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A poor means test? Econometric targeting in Africa☆

Caitlin Brown[a,b], Martin Ravallion[b,*], Dominique van de Walle[c]

[a] Central European University, Hungary
[b] Department of Economics, Georgetown University, USA
[c] Development Research Group, World Bank, USA

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ABSTRACT

Proxy-means tests (PMTs) are popular for poverty-targeting with imperfect information. In a widely-used version, a regression for log consumption calibrates a PMT score based on covariates, which is then implemented for targeting out-of-sample. The performance of various PMT methods is assessed using data for nine African countries. Standard PMTs help filter out the non-poor, but exclude many poor people, thus diminishing the impact on poverty. Poverty-focused econometric methods such as using quantile regression generally do better. We also characterize the optimal informationally-feasible solution for poverty targeting and compare it to econometric methods. Even with a budget sufficient to eliminate poverty with full information, none of the targeting methods studied bring the poverty rate below about three-quarters of its initial value. The prevailing methods are particularly deficient in reaching the poorest. A basic-income scheme or transfers using a simple demographic scorecard often do as well, or even better, in reducing poverty.

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.[1] 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.[2] Practitioners have turned to more sophisticated statistical methods in an effort to further improve targeting accuracy.

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* Corresponding author.
E-mail address: mr1185@georgetown.edu (M. Ravallion).

[1] 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.

[2] 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.

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This has come to be known as a *proxy means test* (PMT).[3]

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 well-established measure of household economic welfare and, indeed, the same variable is typically used in measuring poverty.[4] 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 non-poor person is counted as poor. Various versions of econometric targeting have since been proposed, and the method has been widely implemented in developing countries.[5]

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, such that observers on the ground often do not 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 a 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. 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 offer short-lived benefits through their claimed 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, while standard regression-based calibrations of the PMT score are widely used in (explicitly) trying to reach the "extreme poor"—as in the famous case of Mexico's PROGRESA (Skoufias et al., 2001, p.1770, p.1770)—by its very construction this method will tend to work less well toward the extremes of the distribution of household consumption, including the poorest. 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).[6] One can expect the method 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 consumption. Another possibility is that the variables used are not sufficiently good proxies for household consumption. In this case, the problem is due to information limitations.

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 study what appears to be the most common form of what we call "Basic PMT." Here we use a reasonably standard set of readily verifiable covariates, which yield regressions with similar explanatory power to those found in the literature. We also consider some alternative methods using extra covariates and methods that are arguably more appropriate when it is recognized explicitly that the PMT is for anti-poverty policy making. Following the practice of other evaluative research, we compare the method to explicit counterfactuals. A natural counterfactual for evaluating any form of PMT is a uniform allocation—a "basic income" transfer that is the same for everyone. Other counterfactuals of interest to policy makers are examined including simpler forms of group-based targeting.

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). Among the World Bank's regional groupings of countries, SSA is both the poorest region by standard measures and the region where existing social spending has been least effective in reaching the poorest.[7] 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).[8] For a subset of these countries we also have panel data. We assess national PMT (calibrated to the full national data set), although we allow for sub-national regional effects and differences between urban and rural areas. If one confines attention to poor areas within a country, then one can expect better performance in reaching poor people.[9] Our focus here is on national programs.

In advocating and evaluating 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.,

[3] 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." A famous early example is found in the targeting method used by Mexico's PROGRESA program (Skoufias et al., 2001). This has been copied by many transfer programs since.

[4] For a critical review of the methods used see Ravallion (2016, Part 2).

[5] Useful overviews can be found in Mills et al. (2015) and USAID's website on Poverty Assessment Tools. Note that USAID does not officially endorse these tools for targeting purposes.

[6] 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).

[7] For evidence on these points see Ravallion (2016, Chapters 7 and 10).

[8] 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. At the time of writing, PMT is also being considered for Mali.

[9] For example, Skoufias et al. (2001) report seemingly high coverage rates for poor people in Mexico's PROGRESA, but their analysis is confined to about the poorest third of all rural localities (using a Census-based "marginality index").

counting someone as poor who is not) and "errors of exclusion" (i.e., counting someone as non-poor who is in fact poor).[10] 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 put greater emphasis on avoiding inclusion errors as a means of cutting the cost to the government without hurting poor people.[11] Some observers have questioned this prioritization, arguing that exclusion errors should get higher weight when the policy objective is to minimize poverty.[12] 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.[13] 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 go further than past work in a number of other respects. We consider alternative econometric methods for calibrating the PMT scores applied consistently across countries. We study these methods recognizing 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 in each country. 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."[14]

Given that there are many methodological options it is also of interest to calculate the score weights for optimally differentiated transfers based on the same information set—the best one could do to reduce poverty with that information. This provides a natural "best-case" benchmark for assessing other (sub-optimal) estimation methods. 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. 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 in implementation.[15] We can expect a degree of churning, with households moving in and out of poverty.[16] 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 generally accept it for the purpose of evaluating the performance of these methods. The only exception is that we use the panel data available to help address concerns about time-varying measurement errors in consumption, recognizing that, to some degree, what are called "targeting errors" are 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 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. There are also relevant issues of data quality that we do not address. For example, there is evidence that short surveys—as used to calculate a PMT score for which the weights were derived from a longer prior survey—can yield non-negligible prediction errors on top of the regression errors from the original survey (Kilic and Sohnesen, 2015).

A further limitation is that, 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.[17] Nor do we consider the (economic, social and political) costs of targeting, which have received some attention in the literature.[18] For example, we do not discuss behavioral responses, social stigmas, or implications for social cohesion and political support for poverty programs.[19]

The paper finds that when the counterfactual is a uniform allocation of the same budget, even with a seemingly modest set of covariates, 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. However, when judged against a fixed poverty line, this success at avoiding leakage to the non-poor 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. Among potential modifications to current econometric targeting practice, we show that poverty-quantile regressions generate the largest improvements in targeting and poverty impact performance. When survey data allow it, focusing the analysis entirely on poor sub-regions can also achieve better results. Nevertheless, the paper finds that econometric targeting typically provides at most modest gains in the poverty impacts over other policy-relevant alternatives. Indeed, 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 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 over what targeting method to use or even whether to target households at all.

The following section describes the PMT method that we assess, while

[10] 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).

[11] 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).

[12] See Cornia and Stewart (1995), Smolensky et al. (1995) and Ravallion (2009).

[13] 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 and Subbarao, 2015 (Malawi).

[14] On the history of these policy options see Ravallion (2016, Part 1).

[15] 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.

[16] The implications of such churning for assessing the performance of social protection policies are examined further in Ravallion et al. (1995).

[17] 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.

[18] See the discussions in van de Walle (1998), Gelbach and Pritchett (2000) and Ravallion (2016, Ch. 10).

[19] 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.

Section 3 describes the measures we use in assessing econometric targeting. Section 4 studies a basic version of the 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 concludes with some recommendations for future practice.

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 here uses household-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}:

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

The PMT score is then based on:

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

The most common method in practice for estimating α_{jt} and β_{jt} is 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 $\widehat{\alpha}_{jt}$ and $\widehat{\beta}_{jt}$ to minimize $\sum \widehat{\varepsilon}_{ijt}^2$ 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 model. 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.[20] 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. That is, we run the regression on poor households only. We extend this method by including households somewhat above the line.

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 are likely to rise with the number of variables. There are also incentive constraints stemming from the scope for manipulation by local agents (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 household head.[21] 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 very fine geographic fixed effects, such as for 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). Thus, one does not know the geographic effect for the population, as required for implementing the PMT.[22] However, we include community-level variables in one version we consider 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. A Statistical Addendum (available from the authors) provides descriptive statistics for the variables included in the PMTs.

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.[23] 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.

Measures of targeting performance: 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[24]:

$$IER_{jt} = \frac{\sum_{i=1}^{N_{jt}} w_{ijt} 1\left(y_{ijt} > z_{jt} \middle| \widehat{y}_{ijt} \le z_{jt}\right)}{\sum_{i=1}^{N_{jt}} w_{ijt} 1\left(\widehat{y}_{ijt} \le z_{jt}\right)} \tag{3}$$

Here the poverty line (in consumption space) is z_{jt} and the sample size is N_{jt} with households indexed $i = 1, \ldots, N_{jt}$ 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. If everyone is deemed "poor," so the transfer payment is

[20] The earliest antecedent we know of is Skoufias et al. (2001), using a median regression. Muller (2005) and Muller and Bibi (2010) also used quantile regressions. This is also one of the methods considered by USAID (2011) for Peru. The method is also discussed in Mills et al. (2015).

[21] See, for example, the various studies in the compilation by Del Ninno and Mills (2015).

[22] The same limitation is shared by small-area estimation methods ("poverty mapping") as in Elbers et al. (2003).

[23] 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.

[24] The indicator function 1(.) takes the value unity when the condition in parentheses is true and zero otherwise.

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—more than any other targeting measure that we are aware of—in comparing the targeting performance of social programs across developing countries.[25] 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.[26]

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 impact on poverty.[27] 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.[28] The Exclusion Error Rate can be written as:

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

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

$$IER_{jt} = \Pr\left(y_{ijt} > z_{jt} \middle| \hat{y}_{ijt} \le z_{jt}\right) = \frac{\Pr\left(y_{ijt} > z_{jt}, \hat{y}_{ijt} \le z_{jt}\right)}{\Pr\left(\hat{y}_{ijt} \le z_{jt}\right)} \tag{5.1}$$

$$EER_{jt} = \Pr\left(\hat{y}_{ijt} > z_{jt} \middle| y_{ijt} \le z_{jt}\right) = \frac{\Pr\left(\hat{y}_{ijt} > z_{jt}, y_{ijt} \le z_{jt}\right)}{\Pr\left(y_{ijt} \le z_{jt}\right)} \tag{5.2}$$

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:

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

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

Also note that $\Pr(\hat{y}_{ijt} \le z_{jt}) = \Pr(y_{ijt} \le z_{jt})$ implies $\Pr(y_{ijt} > z_{jt}, \hat{y}_{ijt} \le z_{jt}) = \Pr(y_{ijt} \le z_{jt}, \hat{y}_{ijt} > z_{jt})$. Then, from (5.1) and (5.2) we see that $IER_{jt} = EER_{jt}$. The error rates are equal when the poverty rates are equal,[29] in which case we will simply refer to the "Targeting Error Rate" (*TER*).

In the methodology of PMT there is the option of either fixing the poverty line or fixing the poverty rate for predicted values according to the survey-based measure using actual consumption. (For example, if the survey indicates that 20% of the population is poor then one targets the poorest 20% based on the PMT scores.) We consider both options.

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 that made to the non-poor.[30] 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 measure more comparable across countries and programs. For a universal basic income, $NTD = 0$. 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. 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).[31] 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[32]:

$$T1_{jt} = \frac{\sum_{i=1}^{N_{jt}} w_{ijt} 1\left(\hat{y}_{ijt} \le z_{jt} \middle| y_{ijt} > z_{jt}\right)}{\sum_{i=1}^{N_{jt}} w_{ijt} 1\left(y_{ijt} > z_{jt}\right)} = \frac{\hat{H}_{jt} - (1 - EER_{jt})H_{jt}}{(1 - H_{jt})} \tag{6}$$

where H_{jt} is the headcount index 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 corresponding headcount index based on the empirical distribution of $\hat{y}_{ijt}$ $(i = 1, \ldots, N_{jt})$.

On the other hand, the Type 2 error rate is $T2_{jt} = EER_{jt}$. This yields an interpretation of the *NTD* as (one minus) the aggregate of Type 1 and 2 errors:

$$NTD_{jt} = 1 - \left(T1_{jt} + T2_{jt}\right) \tag{7}$$

When the poverty rate is fixed the *NTD* is 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:

$$H_{jt} = F_{jt}\left(z_{jt}\right) = \sum_{i=1}^{N_{jt}} w_{ijt} 1\left(y_{ijt} \le z_{jt}\right) \tag{8}$$

While *H* is (by far) the most popular measure in practice, its limitations are widely appreciated, notably that the measure does not reflect changes

25 This normalized share of transfers going to the poor was used by Coady et al. (2004a, b) to compare 85 programs across many countries.

26 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).

27 See Ravallion (2009) who finds supportive evidence using data for a large cash transfer program in China.

28 For example, for the headcount index of poverty one focuses on whether there is exclusion at the poverty line.

29 Intuitively, 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.

30 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).

31 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.

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

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. The poverty gap index can be written as:

$$PG_{jt} = \sum_{y_{ijt} \le z_{jt}} w_{ijt}\left(1 - y_{ijt}/z_{jt}\right) \tag{9}$$

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. The Watts index is known to have a number of other desirable theoretical properties (Zheng, 1993). The index can be written as:

$$W_{jt} = \sum_{y_{ijt} \le z_{jt}} w_{ijt} \ln\left(z_{jt}/y_{ijt}\right) \tag{10}$$

Optimal transfers for a given budget: In policy applications, 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 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 for a given information set. This also identifies a bound to the potential gains from econometric targeting using PMT. By using a budget that eliminates poverty with perfect information, we are also able to quantify the extent to which imperfect information constrains poverty reduction through targeting.

Quite generally one can think of the informationally-feasible transfers as a function of m observed x's. The problem is to choose the parameters of a score for assigning the transfers based on the x's, as given by:

$$\tau_{ijt} = \sum_{k=0}^{m}\left(\gamma_{jt}^{k} x_{ijt}^{k}\right)^{\vartheta} \ge 0 \tag{11}$$

(Here $x_{ijt}^0 = 1$ so that γ_{jt}^0 is the intercept—the transfer received by someone with $x_{ijt}^k = 0$ for $k = 1, \ldots, m$.) In one version we estimate 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.[33] The choice of the score parameters γ_{jt}^k is made to minimize the Watts index in the sample survey data subject to the budget:

$$B_{jt} = \sum_{i=1}^{N_{jt}} w_{ijt}\tau_{ijt} \tag{12}$$

We solve this problem numerically.[34] One start value we use for the optimization is the uniform case obtained by setting $\gamma_{jt}^0 = B_{jt}$ and $\gamma_{jt}^k = 0$ for $k = 1, \ldots, m$. (Other start values are tested.) When there are multiple local optima the solution for γ_{jt}^k that gives the lowest value of the poverty measure is chosen.

Data: The data for implementing these measures come from the World Bank's LSMS.[35] Table 1 lists the countries, years of survey, and numbers of households surveyed. (For the five countries for which the surveys have a panel structure we will also use an earlier round.) In keeping with the bulk of the literature, our dependent variable is log total consumption per capita.[36] Consumption is measured in local currency units. Spatially deflated consumption values are available for all countries except Burkina Faso.[37] 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.[38] 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 $F_{jt}^{-1}(0.4)$).

Table 1
Summary statistics.

Country	Year	N	R^2 for Basic PMT regression	Proportion of population below median income
Burkina Faso	2014	10,378	0.643	0.690
Ethiopia	2013/14	5074	0.319	0.709
Ghana	2009	4617	0.530	0.679
Malawi	2013/14	3952	0.573	0.672
Mali	2014	3240	0.484	0.720
Niger	2011	3859	0.634	0.640
Nigeria	2012/13	3741	0.580	0.647
Tanzania	2012/13	4775	0.585	0.654
Uganda	2011/12	2671	0.494	0.887

Note: All surveys except for Ghana are LSMS-ISA surveys. N is the number of observations used to calibrate the PMT and excludes households with missing weights or missing values for the variables included in the PMT regressions. A list of variables included in the Basic PMT can be found in the Addendum.

4. Results for basic PMT

The "Basic PMT" closely follows prevailing practice. Variables used comprise 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. We also consider a version of PMT in which the regressions are estimated separately, and thus allowed to differ, for urban and rural areas.

The on-line Statistical Addendum gives the OLS regression results for the Basic PMT. Table 1 provides R^2s. The simple average R^2 is 0.54, with a range from 0.32 (for Ethiopia) to 0.64 (Burkina Faso). This explanatory power is comfortably within the range of past studies.[39] We did not try to

[33] Glewwe (1992) recommended this in his formulation of the optimal targeting problem.

[34] We use the "fmincon" program in Matlab.

[35] 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.

[36] 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.

[37] We use nominal consumption for Burkina Faso.

[38] 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).

[39] 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 and Subbarao (2015) ($R^2 = 0.5$ to 0.7). The simple average is 0.52.

Table 2
Targeting errors using Basic PMT.

	Inclusion error rate (*IER*)	Exclusion error rate (*EER*)	Inclusion error rate (*IER*)	Exclusion error rate (*EER*)	Targeting error rate (*TER*)	Targeting error rate (*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	0.414	0.669	0.284	0.316	0.483	0.292
Ethiopia	0.451	0.922	0.365	0.488	0.593	0.387
Ghana	0.323	0.543	0.207	0.349	0.401	0.260
Malawi	0.412	0.777	0.314	0.406	0.522	0.340
Mali	0.432	0.818	0.316	0.331	0.527	0.323
Niger	0.513	0.737	0.339	0.317	0.548	0.345
Nigeria	0.299	0.457	0.222	0.256	0.343	0.237
Tanzania	0.356	0.669	0.296	0.269	0.468	0.288
Uganda	0.403	0.619	0.344	0.280	0.486	0.326
Mean	0.371	0.716	0.306	0.361	0.495	0.320
Using time-mean consumption from panel data						
Ethiopia	0.431	0.949	0.365	0.736	0.604	0.407
Malawi	0.331	0.752	0.313	0.366	0.505	0.332
Nigeria	0.275	0.475	0.208	0.269	0.348	0.231
Tanzania	0.338	0.684	0.273	0.262	0.471	0.272
Uganda	0.357	0.678	0.309	0.270	0.485	0.302
Mean	0.324	0.754	0.286	0.465	0.504	0.326

Note: Targeting errors are calculated using the predicted values from the regressions given in the Addendum. Definitions of the various error rates can be found in the text. Statistics are household weighted.

prune this model, by either *ad hoc* or more systematic methods (such as 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.

OLS results for Basic PMT: We turn now to the targeting measures. Table 2 gives the results. Let us focus first on the fixed poverty line case with $H = 0.2$. Across countries, the rate of inclusion errors implies that 37% of those identified as poor by the Basic PMT method are not in fact poor, i.e., 63% 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 roughly halved the rate of inclusion errors of 0.8 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 72% 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 30% to 51%, and *EER* from 46% to 92%. In Ethiopia, the country with the lowest coverage rate of the poor implied by PMT, almost 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: Ethiopia, for example, has the lowest R^2 in Table 2.

Both inclusion and exclusion errors are lower for $H = 0.4$. Taking a 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 predicted to be 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 21% to 37% and *EER* from 26% to 49%.

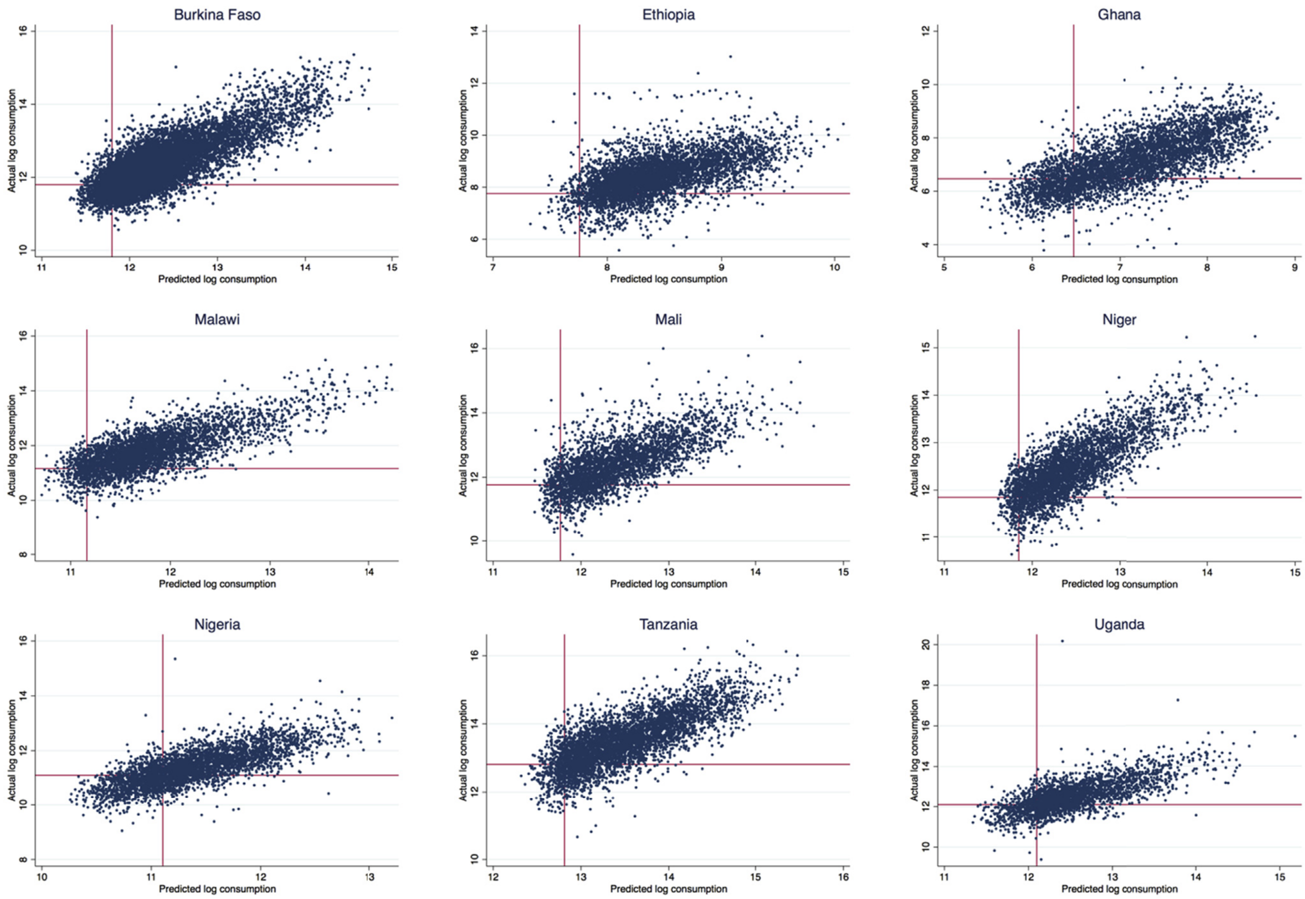
The finding that the errors tend to be higher using the lower poverty line again suggests that econometric targeting may have difficulty in identifying those who are very poor. A further insight on this is found in Fig. 1, which plots actual consumption against predicted consumption by country. The poverty lines at $H = 0.2$ (i.e., $F_{jt}^{-1}(0.2)$) are indicated for each country. 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. It is clear that PMT is missing many of the poorest households in all countries. In Ethiopia, for example, only a very small proportion of points appear in the bottom left quadrant, thus giving very high exclusion error rates. Fig. 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 −0.57 to −0.36, implying that the PMT regressions yield predicted consumptions for the poor between 50% and 100% above their actual consumption.[40] Consistently with this, the PMT scores with a fixed poverty line underestimate the poverty rate; for Basic PMT using $z_{jt} \equiv F_{jt}^{-1}(0.2)$ the mean $\widehat{H}_{jt}$ is 0.090, while for $F_{jt}^{-1}(0.4)$ it is 0.369.

Looking at Figs. 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 (Fig. 2). 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 (Fig. 1).

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 2 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 50%, 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*. This helps lower error rates for countries (such as Ethiopia) that have a small proportion of households predicted as poor.

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

[40] The mean residuals for the poorest 20% by country are −0.36 (Burkina Faso), −0.70 (Ethiopia), −0.57 (Ghana), −0.54 (Malawi), −0.56 (Mali), −0.39 (Niger), −0.34 (Nigeria), −0.53 (Tanzania), and −0.49 (Uganda).



Note: The figure shows actual and predicted household per capita consumption in logged values from regressions using Basic PMT variables. The red lines represent the country 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.

Fig. 1. Actual and predicted log consumption for Basic PMT.

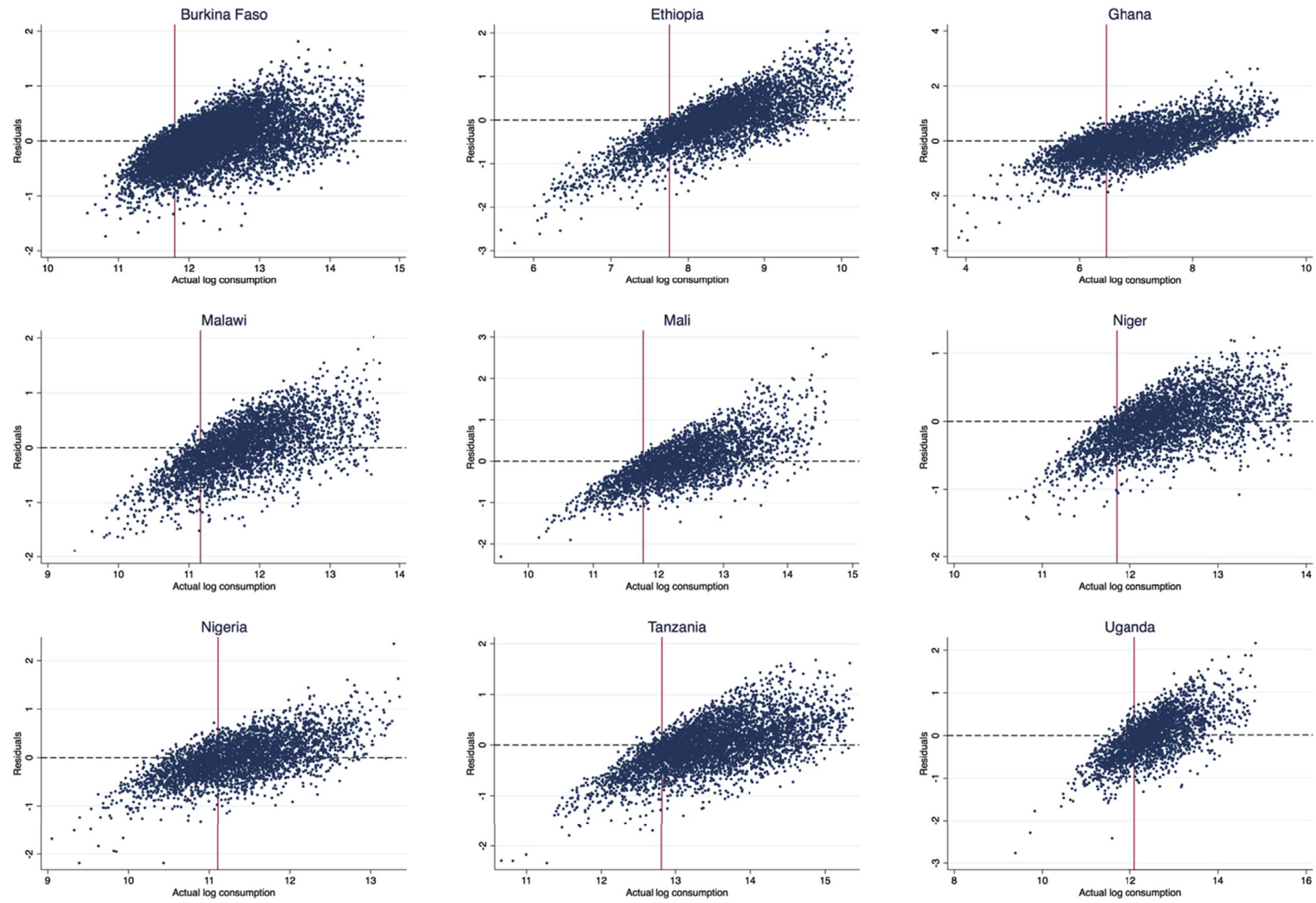
current consumption. We expect that this will help attenuate time-varying measurement errors. The lower panel of Table 2 gives the results. In the majority of cases, targeting performance as measured by inclusion errors improve, while it worsens slightly with respect to exclusion errors when using a fixed line (there is little difference in the TERs). 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.

Results using "poverty-focused" estimation methods: The OLS method used for the Basic PMT chooses the parameter estimates for the PMT scores to minimize the unweighted sum of squared errors, without prioritizing precision for the poor. Here 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 3 gives the analogous results to Table 2. 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 (at the higher line the method does almost as well on the IER while greatly reducing EER). Targeting errors are similar to those for Basic PMT when using a fixed poverty rate instead. For this case, quantile regression offers little improvement.

Table 4 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 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. However, when one uses a fixed poverty rate, PLS does not do as well as OLS.

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. The Addendum provides the inclusion and exclusion errors for this version. We find a decrease in the *IER* relative to Table 4, but with higher *EER* (though still lower than for the OLS). Targeting errors are comparable to those generated by the poverty-quantile regression.

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 scores 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



Note: The figure shows actual household consumption per capita logged and the regression residuals for the predicted consumption values. Basic PMT is used to generate the predicted consumption values. The red line represents the country poverty line set at the 20th percentile.

Fig. 2. Residuals for Basic PMT plotted against log real consumption per capita.

Table 3
Targeting errors for Basic PMT using quantile regression centered at the poverty line.

	Inclusion error rate	Exclusion error rate	Inclusion error rate	Exclusion error rate	Targeting error rate	Targeting error rate
	(*IER*)	(*EER*)	(*IER*)	(*EER*)	(*TER*)	(*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	0.595	0.179	0.329	0.201	0.493	0.292
Ethiopia	0.664	0.233	0.415	0.259	0.589	0.386
Ghana	0.510	0.191	0.270	0.234	0.395	0.256
Malawi	0.613	0.220	0.359	0.243	0.520	0.335
Mali	0.613	0.165	0.356	0.156	0.527	0.313
Niger	0.643	0.211	0.386	0.204	0.555	0.335
Nigeria	0.484	0.139	0.263	0.194	0.334	0.239
Tanzania	0.603	0.151	0.337	0.179	0.474	0.285
Uganda	0.664	0.152	0.394	0.169	0.506	0.318
Mean	0.614	0.189	0.358	0.217	0.496	0.317

Note: Targeting 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 for the first and second two columns respectively is used to estimate the PMT score parameters. Definitions of the various error rates can be found in the text. Statistics are household weighted.

PMT. We found that there is a small 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 0.35 and 0.74 to 0.66, respectively. But again, the gains are small (see the Addendum for full results).

While our focus here is on national-level applications, it should be noted that sub-national applications of these methods to poor areas can also reduce targeting errors, especially exclusion errors. However, we

Table 4
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 rate (*TER*)	Targeting error rate (*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	0.792	0.000	0.564	0.000	0.607	0.331
Ethiopia	0.799	0.002	0.599	0.001	0.684	0.490
Ghana	0.787	0.004	0.585	0.003	0.530	0.305
Malawi	0.794	0.002	0.591	0.000	0.590	0.350
Mali	0.769	0.002	0.562	0.003	0.550	0.346
Niger	0.793	0.000	0.592	0.000	0.591	0.386
Nigeria	0.726	0.012	0.518	0.008	0.442	0.274
Tanzania	0.794	0.001	0.577	0.001	0.676	0.326
Uganda	0.774	0.000	0.563	0.006	0.620	0.383
Mean	0.786	0.003	0.578	0.002	0.613	0.380

Note: Targeting errors are calculated using the predicted values from the regressions given in the Addendum with full weight on the bottom 20th and 40th percentiles for the first and second two columns respectively. Statistics are household weighted.

Table 5
Targeting errors using the Extended PMT.

	Inclusion error rate (*IER*)	Exclusion error rate (*EER*)	Inclusion error rate (*IER*)	Exclusion error rate (*EER*)	Targeting error rate (*TER*)	Targeting error rate (*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	0.336	0.577	0.240	0.269	0.416	0.250
Ethiopia	0.398	0.853	0.367	0.427	0.514	0.381
Ghana	0.331	0.517	0.197	0.323	0.387	0.240
Malawi	0.355	0.639	0.269	0.316	0.451	0.285
Mali	0.377	0.750	0.278	0.258	0.448	0.276
Niger	0.439	0.686	0.325	0.300	0.476	0.323
Nigeria	0.270	0.390	0.185	0.218	0.314	0.199
Tanzania	0.365	0.572	0.268	0.268	0.430	0.268
Uganda	0.392	0.512	0.283	0.252	0.416	0.276
Mean	0.353	0.639	0.284	0.321	0.439	0.296

Note: Targeting errors are calculated using the predicted values from the Extended PMT regressions shown in the Addendum. Statistics are household weighted.

found that the version of the Basic PMT in which the regressions are estimated separately for urban and rural areas produces exclusion and inclusion error rates that are only slightly different from those on the national model; details are found in the Addendum. We also repeated the above analysis for the regions of Nord and Boucle du Mouhoun in Burkina Faso, which are widely agreed to be the country's two poorest administrative regions and for which the national sample includes a sufficiently large number of observations.[41] Then we do much better in terms of exclusion errors: for $H=0.2$, IER is 0.40 and EER is 0.46, as compared to 0.41 and 0.70 respectively for the country as a whole; for $H=0.4$, the IER is 0.23 and EER is 0.15 compared to the national 0.28 and 0.32. This is not surprising: by confining PMT to poor areas one is less likely to exclude poor people. However, survey sample sizes and lack of sub-regional representativeness rarely allow this option for relatively small areas. In addition, PMT-based household-level targeting may do little to improve on geographic targeting, as concluded by Skoufias et al. (2001) in the context of Mexico's PROGRESA which was initially confined to the poorest third of all rural localities (using a Census-based “marginality index”).[42]

[41] We use Burkina Faso as it has the largest national survey size. There are 1678 observations for these regions.

[42] Skoufias et al. (2001, abstract) conclude that: “…the closeness of PROGRESA's performance to what could be achieved by geographic targeting alone raises some serious questions about the costs and benefits associated with the practice of household targeting within poor localities.”

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. R^2 values 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 5). However, the improvement would have to be judged as modest (comparing Tables 5 and 2). For example, 64% of the poorest 20% are still misidentified as non-poor.

We also reran the poverty-focused PMT regressions for the Extended PMT specification.[43] The Addendum gives the targeting errors for the poverty line and poverty rate method when the PMT is estimated using the poverty-quantile regression, as well as the poverty-weighted regressions using poor households and the poor plus the next 20 percent of the distribution. The key findings for Basic PMT using the poverty-focused regressions (Tables 4 and 5) are confirmed using the Extended PMT.

[43] The Addendum gives the regression results when the extended PMT scores are estimated on the bottom 20th, 40th and 60th percentiles.

Table 6
Targeting differentials for various PMT specifications.

	Burkina Faso	Ethiopia	Ghana	Malawi	Mali
Basic PMT covariates					
Basic PMT	0.272	0.062	0.403	0.184	0.148
Using means from panel data	n.a.	0.041	n.a.	0.217	n.a.
Poverty quantile regression	0.520	0.388	0.599	0.470	0.504
Poverty weighted: Poor only	0.049	0.006	0.076	0.038	0.166
Poverty weighted: Poor + 20	0.478	0.259	0.582	0.427	0.468
PMT: separate Urban/ Rural models	0.288	0.098	0.412	0.185	0.150
Extended PMT covariates					
Extended PMT	0.370	0.123	0.423	0.311	0.212
Using means from panel data	n.a.	0.086	n.a.	0.307	n.a.
Poverty quantile regression	0.576	0.408	0.631	0.538	0.564
Poverty weighted: Poor only	0.212	0.026	0.198	0.151	0.362
Poverty weighted: Poor + 20	0.563	0.336	0.590	0.496	0.511
Stepwise (p = 0.01)	0.259	0.075	0.423	0.323	0.237
HH shocks + food security	0.374	0.148	n.a.	0.359	n.a.
Shocks, food security + community variables	n.a.	0.118	n.a.	0.319	n.a.

	Niger	Nigeria	Tanzania	Uganda	Mean
Basic PMT covariates					
Basic PMT	0.193	0.484	0.285	0.316	0.261
Using means from panel data	n.a.	0.475	0.275	0.278	0.257
Poverty quantile regression	0.430	0.659	0.527	0.426	0.503
Poverty weighted: Poor only	0.036	0.332	0.038	0.138	0.098
Poverty weighted: Poor + 20	0.407	0.635	0.489	0.426	0.464
PMT: separate Urban/ Rural models	0.181	0.511	0.283	0.298	0.267
Extended PMT covariates					
Extended PMT	0.252	0.554	0.367	0.409	0.336
Using means from panel data	n.a.	0.524	0.383	0.408	0.342
Poverty quantile regression	0.437	0.709	0.571	0.534	0.552
Poverty weighted: Poor only	0.148	0.486	0.125	0.193	0.211
Poverty weighted: Poor + 20	0.413	0.687	0.545	0.485	0.514
Stepwise (p = 0.01)	0.218	0.480	0.350	0.344	0.301
HH shocks + food security	0.275	0.546	0.367	0.439	0.358
Shocks, food security + community variables	0.274	0.555	0.358	0.434	0.343

Note: The targeting differential is computed using the poverty line set at the 20th percentile. A definition of how the differential is calculated can be found in the main text. Means from panel data refer to the differential calculated using a PMT run on the average of two survey rounds. Statistics are household weighted.

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, using a cut-off point of $p = 0.01$. We see a modest increase in the targeting errors, which are now back to approximately the same values we found for Basic PMT (see Addendum for full results).

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 specification with these food security and shock variables.[44] As expected, the R^2 increases slightly for all countries. 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 well as the targeting error rate) 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 6 gives summary statistics on the normalized targeting differential using both the Basic and Extended PMTs. The cross-country mean *NTD* for Basic PMT is 0.261, meaning that if program participation was based on the PMT scores the participation rate for the poor would be 26% points higher than that for the non-poor. Using separate urban/rural models achieves a slightly higher *NTD* in 6 out of 9 countries. An Extended PMT does slightly better, yielding an average *NTD* of 0.336 across the nine countries. Using time-mean consumption does little to improve the *NTD* on average. Returning to the cross-sectional surveys, the poverty-focused methods yield the highest *NTDs*, with the poverty-quantile method giving the highest *NTD* at 0.502 on average for the Basic PMT and 0.552 for the Extended PMT. Indeed, the poverty-quantile regression method comes out best for all nine countries. The poverty-weighted method does almost as well, provided that the 20% of households above the line are included.[45]

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. 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 $H = 0.2$. 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 by the PMT, 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

[44] The Addendum lists the variables, their means and the regression results for this specification.

[45] The poverty-weighted method that just includes households below the poverty line does particularly poorly: while it includes almost all poor households, it also includes too many non-poor households (recall that the inclusion errors for this method were almost 80%).

Table 7
Headcount index of poverty post transfer.

	Burkina Faso	Ethiopia	Ghana	Malawi	Mali
Universal (basic income)	0.165	0.168	0.152	0.170	0.159
Basic PMT covariates					
Basic PMT	0.151	0.184	0.156	0.164	0.164
Using means from panel data	n.a.	0.190	n.a.	0.156	n.a.
Poverty quantile regression	0.153	0.156	0.145	0.154	0.141
Poverty weighted: Poor only	0.164	0.168	0.151	0.171	0.157
Poverty weighted: Poor + 20	0.156	0.162	0.143	0.163	0.149
PMT: separate Urban/Rural models	0.152	0.176	0.155	0.165	0.162
Extended PMT covariates					
Extended PMT	0.146	0.171	0.154	0.152	0.151
Using means from panel data	n.a.	0.180	n.a.	0.149	n.a.
Poverty quantile regression	0.151	0.152	0.142	0.149	0.138
Poverty weighted: Poor only	0.161	0.168	0.151	0.166	0.147
Poverty weighted: Poor + 20	0.148	0.154	0.142	0.155	0.146
Stepwise ($p = 0.01$)	0.165	0.181	0.154	0.150	0.161
HH shocks + food security	0.146	0.166	n.a.	0.148	n.a.
Shocks, food security + community variables	n.a.	0.172	n.a.	0.151	n.a.
Categorical targeting					
Elderly 65+	0.168	0.174	0.160	0.176	0.167
Widowed or disabled	0.169	0.175	0.155	0.173	0.173
Elderly, widows & disabled	0.165	0.170	0.152	0.174	0.168
Children under 14 (max 3)	0.160	0.167	0.148	0.167	0.155
Elderly, widows, disabled & children	0.160	0.162	0.150	0.168	0.158
Female heads with children	0.179	0.178	0.161	0.165	0.197
Drought, flood or livestock death	0.159	0.174	n.a.	0.166	n.a.

	Niger	Nigeria	Tanzania	Uganda	Mean
Universal (basic income)	0.177	0.144	0.175	0.161	0.164
Basic PMT covariates					
Basic PMT	0.159	0.146	0.153	0.147	0.163
Using means from panel data	n.a.	0.147	0.150	0.151	0.166
Poverty quantile regression	0.157	0.133	0.163	0.151	0.152
Poverty weighted: Poor only	0.175	0.140	0.175	0.160	0.163
Poverty weighted: Poor + 20	0.155	0.134	0.164	0.155	0.155
PMT: separate Urban/Rural models	0.161	0.142	0.153	0.151	0.160
Extended PMT covariates					
Extended PMT	0.159	0.140	0.149	0.145	0.156
Using means from panel data	n.a.	0.146	0.145	0.143	0.159
Poverty quantile regression	0.159	0.126	0.160	0.145	0.148
Poverty weighted: Poor only	0.173	0.133	0.173	0.158	0.161
Poverty weighted: Poor + 20	0.158	0.127	0.160	0.151	0.150
Stepwise ($p = 0.01$)	0.161	0.150	0.154	0.158	0.163
HH shocks + food security	0.156	0.141	0.150	0.141	0.154
Shocks, food security + community variables	0.156	0.141	0.150	0.142	0.156
Categorical Targeting					
Elderly 65+	0.180	0.152	0.176	0.162	0.169
Widowed or disabled	0.185	0.159	0.176	0.165	0.170
Elderly, widows & disabled	0.179	0.143	0.174	0.157	0.164
Children under 14 (max 3)	0.172	0.142	0.172	0.150	0.161
Elderly, widows, disabled & children	0.172	0.129	0.173	0.150	0.158
Female heads with children	0.185	0.182	0.167	0.153	0.172
Drought, flood or livestock death	0.169	0.171	0.175	0.168	0.176

Note: Eligible households receive uniform per capita transfers. The total transfer amount for each country is equal to each country's aggregate poverty gap. The poverty line is used to determine whether a household is eligible. The statistics in the table give the country's headcount index following the transfer. The starting value of the headcount index is 0.2. Universal is a transfer to all households adjusted for household size. All households with PMT-predicted consumption below the poverty line receive a transfer of equal size. Descriptions of the various PMTs can be found in the text. Categorical targeting gives transfers to each household member who meets the specified category. For example, 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 child category is capped at 3. In the final category, a household receives a transfer if it has received one or more household-level shocks. Statistics are household weighted.

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.[46] The first category is the set of persons 65 years or older. The second is any person who is a (female) widow, disabled (where disability 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

[46] 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.

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 under the age of 14 are each allotted a transfer. We next combine all previous schemes, where children, the elderly, widowed, disabled or orphaned are eligible. Finally, we consider households headed by females with children, and households who have received some type of shock, namely drought, flood or unexpected death of livestock. (Recall that all stylized schemes considered have the same aggregate budget.)

Table 7 shows the implied headcount index for each case (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. Basic PMT does as well as the universal transfer on average, and does worse in almost half of the countries considered. Using separate urban/rural models or the time-mean consumptions for the countries with panel data makes little difference on average. The poverty quantile regression method does noticeably better across countries. This method brings the poverty rate down close to two percentage points below the level attainable with the Basic and Extended PMTs, reflecting the method's lower exclusion errors.[47] In general, the Extended PMT models do slightly better. However, it is notable how well categorical targeting does in many cases. On average, targeting to households with elderly or disabled members, widows or children does as well as Basic PMT. While categorical targeting does not have quite as much impact on poverty as the Basic PMT, it clearly comes close and is likely to be administratively simpler and far more transparent.

The Addendum gives the corresponding results for the poverty gap index and Watts index. Aggregating across countries, the Basic PMT methods reduce the poverty gap and the Watts index by around 29%. As for the headcount index, the Extended PMT gives a larger reduction, namely 35% and 38% 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.

7. 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—PMT works poorly in predicting the levels of living of the poorest.

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 calculate the optimal allocation based on the set of 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 8
Watts index post transfer using differentiated transfers.

	Actual	PMT		Optimal transfers	
		Uniform	Predicted Gap	Linear	Non-linear
Burkina Faso	0.048	0.036	0.037	0.034	0.034
Ethiopia	0.074	0.070	0.071	0.054	0.053
Ghana	0.091	0.055	0.057	0.051	0.051
Malawi	0.063	0.048	0.050	0.046	0.045
Mali	0.066	0.054	0.055	0.048	0.047
Niger	0.050	0.043	0.044	0.040	0.039
Nigeria	0.073	0.045	0.046	0.039	0.038
Tanzania	0.077	0.058	0.060	0.055	0.053
Uganda	0.080	0.057	0.058	0.049	0.048
Mean	0.074	0.057	0.059	0.049	0.048

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

Table 8 gives the results for the Watts index for $H = 0.2$ using differentiated transfers that are determined by both the PMT gaps and optimization. Overall, filling the predicted gaps based on the PMT scores reduces the Watts index by less than uniform transfers (as determined by the PMT). This reflects weaknesses in predicting the living standards of the poorest. As expected, the non-linear specification in the optimization routine ($\vartheta = 2$) does better than the linear one in reducing poverty, and both versions do better than PMT (for both uniform and differentiated transfers) at filling the poverty gaps. But the severity of the information constraint is evident: even the optimal nonlinear allocation only reduces the Watts index by about 40% despite the fact that the budget is sufficient to eliminate poverty with perfect information.

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: 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.[48] If anything, our lags appear to be less than found in practice.[49]

We consider two types of lags. In the first (Method 1), we take the regression parameters from Round 1, but use the covariates from Round 2. 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).

[47] Note, however, that higher inclusion errors place a limit on the quantile regression's poverty impacts.

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

[49] 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).

Table 9
Targeting errors allowing for lags in implementation.

	Inclusion errorrate (IER)	Exclusion error rate (EER)	Inclusion error rate (IER)	Exclusion error rate (EER)	Targeting error rate (TER)	Targeting error rate (TER)
	Fixed poverty line				Fixed poverty rate	
	$z = F^{-1}$ (0.2)		$z = F^{-1}$ (0.4)		$H = 0.2$	$H = 0.4$
Basic PMT						
Ethiopia	0.512	0.995	0.297	0.894	0.678	0.450
Malawi	0.652	0.222	0.476	0.083	0.557	0.352
Nigeria	0.225	0.826	0.152	0.500	0.412	0.273
Tanzania	0.423	0.723	0.296	0.296	0.470	0.296
Uganda	0.418	0.628	0.389	0.362	0.523	0.393
Mean	0.507	0.801	0.325	0.565	0.554	0.367
Extended PMT						
Ethiopia	0.453	0.991	0.310	0.842	0.614	0.438
Malawi	0.626	0.170	0.454	0.059	0.467	0.305
Nigeria	0.147	0.828	0.122	0.585	0.405	0.255
Tanzania	0.354	0.670	0.263	0.320	0.447	0.272
Uganda	0.453	0.465	0.381	0.289	0.451	0.336
Mean	0.476	0.770	0.312	0.558	0.508	0.344

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. These calculations are for Method 1 described in the main text. Results for Method 2 are found in the Addendum. Statistics are household weighted.

Table 10
Headcount index post transfer allowing for lags.

	Ethiopia	Malawi	Nigeria	Tanzania	Uganda	Mean
Universal (basic income)	0.165	0.170	0.142	0.174	0.160	0.162
Basic PMT covariates						
Basic PMT	0.183	0.165	0.147	0.153	0.153	0.165
Extended PMT	0.171	0.153	0.143	0.150	0.143	0.157
Method 1 Basic PMT	0.199	0.154	0.166	0.157	0.146	0.175
Method 1 Extended PMT	0.199	0.150	0.166	0.149	0.140	0.172
Method 2 Basic PMT	0.198	0.158	0.169	0.155	0.158	0.176
Method 2 Extended PMT	0.196	0.155	0.161	0.150	0.154	0.172
Categorical Targeting						
Elderly 65+	0.177	0.177	0.146	0.176	0.159	0.169
Widowed or disabled	0.177	0.173	0.152	0.175	0.167	0.170
Elderly, widows & disabled	0.175	0.173	0.139	0.174	0.155	0.165
Children under 14	0.164	0.166	0.147	0.170	0.155	0.161
Elderly, widows, disabled & children	0.158	0.168	0.126	0.169	0.153	0.154
Female heads with children	0.180	0.165	0.181	0.167	0.154	0.174

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 household per capita consumption. The Basic and Extended PMT methods use Round 2 data only (i.e. no lags). Universal refers to a transfer to all households weighted by household size. Categorical targeting categories are defined in Table 9. Only panel households are included; hence the results without lags differ slightly from Table 7 which is based on the full sample for Round 2. Statistics are household weighted.

The targeting errors obtained using Method 1 are found in Table 9. Comparing the results in Table 9 (top panel) with Table 2 we see that allowing for lags increases the targeting errors on average.[50] 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 0.507, as compared to 0.371 from Table 2).[51] Exclusion errors are also affected, rising from 0.716 to 0.801, and the targeting error rate is higher for all countries. The Extended PMT also results in a substantial increase in targeting errors, especially for inclusion, when we allow for lags using Method 1. A similar pattern is found for Method 2; details are given in the Addendum. Allowing for lags, we find a substantial decrease in the *NTD* on average (see Addendum).

The post-transfer poverty rates allowing for lags are provided in Table 10. PMT still brings the poverty measures down, but by about one percentage point less when allowing for lags by either Method 1 or 2.[52] For example, allowing for lags achieves an average post-transfer headcount index of 18% instead of 17%. The stylized categorical targeting schemes now attain similar or somewhat lower post-transfer poverty rates. On average, the simple demographic scorecards do as well. The Addendum gives results for other poverty measures, which follow a similar pattern.

9. Conclusions

Highly imperfect information and limited administrative capabilities create challenges for implementing effective antipoverty programs in

[50] Note that Table 2 uses the full sample while Table 9 uses the panel subsample. The targeting measures are slightly different between the two samples but the following observations still hold.

[51] The proportion of households predicted to be poor differs between the two methods. For example, more households are predicted to be poor for Malawi under Method 1 than for both Method 2 and when no lags are introduced (the opposite is true for Nigeria). Malawi therefore performs better with the lag. The targeting error rate addresses this issue. Comparisons with Table 2 show that the TER is worse for all countries when lags are introduced.

[52] Recall that the sample here is just based on panel households, and the Basic and Extended PMT results will differ slightly from those in Table 7.

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. However, if poverty-reduction 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 by reducing exclusion errors, we have shown that better methods can be proposed, which give higher weight to performance in predicting the living standards of poor people. The econometric targeting method that performs best from the point of view of reducing exclusion errors and reducing poverty is a "poverty-quantile regression." This generates more inclusion errors than prevailing PMT methods, though still less than un-targeted transfers.

Five main recommendations for future practice emerge from this study. First, it makes more sense to fix the poverty rate rather than the poverty line when using these methods, especially for OLS which (as we have shown) overestimates the living standards of the poorest. Second, in keeping with others in the literature, we recommend against the current focus on inclusion errors of targeting, toward a focus on exclusion errors and (most importantly) impacts on poverty. Third, poverty-quantile regressions should replace OLS in future applications.

A fourth, somewhat, larger recommendation is to remain open as to whether econometric targeting using PMT is the best option. Looking at our findings as a whole, these econometric methods do not appear to perform well when one is striving to reduce poverty. Starting from a situation in which 20% of the population is deemed to be poor, when the budget required for a set of transfer payments that would eliminate this poverty is allocated by any of these methods, about three-quarters of the original (pre-intervention) count of poor people remain poor. The most widely-used form of PMT in practice does only slightly better on average than an untargeted universal basic income scheme, in which everyone gets the same transfer, whatever their characteristics. 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. The reason is that we find that the objective function of minimizing a poverty index is fairly flat with respect to the PMT score weights over a broad region, so the gains from better methods are never large. Indeed, we find that an especially simple demographic "scorecard" method can do almost as well as econometric targeting in terms of the impacts on poverty. And when we allow 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.

Our final recommendation to practitioners is to do ex ante evaluations of any proposed targeting method. In particular, before deciding how, or whether, to finely target social spending, practitioners should do the types of calculations we have done here, in the specific setting.

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