017 | 0.316 | 0.339 |
| Niger | 0.054 | 0.092 | 0.429 | 0.404 |
| Nigeria | 0.117 | 0.151 | 0.393 | 0.406 |
| Tanzania | 0.059 | 0.111 | 0.419 | 0.403 |
| Uganda | 0.104 | 0.144 | 0.439 | 0.424 |
| Mean | 0.079 | 0.113 | 0.372 | 0.387 |

Note: Predicted values are calculated from the Basic PMT and Extended PMT regressions (Addendum). Statistic are population weighted.

## Table 5: Targeting errors using Basic PMT

| | Inclusion error rate ($IER$) | Exclusion error rate ($EER$) | Inclusion error rate ($IER$) | Exclusion error rate ($EER$) | Targeting Error ($TER$) | Targeting Error ($TER$) |
|---|---|---|---|---|---|---|
| | Fixed poverty line | | | | Fixed poverty rate | |
| | $z = F^{-1}(0.2)$ | | $z = F^{-1}(0.4)$ | | $H = 0.2$ | $H = 0.4$ |
| Burkina Faso | 0.401 | 0.751 | 0.304 | 0.375 | 0.522 | 0.329 |
| Ethiopia | 0.515 | 0.945 | 0.396 | 0.562 | 0.621 | 0.413 |
| Ghana | 0.354 | 0.628 | 0.257 | 0.350 | 0.428 | 0.288 |
| Malawi | 0.431 | 0.880 | 0.333 | 0.451 | 0.553 | 0.373 |
| Mali | 1.000 | 1.000 | 0.348 | 0.485 | 0.553 | 0.375 |
| Niger | 0.539 | 0.875 | 0.384 | 0.340 | 0.584 | 0.362 |
| Nigeria | 0.332 | 0.548 | 0.247 | 0.243 | 0.392 | 0.244 |
| Tanzania | 0.396 | 0.822 | 0.323 | 0.291 | 0.513 | 0.314 |
| Uganda | 0.357 | 0.663 | 0.350 | 0.294 | 0.455 | 0.335 |
| Mean | 0.481 | 0.807 | 0.309 | 0.359 | 0.505 | 0.319 |
| | Using time-mean consumption from panel data | | | | | |
| Ethiopia | 0.310 | 0.947 | 0.366 | 0.746 | 0.638 | 0.427 |
| Malawi | 0.311 | 0.837 | 0.321 | 0.429 | 0.517 | 0.341 |
| Nigeria | 0.309 | 0.544 | 0.245 | 0.261 | 0.412 | 0.249 |
| Tanzania | 0.340 | 0.765 | 0.291 | 0.339 | 0.461 | 0.303 |
| Uganda | 0.370 | 0.687 | 0.328 | 0.293 | 0.483 | 0.318 |
| Mean | 0.317 | 0.691 | 0.276 | 0.397 | 0.482 | 0.307 |

Note: Errors are calculated using the predicted values from the regression given in the Addendum. Statistics are population weighted.

**Table 6: Targeting errors for Basic PMT using quantile regression centered at the poverty line**

| | Inclusion error rate (*IER*) | Exclusion error rate (*EER*) | Inclusion error rate (*IER*) | Exclusion error rate (*EER*) | Targeting Error (*TER*) | Targeting Error (*TER*) |
|---|---|---|---|---|---|---|
| | Fixed poverty line | | | | Fixed poverty rate | |
| | $z = F^{-1}(0.2)$ | | $z = F^{-1}(0.4)$ | | $H = 0.2$ | $H = 0.4$ |
| Burkina Faso | 0.627 | 0.218 | 0.365 | 0.228 | 0.525 | 0.326 |
| Ethiopia | 0.684 | 0.260 | 0.441 | 0.292 | 0.621 | 0.420 |
| Ghana | 0.540 | 0.163 | 0.301 | 0.228 | 0.426 | 0.290 |
| Malawi | 0.636 | 0.267 | 0.383 | 0.304 | 0.548 | 0.364 |
| Mali | 0.660 | 0.231 | 0.401 | 0.253 | 0.630 | 0.375 |
| Niger | 0.663 | 0.199 | 0.408 | 0.212 | 0.603 | 0.378 |
| Nigeria | 0.519 | 0.136 | 0.299 | 0.164 | 0.372 | 0.241 |
| Tanzania | 0.632 | 0.173 | 0.364 | 0.153 | 0.528 | 0.327 |
| Uganda | 0.661 | 0.147 | 0.407 | 0.172 | 0.488 | 0.336 |
| Mean | 0.615 | 0.191 | 0.363 | 0.204 | 0.505 | 0.324 |

Note: Errors are calculated using the predicted values from the regressions given in the Addendum. A quantile regression centered at the poverty line at the 20th and 40th percentile is used to estimate the model. Statistics are population weighted.

**Table 7: Targeting errors for Basic PMT using a poverty-weighted regression**

| | Inclusion error rate (*IER*) | Exclusion error rate (*EER*) | Inclusion error rate (*IER*) | Exclusion error rate (*EER*) | Targeting Error (*TER*) | Targeting Error (*TER*) |
|---|---|---|---|---|---|---|
| | Fixed poverty line | | | | Fixed poverty rate | |
| | $z = F^{-1}(0.2)$ | | $z = F^{-1}(0.4)$ | | $H = 0.2$ | $H = 0.4$ |
| Burkina Faso | 0.798 | 0.000 | 0.582 | 0.000 | 0.695 | 0.380 |
| Ethiopia | 0.799 | 0.002 | 0.598 | 0.000 | 0.707 | 0.512 |
| Ghana | 0.788 | 0.004 | 0.567 | 0.003 | 0.589 | 0.335 |
| Malawi | 0.795 | 0.000 | 0.597 | 0.000 | 0.624 | 0.385 |
| Mali | 0.790 | 0.004 | 0.596 | 0.000 | 0.646 | 0.418 |
| Niger | 0.798 | 0.000 | 0.593 | 0.000 | 0.729 | 0.434 |
| Nigeria | 0.756 | 0.007 | 0.560 | 0.004 | 0.619 | 0.325 |
| Tanzania | 0.791 | 0.001 | 0.588 | 0.001 | 0.721 | 0.398 |
| Uganda | 0.782 | 0.002 | 0.581 | 0.001 | 0.573 | 0.408 |
| Mean | 0.781 | 0.004 | 0.579 | 0.002 | 0.655 | 0.391 |

Note: Errors are calculated using the predicted values from the regression given in the Addendum with full weight on the bottom 20 and 40 percentiles respectively. Statistics are population weighted.

**Table 8: Targeting errors using a Basic PMT weighted regression with "poor plus 20 percent"**

| | Inclusion error rate (*IER*) | Exclusion error rate (*EER*) | Inclusion error rate (*IER*) | Exclusion error rate (*EER*) | Targeting Error (*TER*) | Targeting Error (*TER*) |
|---|---|---|---|---|---|---|
| | Fixed poverty line | | | | Fixed poverty rate | |
| | $z = F^{-1}(0.2)$ | | $z = F^{-1}(0.4)$ | | $H = 0.2$ | $H = 0.4$ |
| Burkina Faso | 0.666 | 0.181 | 0.501 | 0.038 | 0.566 | 0.339 |
| Ethiopia | 0.756 | 0.172 | 0.577 | 0.024 | 0.663 | 0.475 |
| Ghana | 0.539 | 0.197 | 0.425 | 0.062 | 0.439 | 0.297 |
| Malawi | 0.681 | 0.160 | 0.521 | 0.040 | 0.564 | 0.363 |
| Mali | 0.702 | 0.173 | 0.520 | 0.040 | 0.653 | 0.380 |
| Niger | 0.708 | 0.099 | 0.525 | 0.027 | 0.624 | 0.390 |
| Nigeria | 0.573 | 0.166 | 0.428 | 0.051 | 0.485 | 0.274 |
| Tanzania | 0.653 | 0.172 | 0.466 | 0.049 | 0.584 | 0.334 |
| Uganda | 0.695 | 0.127 | 0.500 | 0.037 | 0.508 | 0.369 |
| Mean | 0.665 | 0.164 | 0.491 | 0.042 | 0.551 | 0.347 |

Note: Errors are calculated using the predicted values from the regression in the Addendum with full weight on the bottom 40 and 60 percentiles respectively. Statistics are population weighted.

**Table 9: Targeting errors using the Extended PMT**

| | Inclusion error rate (*IER*) | Exclusion error rate (*EER*) | Inclusion error rate (*IER*) | Exclusion error rate (*EER*) | Targeting Error (*TER*) | Targeting Error (*TER*) |
|---|---|---|---|---|---|---|
| | Fixed poverty line | | | | Fixed poverty rate | |
| | $z = F^{-1}(0.2)$ | | $z = F^{-1}(0.4)$ | | $H = 0.2$ | $H = 0.4$ |
| Burkina Faso | 0.334 | 0.626 | 0.257 | 0.336 | 0.449 | 0.282 |
| Ethiopia | 0.419 | 0.857 | 0.373 | 0.455 | 0.541 | 0.405 |
| Ghana | 0.349 | 0.591 | 0.256 | 0.331 | 0.421 | 0.267 |
| Malawi | 0.439 | 0.698 | 0.295 | 0.374 | 0.470 | 0.315 |
| Mali | 0.444 | 0.951 | 0.344 | 0.444 | 0.572 | 0.341 |
| Niger | 0.458 | 0.751 | 0.328 | 0.323 | 0.539 | 0.327 |
| Nigeria | 0.330 | 0.496 | 0.228 | 0.217 | 0.384 | 0.223 |
| Tanzania | 0.403 | 0.670 | 0.283 | 0.279 | 0.481 | 0.281 |
| Uganda | 0.379 | 0.552 | 0.313 | 0.279 | 0.467 | 0.307 |
| Mean | 0.362 | 0.639 | 0.283 | 0.308 | 0.456 | 0292 |

Note: Errors are calculated using the predicted values from the extended PMT regressions shown in the Addendum. Statistics are population weighted.

**Table 10: Targeting differential for various PMT specifications**

| | Burkina Faso | Ethiopia | Ghana | Malawi | Mali |
|---|---|---|---|---|---|
| **Basic PMT covariates** | | | | | |
| Basic PMT | 0.207 | 0.040 | 0.321 | 0.098 | 0.000 |
| Using means from panel data | n.a. | 0.380 | n.a. | 0.378 | n.a. |
| Poverty quantile regression | 0.453 | 0.338 | 0.591 | 0.412 | 0.395 |
| Poverty weighted: Poor only | 0.012 | 0.004 | 0.068 | 0.031 | 0.060 |
| Poverty weighted: Poor + 20 | 0.411 | 0.185 | 0.568 | 0.392 | 0.339 |
| PMT with Urban/Rural | 0.210 | 0.073 | 0.321 | 0.123 | 0.001 |
| **Extended PMT covariates** | | | | | |
| Extended PMT | 0.327 | 0.117 | 0.354 | 0.243 | 0.039 |
| Using means from panel data | n.a. | 0.182 | n.a. | 0.354 | n.a. |
| Poverty quantile regression | 0.523 | 0.372 | 0.605 | 0.507 | 0.427 |
| Poverty weighted: Poor only | 0.117 | 0.042 | 0.124 | 0.138 | 0.147 |
| Poverty weighted: Poor + 20 | 0.494 | 0.283 | 0.557 | 0.492 | 0.443 |
| Stepwise (p=0.01) | 0.292 | 0.064 | 0.350 | 0.289 | 0.162 |
| HH Shocks + Food Security | 0.327 | 0.121 | | 0.298 | |
| Shocks, Food Security + Community Variables | | 0.120 | | 0.324 | |

| | Niger | Nigeria | Tanzania | Uganda | Mean |
|---|---|---|---|---|---|
| **Basic PMT covariates** | | | | | |
| Basic PMT | 0.088 | 0.339 | 0.149 | 0.289 | 0.214 |
| Using means from panel data | n.a. | 0.382 | 0.319 | 0.259 | 0.366 |
| Poverty quantile regression | 0.408 | 0.594 | 0.471 | 0.437 | 0.485 |
| Poverty weighted: Poor only | 0.010 | 0.222 | 0.051 | 0.100 | 0.107 |
| Poverty weighted: Poor + 20 | 0.354 | 0.554 | 0.437 | 0.375 | 0.421 |
| PMT with Urban/Rural | 0.095 | 0.383 | 0.163 | 0.313 | 0242 |
| **Extended PMT covariates** | | | | | |
| Extended PMT | 0.196 | 0.442 | 0.275 | 0.380 | 0.309 |
| Using means from panel data | n.a. | 0.317 | 0.455 | 0.411 | 0.334 |
| Poverty quantile regression | 0.455 | 0.635 | 0.524 | 0.509 | 0.531 |
| Poverty weighted: Poor only | 0.122 | 0.353 | 0.084 | 0.201 | 0.196 |
| Poverty weighted: Poor + 20 | 0.359 | 0.606 | 0.491 | 0.450 | 0.484 |
| Stepwise (p=0.01) | 0.169 | 0.307 | 0.274 | 0.373 | 0.249 |
| HH Shocks + Food Security | 0.225 | 0.464 | 0.300 | 0.386 | 0.333 |
| Shocks, Food Security + Community Variables | 0.227 | 0.451 | 0.308 | 0.405 | 0.331 |

Note: The targeting differential is computed using the poverty line at the 20$^{th}$ percentile. Statistics are population weighted.

**Table 11: Headcount index of poverty post transfer**

| | Burkina Faso | Ethiopia | Ghana | Malawi | Mali |
|---|---|---|---|---|---|
| **Universal (basic income)** | 0.176 | 0.171 | 0.166 | 0.171 | 0.166 |
| **Basic PMT covariates** | | | | | |
| Basic PMT | 0.152 | 0.190 | 0.149 | 0.176 | 0.200 |
| Using means from panel data | n.a. | 0.189 | n.a. | 0.167 | n.a. |
| Poverty quantile regression | 0.154 | 0.160 | 0.154 | 0.157 | 0.155 |
| Poverty weighted: Poor only | 0.175 | 0.171 | 0.164 | 0.170 | 0.167 |
| Poverty weighted: Poor + 20 | 0.162 | 0.174 | 0.156 | 0.163 | 0.159 |
| PMT with Urban/Rural | 0.152 | 0.182 | 0.153 | 0.170 | 0.200 |
| **Extended PMT covariates** | | | | | |
| Extended PMT | 0.147 | 0.172 | 0.147 | 0.154 | 0.190 |
| Using means from panel data | n.a. | 0.182 | n.a. | 0.151 | n.a. |
| Poverty quantile regression | 0.153 | 0.158 | 0.153 | 0.149 | 0.147 |
| Poverty weighted: Poor only | 0.173 | 0.170 | 0.163 | 0.167 | 0.162 |
| Poverty weighted: Poor + 20 | 0.155 | 0.169 | 0.156 | 0.155 | 0.152 |
| Stepwise (p=0.01) | 0.153 | 0.184 | 0.151 | 0.150 | 0.163 |
| HH Shocks + Food Security | 0.147 | 0.171 | | 0.154 | |
| Shocks, Food Security + Community Variables | | 0.170 | | 0.148 | |
| **Categorical targeting** | | | | | |
| Elderly 65+ | 0.176 | 0.184 | 0.178 | 0.185 | 0.178 |
| Widowed or disabled | 0.179 | 0.186 | 0.171 | 0.177 | 0.180 |
| Elderly, widows & disabled | 0.176 | 0.186 | 0.169 | 0.180 | 0.175 |
| Children under 14 (max 3) | 0.178 | 0.168 | 0.161 | 0.166 | 0.174 |
| Elderly, widows, disabled & children | 0.175 | 0.170 | 0.166 | 0.168 | 0.170 |
| Female heads with children | 0.189 | 0.189 | 0.181 | 0.176 | 0.191 |
| Shock: drought, flood or livestock death | 0.198 | 0.196 | | 0.195 | |
| Shock: drought, flood, livestock death, job loss | 0.198 | 0.197 | | 0.195 | |

| | Niger | Nigeria | Tanzania | Uganda | Mean |
|---|---|---|---|---|---|
| **Universal (basic income)** | 0.177 | 0.169 | 0.183 | 0.168 | 0.171 |
| **Basic PMT covariates** | | | | | |
| Basic PMT | 0.175 | 0.149 | 0.165 | 0.153 | 0.163 |
| Using means from panel data | n.a. | 0.147 | 0.153 | 0.150 | 0.159 |
| Poverty quantile regression | 0.166 | 0.150 | 0.162 | 0.154 | 0.155 |
| Poverty weighted: Poor only | 0.177 | 0.166 | 0.180 | 0.166 | 0.170 |
| Poverty weighted: Poor + 20 | 0.164 | 0.156 | 0.165 | 0.157 | 0.162 |
| PMT with Urban/Rural | 0.173 | 0.145 | 0.161 | 0.147 | 0.159 |

| | | | | | |
|---|---|---|---|---|---|
| **Extended PMT covariates** | | | | | |
| Extended PMT | 0.159 | 0.144 | 0.150 | 0.149 | 0.154 |
| Using means from panel data | n.a. | 0.151 | 0.137 | 0.128 | 0.155 |
| Poverty quantile regression | 0.168 | 0.151 | 0.159 | 0.146 | 0.154 |
| Poverty weighted: Poor only | 0.175 | 0.163 | 0.181 | 0.162 | 0.168 |
| Poverty weighted: Poor + 20 | 0.168 | 0.151 | 0.164 | 0.151 | 0.157 |
| Stepwise (p=0.01) | 0.161 | 0.153 | 0.154 | 0.149 | 0.160 |
| HH Shocks + Food Security | 0.155 | 0.140 | 0.148 | 0.149 | 0.155 |
| Shocks, Food Security + Community Variables | 0.156 | 0.142 | 0.147 | 0.146 | 0.157 |
| **Categorical Targeting** | | | | | |
| Elderly 65+ | 0.185 | 0.182 | 0.185 | 0.171 | 0.181 |
| Widowed or disabled | 0.192 | 0.181 | 0.187 | 0.174 | 0.182 |
| Elderly, widows & disabled | 0.182 | 0.180 | 0.188 | 0.169 | 0.180 |
| Children under 14 (max 3) | 0.179 | 0.169 | 0.178 | 0.165 | 0.170 |
| Elderly, widows, disabled & children | 0.179 | 0.170 | 0.183 | 0.163 | 0.171 |
| Female heads with children | 0.191 | 0.190 | 0.179 | 0.166 | 0.185 |
| Shock: drought, flood or livestock death | 0.192 | 0.196 | 0.196 | 0.197 | 0.196 |
| Shock: drought, flood, livestock death, job loss | 0.192 | 0.196 | 0.195 | 0.198 | 0.197 |

Note: Eligible households receive uniform per capita transfers. The total transfer amount for each country is equal to the country's aggregate poverty gap. In the top two panels uniform transfers are based on their predicted consumption under the various PMT models. The poverty line is used to determine whether a household is eligible. The statistics in the table give the change in the country's headcount index following the transfer. The starting value of the headcount index is 0.2. Statistics are population weighted. Categorical targeting gives transfers to each household member who meets the specified category. If a member meets the category twice he receives two transfers (e.g. elderly and disabled). The number of children who can receive transfers under the Children category is capped at 3. If a household satisfies either of the Shock categories, every household member receives a transfer.

**Table 12: Poverty gap index post transfer**

| | Burkina Faso | Ethiopia | Ghana | Malawi | Mali |
|---|---|---|---|---|---|
| Pre-transfer poverty gap | 0.037 | 0.055 | 0.054 | 0.047 | 0.049 |
| **Universal (basic income)** | 0.030 | 0.045 | 0.040 | 0.039 | 0.039 |
| **Basic PMT covariates** | | | | | |
| Basic PMT | 0.027 | 0.051 | 0.034 | 0.039 | 0.049 |
| Using means from panel data | n.a. | 0.042 | n.a. | 0.037 | n.a. |
| Poverty quantile regression | 0.025 | 0.037 | 0.032 | 0.033 | 0.033 |
| Poverty weighted: Poor only | 0.030 | 0.045 | 0.039 | 0.038 | 0.039 |
| Poverty weighted: Poor + 20 | 0.026 | 0.042 | 0.031 | 0.034 | 0.035 |
| PMT with Urban/Rural | 0.026 | 0.048 | 0.033 | 0.038 | 0.049 |
| **Extended PMT covariates** | | | | | |
| Extended PMT | 0.021 | 0.046 | 0.032 | 0.032 | 0.046 |
| Using means from panel data | n.a. | 0.040 | n.a. | 0.030 | n.a. |
| Poverty quantile regression | 0.025 | 0.037 | 0.032 | 0.033 | 0.033 |
| Poverty weighted: Poor only | 0.030 | 0.044 | 0.038 | 0.037 | 0.038 |
| Poverty weighted: Poor + 20 | 0.024 | 0.041 | 0.031 | 0.031 | 0.033 |
| Stepwise (p=0.01) | 0.024 | 0.049 | 0.032 | 0.030 | 0.036 |
| HH Shocks + Food Security | 0.022 | 0.043 | | 0.029 | |
| Shocks, Food Security + Community Variables | | 0.043 | | 0.028 | |
| **Categorical targeting** | | | | | |
| Elderly 65+ | 0.031 | 0.049 | 0.047 | 0.043 | 0.043 |
| Widowed or disabled | 0.032 | 0.050 | 0.042 | 0.042 | 0.044 |
| Elderly, widows & disabled | 0.031 | 0.049 | 0.043 | 0.041 | 0.043 |
| Children under 14 (max 3) | 0.031 | 0.044 | 0.038 | 0.038 | 0.042 |
| Elderly, widows, disabled & children | 0.031 | 0.044 | 0.040 | 0.038 | 0.043 |
| Female heads with children | 0.035 | 0.051 | 0.046 | 0.040 | 0.047 |
| Shock: drought, flood or livestock death | 0.036 | 0.053 | | 0.045 | |
| Shock: drought, flood, livestock death, job loss | 0.036 | 0.053 | | 0.045 | |

| | Niger | Nigeria | Tanzania | Uganda | Mean |
|---|---|---|---|---|---|
| Pre-transfer poverty gap | 0.039 | 0.050 | 0.053 | 0.059 | 0.051 |
| **Universal (basic income)** | 0.034 | 0.039 | 0.044 | 0.045 | 0.041 |
| **Basic PMT covariates** | | | | | |
| Basic PMT | 0.034 | 0.029 | 0.041 | 0.038 | 0.037 |
| Using means from panel data | n.a. | 0.026 | 0.031 | 0.033 | 0.031 |
| Poverty quantile regression | 0.026 | 0.030 | 0.036 | 0.038 | 0.033 |
| Poverty weighted: Poor only | 0.034 | 0.038 | 0.044 | 0.044 | 0.040 |

| | | | | | |
|---|---|---|---|---|---|
| Poverty weighted: Poor + 20 | 0.029 | 0.031 | 0.037 | 0.040 | 0.035 |
| PMT with Urban/Rural | 0.033 | 0.028 | 0.040 | 0.036 | 0.036 |
| **Extended PMT covariates** | | | | | |
| Extended PMT | 0.028 | 0.027 | 0.033 | 0.033 | 0.033 |
| Using means from panel data | n.a. | 0.025 | 0.022 | 0.026 | 0.029 |
| Poverty quantile regression | 0.026 | 0.030 | 0.036 | 0.038 | 0.033 |
| Poverty weighted: Poor only | 0.033 | 0.036 | 0.044 | 0.043 | 0.039 |
| Poverty weighted: Poor + 20 | 0.029 | 0.029 | 0.035 | 0.038 | 0.033 |
| Stepwise (p=0.01) | 0.031 | 0.032 | 0.034 | 0.032 | 0.035 |
| HH Shocks + Food Security | 0.026 | 0.026 | 0.031 | 0.032 | 0.033 |
| Shocks, Food Security + Community Variables | 0.026 | 0.027 | 0.031 | 0.032 | 0.034 |
| **Categorical targeting** | | | | | |
| Elderly 65+ | 0.036 | 0.044 | 0.048 | 0.044 | 0.045 |
| Widowed or disabled | 0.036 | 0.044 | 0.047 | 0.048 | 0.045 |
| Elderly, widows & disabled | 0.036 | 0.043 | 0.047 | 0.044 | 0.044 |
| Children under 14 (max 3) | 0.034 | 0.039 | 0.043 | 0.045 | 0.040 |
| Elderly, widows, disabled & children | 0.034 | 0.040 | 0.043 | 0.044 | 0.041 |
| Female heads with children | 0.038 | 0.047 | 0.045 | 0.046 | 0.047 |
| Shock: drought, flood or livestock death | 0.038 | 0.049 | 0.051 | 0.057 | 0.050 |
| Shock: drought, flood, livestock death, job loss | 0.038 | 0.049 | 0.051 | 0.057 | 0.050 |

Note: The initial value of the poverty gap index is shown in the first row. See notes to Table 11 for other details.

**Table 13: Watts index post transfer**

| | Burkina Faso | Ethiopia | Ghana | Malawi | Mali |
|---|---|---|---|---|---|
| Actual | 0.044 | 0.074 | 0.069 | 0.060 | 0.064 |
| **Universal (basic income)** | 0.036 | 0.059 | 0.049 | 0.049 | 0.050 |
| **Basic PMT covariates** | | | | | |
| Basic PMT | 0.032 | 0.069 | 0.042 | 0.050 | 0.064 |
| Using means from panel data | n.a. | 0.053 | n.a. | 0.046 | n.a. |
| Poverty quantile regression | 0.029 | 0.048 | 0.038 | 0.040 | 0.041 |
| Poverty weighted: Poor only | 0.035 | 0.059 | 0.048 | 0.049 | 0.049 |
| Poverty weighted: Poor + 20 | 0.030 | 0.055 | 0.037 | 0.042 | 0.044 |
| PMT with Urban/Rural | 0.031 | 0.064 | 0.040 | 0.048 | 0.064 |
| **Extended PMT covariates** | | | | | |
| Extended PMT | 0.025 | 0.061 | 0.040 | 0.041 | 0.059 |
| Using means from panel data | n.a. | 0.050 | n.a. | 0.036 | n.a. |
| Poverty quantile regression | 0.027 | 0.047 | 0.038 | 0.037 | 0.041 |
| Poverty weighted: Poor only | 0.035 | 0.058 | 0.047 | 0.047 | 0.048 |
| Poverty weighted: Poor + 20 | 0.027 | 0.053 | 0.037 | 0.038 | 0.041 |
| Stepwise (p=0.01) | 0.027 | 0.066 | 0.040 | 0.037 | 0.046 |
| HH Shocks + Food Security | 0.025 | 0.057 | | 0.036 | |
| Shocks, Food Security + Community Variables | | 0.057 | | 0.034 | |
| **Categorical targeting** | | | | | |
| Elderly 65+ | 0.037 | 0.066 | 0.060 | 0.055 | 0.056 |
| Widowed or disabled | 0.038 | 0.067 | 0.052 | 0.053 | 0.057 |
| Elderly, widows & disabled | 0.037 | 0.065 | 0.053 | 0.052 | 0.056 |
| Children under 14 (max 3) | 0.037 | 0.057 | 0.047 | 0.048 | 0.055 |
| Elderly, widows, disabled & children | 0.037 | 0.058 | 0.049 | 0.048 | 0.055 |
| Female heads with children | 0.042 | 0.069 | 0.059 | 0.051 | 0.061 |
| Shock: drought, flood or livestock death | 0.043 | 0.071 | | 0.058 | |
| Shock: drought, flood, livestock death, job loss | 0.043 | 0.071 | | 0.058 | |

| | Niger | Nigeria | Tanzania | Uganda | Mean |
|---|---|---|---|---|---|
| Actual | 0.048 | 0.064 | 0.069 | 0.085 | 0.067 |
| **Universal (basic income)** | 0.040 | 0.049 | 0.057 | 0.062 | 0.052 |
| **Basic PMT covariates** | | | | | |
| Basic PMT | 0.041 | 0.036 | 0.053 | 0.049 | 0.048 |
| Using means from panel data | n.a. | 0.031 | 0.037 | 0.040 | 0.038 |
| Poverty quantile regression | 0.030 | 0.037 | 0.046 | 0.050 | 0.041 |
| Poverty weighted: Poor only | 0.040 | 0.047 | 0.056 | 0.061 | 0.051 |

| | | | | | |
|---|---|---|---|---|---|
| Poverty weighted: Poor + 20 | 0.034 | 0.038 | 0.047 | 0.054 | 0.044 |
| PMT with Urban/Rural | 0.040 | 0.035 | 0.051 | 0.047 | 0.046 |
| **Extended PMT covariates** | | | | | |
| Extended PMT | 0.033 | 0.032 | 0.042 | 0.040 | 0.041 |
| Using means from panel data | n.a. | 0.030 | 0.026 | 0.032 | 0.035 |
| Poverty quantile regression | 0.030 | 0.036 | 0.043 | 0.047 | 0.040 |
| Poverty weighted: Poor only | 0.039 | 0.045 | 0.056 | 0.059 | 0.050 |
| Poverty weighted: Poor + 20 | 0.035 | 0.035 | 0.044 | 0.050 | 0.041 |
| Stepwise (p=0.01) | 0.038 | 0.039 | 0.043 | 0.042 | 0.045 |
| HH Shocks + Food Security | 0.031 | 0.032 | 0.040 | 0.040 | 0.041 |
| Shocks, Food Security + Community Variables | 0.031 | 0.032 | 0.039 | 0.039 | 0.042 |
| **Categorical targeting** | | | | | |
| Elderly 65+ | 0.043 | 0.055 | 0.063 | 0.058 | 0.058 |
| Widowed or disabled | 0.043 | 0.056 | 0.062 | 0.066 | 0.058 |
| Elderly, widows & disabled | 0.043 | 0.054 | 0.062 | 0.058 | 0.056 |
| Children under 14 (max 3) | 0.041 | 0.049 | 0.055 | 0.064 | 0.052 |
| Elderly, widows, disabled & children | 0.041 | 0.050 | 0.056 | 0.061 | 0.052 |
| Female heads with children | 0.045 | 0.061 | 0.059 | 0.067 | 0.061 |
| Shock: drought, flood or livestock death | 0.046 | 0.062 | 0.067 | 0.083 | 0.065 |
| Shock: drought, flood, livestock death, job loss | 0.046 | 0.062 | 0.067 | 0.083 | 0.065 |

Note: The initial value of the Watts index is shown in the first row. See notes to Table 11 for other details.

**Table 14: Watts index post transfer using differentiated transfers**

| | | | | Optimal transfers | |
|---|---|---|---|---|---|
| | Actual | PMT | PMT Gap | Linear | Non-linear |
| Burkina Faso | 0.044 | 0.032 | 0.044 | 0.038 | 0.036 |
| Ethiopia | 0.074 | 0.069 | 0.074 | 0.056 | 0.053 |
| Ghana | 0.069 | 0.042 | 0.066 | 0.048 | 0.044 |
| Malawi | 0.060 | 0.050 | 0.060 | 0.051 | 0.044 |
| Mali | 0.064 | 0.064 | 0.064 | 0.057 | 0.053 |
| Niger | 0.048 | 0.041 | 0.048 | 0.042 | 0.039 |
| Nigeria | 0.064 | 0.036 | 0.062 | 0.049 | 0.043 |
| Tanzania | 0.069 | 0.053 | 0.069 | 0.060 | 0.051 |
| Uganda | 0.085 | 0.049 | 0.083 | 0.061 | 0.053 |
| Mean | 0.067 | 0.048 | 0.066 | 0.054 | 0.049 |

Note: This table shows the Watts index for each country following the transfers made using the Basic PMT and the differentiated transfers implied by the optimization procedure based on both linear and quadratic transfers as a function of the same variables used in the PMT with weights chosen to minimize the Watts index (see text).

**Table 15: Targeting errors with lags using Method 1**

| | Inclusion error rate (IER) | Exclusion error rate (EER) | Inclusion error rate (IER) | Exclusion error rate (EER) | Targeting error rate (TER) | Targeting error rate (TER) |
|---|---|---|---|---|---|---|
| | | | Basic PMT | | | |
| | Fixed poverty line | | | | Fixed poverty rate | |
| | $z = F^{-1}(0.2)$ | | $z = F^{-1}(0.4)$ | | $H = 0.2$ | $H = 0.4$ |
| Ethiopia | 0.000 | 0.993 | 0.302 | 0.882 | 0.677 | 0.445 |
| Malawi | 0.674 | 0.244 | 0.491 | 0.085 | 0.593 | 0.382 |
| Nigeria | 0.333 | 0.959 | 0.183 | 0.704 | 0.481 | 0.303 |
| Tanzania | 0.481 | 0.848 | 0.319 | 0.321 | 0.556 | 0.321 |
| Uganda | 0.489 | 0.699 | 0.376 | 0.429 | 0.541 | 0.393 |
| Mean | 0.553 | 0.903 | 0.304 | 0.650 | 0.551 | 0.351 |
| | | | Extended PMT | | | |
| | Fixed poverty line | | | | Fixed poverty rate | |
| | $z = F^{-1}(0.2)$ | | $z = F^{-1}(0.4)$ | | $H = 0.2$ | $H = 0.4$ |
| Ethiopia | 0.540 | 0.989 | 0.284 | 0.831 | 0.598 | 0.422 |
| Malawi | 0.644 | 0.178 | 0.470 | 0.053 | 0.514 | 0.332 |
| Nigeria | 0.163 | 0.948 | 0.140 | 0.723 | 0.455 | 0.287 |
| Tanzania | 0.424 | 0.773 | 0.277 | 0.348 | 0.501 | 0.295 |
| Uganda | 0.472 | 0.474 | 0.348 | 0.290 | 0.475 | 0.342 |
| Mean | 0.496 | 0.869 | 0.276 | 0.640 | 0.502 | 0.328 |

Note: The parameters of the PMT score are estimated using Round 1 data, then predicted values are generated using Round 2 covariate values. Underlying regressions are found in the Addendum. Statistics are population weighted.

**Table 16: Targeting errors with lags using method 2**

| | Inclusion error rate (IER) | Exclusion error rate (EER) | Inclusion error rate (IER) | Exclusion error rate (EER) | Targeting error rate (TER) | Targeting error rate (TER) |
|---|---|---|---|---|---|---|
| | | | Basic PMT | | | |
| | Fixed poverty line | | | | Fixed poverty rate | |
| | $z = F^{-1}(0.2)$ | | $z = F^{-1}(0.4)$ | | $H = 0.2$ | $H = 0.4$ |
| Ethiopia | 0.207 | 0.985 | 0.322 | 0.873 | 0.677 | 0.446 |
| Malawi | 0.570 | 0.720 | 0.409 | 0.434 | 0.604 | 0.418 |
| Nigeria | 0.184 | 0.964 | 0.182 | 0.745 | 0.475 | 0.304 |
| Tanzania | 0.444 | 0.851 | 0.345 | 0.382 | 0.569 | 0.352 |
| Uganda | 0.547 | 0.851 | 0.385 | 0.390 | 0.576 | 0.387 |
| Mean | 0.436 | 0.935 | 0.295 | 0.690 | 0.548 | 0.355 |
| | | | Extended PMT | | | |
| | Fixed poverty line | | | | Fixed poverty rate | |
| | $z = F^{-1}(0.2)$ | | $z = F^{-1}(0.4)$ | | $H = 0.2$ | $H = 0.4$ |
| Ethiopia | 0.359 | 0.967 | 0.343 | 0.793 | 0.613 | 0.437 |
| Malawi | 0.568 | 0.648 | 0.396 | 0.404 | 0.585 | 0.401 |
| Nigeria | 0.259 | 0.928 | 0.142 | 0.696 | 0.482 | 0.301 |
| Tanzania | 0.395 | 0.731 | 0.334 | 0.340 | 0.507 | 0.333 |
| Uganda | 0.465 | 0.658 | 0.354 | 0.382 | 0.504 | 0.362 |
| Mean | 0.407 | 0.880 | 0.273 | 0.637 | 0.523 | 0.346 |

Note: The PMT is calibrated using Round 1 panel data, then predicted values are compared to actual consumption values in Round 2 panel data. Regressions are found in the Addendum. Statistics are population weighted.

**Table 17: Targeting differentials for panel PMTs**

| | Basic PMT | | | Extended PMT | | |
|---|---|---|---|---|---|---|
| | No lags | Method 1 | Method 2 | No lags | Method 1 | Method 2 |
| Ethiopia | 0.100 | 1.000 | 0.585 | 0.126 | -0.079 | 0.282 |
| Malawi | 0.102 | -0.349 | -0.141 | 0.178 | -0.288 | -0.136 |
| Nigeria | 0.317 | 0.334 | 0.633 | 0.403 | 0.675 | 0.482 |
| Tanzania | 0.207 | 0.039 | 0.111 | 0.212 | 0.153 | 0.209 |
| Uganda | 0.188 | 0.022 | -0.094 | 0.221 | 0.056 | 0.070 |
| Mean | 0.275 | -0.106 | 0.128 | 0.321 | 0.007 | 0.187 |

Note: The targeting differential is computed using the poverty line at the 20$^{th}$ percentile. Method 1 uses Round 1 PMT calibration and Round 2 data to generate predicted values. Method 2 uses Round 1 predicted values and Round 2 actual consumption. "No lags" refers to results when the PMT is calibrated and compared to Round 2 data (i.e. the panel structure is not used but only panel households are included). Statistics are population weighted.

## Table 18: Headcount index post transfer, round 2

| | Ethiopia | Malawi | Nigeria | Tanzania | Uganda | Mean |
|---|---|---|---|---|---|---|
| *PMT Targeting* | | | | | | |
| Basic PMT | 0.187 | 0.176 | 0.146 | 0.165 | 0.153 | 0.161 |
| Extended PMT | 0.172 | 0.155 | 0.137 | 0.153 | 0.146 | 0.150 |
| Method 1 Basic | 0.199 | 0.160 | 0.193 | 0.170 | 0.143 | 0.186 |
| Method 1 Extended | 0.198 | 0.154 | 0.191 | 0.160 | 0.143 | 0.183 |
| Method 2 Basic | 0.197 | 0.163 | 0.194 | 0.176 | 0.170 | 0.189 |
| Method 2 Extended | 0.194 | 0.161 | 0.187 | 0.157 | 0.152 | 0.181 |
| *Categorical Targeting* | | | | | | |
| Household size | 0.168 | 0.164 | 0.171 | 0.173 | 0.164 | 0.170 |
| Elderly 65+ | 0.182 | 0.180 | 0.176 | 0.179 | 0.168 | 0.178 |
| Widowed or disabled | 0.177 | 0.173 | 0.181 | 0.178 | 0.173 | 0.179 |
| Elderly, widows& disabled | 0.180 | 0.169 | 0.177 | 0.178 | 0.163 | 0.176 |
| Children under 14 (max 3) | 0.166 | 0.163 | 0.171 | 0.168 | 0.157 | 0.168 |
| Elderly, widows, disabled & children | 0.164 | 0.160 | 0.169 | 0.171 | 0.158 | 0.167 |
| Female heads with children | 0.181 | 0.171 | 0.190 | 0.167 | 0.158 | 0.182 |

Note: Method 1 uses Round 1 PMT calibration and Round 2 data to generate predicted values. Method 2 uses Round 1 predicted values and compares to Round 2 actual consumption. The Basic and Extended PMT methods (rows 1 and 2) are using Round 2 data only (i.e. no lags). Only panel households are included. Statistics are population weighted.

### Table 19: Targeting errors as predictors of the post-transfer poverty measures obtained by PMT

| | Headcount index | | Poverty gap index | | Watts index | | | |
|---|---|---|---|---|---|---|---|---|
| | Basic PMT | Extended PMT | Basic PMT | Extended PMT | Basic PMT | Extended PMT | PMT gaps | Optimal (nonlinear) |
| Constant | 0.083*** | 0.116*** | -0.035*** | -0.031*** | -0.047*** | -0.035** | -0.041* | -0.001 |
| | (0.012) | (0.014) | (0.004) | (0.006) | (0.010) | (0.014) | (0.020) | (0.004) |
| Initial poverty measure | n.a. | n.a. | 0.750*** | 0.731*** | 0.657*** | 0.584*** | 1.119*** | 0.483*** |
| | | | (0.062) | (0.074) | (0.095) | (0.098) | (0.182) | (0.041) |
| Inclusion error rate | 0.028 | 0.070 | 0.005 | -0.008 | 0.005 | -0.015 | -0.025 | 0.003 |
| | (0.015) | (0.045) | (0.003) | (0.014) | (0.009) | (0.032) | (0.016) | (0.004) |
| Exclusion error rate | 0.090*** | 0.098*** | 0.043*** | 0.046*** | 0.065*** | 0.065*** | 0.034 | 0.018** |
| | (0.020) | (0.014) | (0.004) | (0.004) | (0.012) | (0.010) | (0.022) | (0.005) |
| $R^2$ | 0.927 | 0.913 | 0.986 | 0.980 | 0.954 | 0.946 | 0.886 | 0.969 |
| F (prob) | 38.240 | 31.337 | 114.373 | 81.257 | 34.458 | 29.259 | 12.917 | 48.725 |
| | (0.000) | (0.001) | (0.000) | (0.000) | (0.001) | (0.001) | (0.009) | (0.000) |

Note: Standard errors in parentheses. ***: significant at 1% level; **: 5%; *: 10%. N=9.

### Table 20: R-squared in PMT regression as a predictor of the post-transfer poverty measures

| | Headcount index | | Poverty gap index | | Watts index | | | |
|---|---|---|---|---|---|---|---|---|
| | Basic PMT | Extended PMT | Basic PMT | Extended PMT | Basic PMT | Extended PMT | PMT gaps | Optimal (nonlinear) |
| Constant | 0.228*** | 0.209*** | 0.080*** | 0.079*** | 0.110*** | 0.106*** | 0.001 | 0.041** |
| | (0.029) | (0.024) | (0.021) | (0.022) | (0.025) | (0.024) | (0.028) | (0.012) |
| Initial poverty measure | n.a. | n.a. | -0.079 | -0.048 | -0.055 | -0.038 | 0.927*** | 0.298** |
| | | | (0.273) | (0.263) | (0.219) | (0.195) | (0.239) | (0.106) |
| $R^2$ from PMT regression | -0.113* | -0.085* | -0.071*** | -0.071*** | -0.107*** | -0.102*** | -0.027 | -0.027* |
| | (0.052) | (0.038) | (0.019) | (0.018) | (0.026) | (0.023) | (0.028) | (0.013) |
| $R^2$ | 0.401 | 0.415 | 0.767 | 0.803 | 0.796 | 0.827 | 0.847 | 0.842 |
| F (prob) | 4.682 | 4.964 | 9.875 | 12.240 | 11.735 | 14.301 | 16.559 | 15.969 |
| | (0.067) | (0.061) | (0.013) | (0.008) | (0.008) | (0.005) | (0.004) | (0.004) |

Note: Standard errors in parentheses. ***: significant at 1% level; **: 5%; *: 10%. N=9.