THE ECONOMICS OF HUMAN DEVELOPMENT: SCHOOLING, NUTRITION, AND SOCIAL PROTECTION

A Dissertation
submitted to the Faculty of the
Graduate School of Arts and Sciences
of Georgetown University
in partial fulfillment of the requirements for the
degree of
Doctor of Philosophy
in Economics

By

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Washington, DC
May 2, 2017

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ABSTRACT

While there have been large gains in poverty reduction globally, there is evidence to suggest that the poorest are being left behind. This dissertation aims to further our knowledge of the economics of human development, focusing specifically on issues related to schooling, nutrition, and social protection. The first chapter investigates whether social effects matter for school enrollment decisions in rural India. Caste-based peer groups are found to significantly influence individual enrollment decisions. The second chapter quantifies the proportion of undernourished women and children who live in non-poor households in Sub-Saharan Africa, and finds that the majority of these individuals do not live in poor households. Rather, undernourished women and children are spread quite widely across the distribution of household wealth and consumption. The final chapter tests existing and new methodology that can be used to accurately target poor households in settings where reliable indicators of welfare are unavailable. Even with a budget sufficient to eliminate poverty with full information, none of the targeting methods considered brings the poverty rate below about three-quarters of its initial value. There is still much work to be done to ensure that anti-poverty policy reaches both poor households and poor individuals.

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Chapter 1: Social Effects on Schooling in Rural India[1]

1.1 Introduction

Policy efforts to encourage school enrollment have often focused on incentives at the household level. However, evidence from developed countries suggests an individual's neighborhood and social circle play an important role in educational decisions (Case and Katz, 1991; Leventhal and Brooks-Gunn, 2000; Sacerdote, 2001; Zimmerman, 2003; Kling et al., 2005). If enrollment is influenced by a child's neighbors and friends, policies that utilize this effect may be more successful at increasing enrollment than policies directed solely at the household. Very little is known about social effects and schooling in developing countries.

This paper investigates whether social effects matter for the school enrollment decisions of all school-aged children in rural India. Two types of social effects are considered. The first is a role model effect, which considers specifically the effect of same-caste women's education on individual child enrollment.[2] The second is a peer effect, where a child's enrollment may be influenced by other children's enrollment decisions. The effect of peers and role models on time spent on activities such as school work, farm and non-farm employment, and household chores is also considered.

There are several issues for identifying the causal nature of social effects. Firstly, individuals choose their peer groups, making it difficult to separate the peer effect from a selection effect. Peer

[1] Many thanks to Martin Ravallion, Garance Genicot, Dominique van de Walle, Frank Vella, Arthur Alik Lagrange, Allison Stashko, Kersten Stamm, Alev Gurbuz, Madhulika Khanna, Dario Sansone, and seminar participants at Georgetown University, the Washington Area Development Economics Symposium, the Southern Economic Association Annual meetings, Bates College and American University for their valuable comments and suggestions.

[2] Existing literature has found that women strongly influence child educational outcomes and girls' career aspirations; for example, Beaman et al. (2012) in the context of female political participation.

groups in this paper are defined within village and by caste. Caste in rural India is hereditary and inextricably linked to social status, influencing where households live and what occupations they hold, as well as regulating social interactions and marriage (Deshpande, 2001; Jodhka, 2002; Munshi and Rosenzweig, 2006; Desai and Dubey, 2012). Caste therefore provides a natural and arguably exogenous definition of a child's likely influences.

A second issue is that there may be unobservables affecting both individual and peer enrollment. The inclusion of village fixed effects accounts for village level factors (either observable or unobservable) which could impact enrollment decisions, such as school quality, government (federal, state, or local) policies, village preferences for education, the enrollment rates of other castes, and any selection in migration.[3]

A final issue is the reverse causality of the peer measure, known as the 'reflection problem' (Manski, 1993). The estimates of both the peer effect and the role model effect in any regression that includes the peer enrollment variable will be biased and inconsistent. An instrumental variables approach using the average education of the peer group maternal grandmothers is used to overcome this problem. The exclusion restriction is based on the prevalence of marriage migration among women. Maternal grandmothers are unlikely to live in the same village as their grandchildren, and therefore will not directly influence enrollment decisions of other children in the village. Maternal grandmother education will also be uncorrelated with out of sample peers and role models who fall into the error term.[4]

[3] The incidence of rural-to-rural household-level migration in India is relatively low (Munshi and Rosenzweig, 2009, 2016). The rural-to-urban migration rate is also low compared to other comparable developing countries such as Brazil. In 2005 the rate was estimated to be between 5 and 7% (Munshi and Rosenzweig, 2016).

[4] Village fixed effects additionally account for any selection in the migration process among women. Household- and individual-level controls such as mother, father, and maternal grandmother education are also included.

The findings indicate a minimal role model effect for school enrollment decisions. This holds among all children, and when the effect is broken down by caste, gender, and age group. Peer enrollment, on the other hand, significantly influences child enrollment: a one percentage point increase in the enrollment rate of a child's peer group is shown to increase the probability that a child enrolls in school by 0.58 percentage points. The peer effect varies in magnitude between caste, age group and gender. Children of lower castes are more influenced by their peers than children of other castes, as are girls relative to boys. The peer effect is also larger for younger children in comparison to older children.

If children are enrolling in school in response to changes in their peers' enrollment, it is of interest to see whether they also spend more time studying, and consequently, what other activity these children are substituting away from. Both peers and role models are found to significantly affect how much time a child spends studying. In addition, a significant negative role model effect is found for time spent on farm and non-farm employment and housework.

The paper contributes to the literature on social effects in educational settings. There is a large and growing literature on peer effects in education in developed countries; see Durlauf (2004); Epple and Romano (2011) and Sacerdote (2011) for reviews. However, there are few papers that consider social effects in education in a developing country context, which may differ from the effects found in developed countries.[5] Bobonis and Finan (2009) and Lalive and Cattaneo (2009) are the papers closest to this one, and exploit the random assignment of the Mexican conditional-cash transfer program PROGRESA to identify peer effects in secondary school enrollment decisions. The authors find that the

[5] Nguyen (2008) finds a larger improvement in poor children's test scores in Madagascar when they are exposed to a role model from a similar background than when the role model is non-poor. Kremer et al. (2009) investigates how merit scholarships awarded to primary school girls in Kenya impact test scores of other students, and find positive externalities among non-recipients of both genders.

increase in enrollment among children eligible for the program significantly increases enrollment among program-ineligible children. Results presented in this paper are consistent with their findings.

This paper extends upon existing work in several key ways. Firstly, it considers jointly the effects of both role models and peers in school enrollment decisions, as well as exploring important heterogeneity in these effects by caste, gender, and age group. Given that enrollment is still a concern in rural India, understanding what motivates children to enroll in school is essential to improving educational outcomes. The impact of women outside the household for child educational outcomes is considered, building on work by Beaman et al. (2012) that looks specifically at the effect of female political representation in India on girls' aspirations. Finally, the paper highlights the importance of caste-based social networks in educational outcomes within the Indian village. In particular, it expands on work by Helmers and Patnam (2014), who show neighborhood peers have a significant impact on child cognitive development.

The findings presented have important policy implications. Peer effects imply the existence of a social multiplier, which will magnify the impacts of a program: policies are likely to have a much greater impact than the individual effects suggest. Furthermore, programs to boost school enrollment targeted across caste groups can have a greater effect than initiatives that are more finely targeted. For example, a program aimed at achieving universal education may choose to focus its efforts on Scheduled Caste/Scheduled Tribe (SC/ST) children, given the low rates of enrollment among the SC/ST children. However, spreading program benefits across caste groups and using the social multiplier to magnify impacts among non-recipient children within castes may lead to better outcomes – the program will increase enrollment not only for SC/ST children but for both recipient and non-recipient children of other caste groups. Additionally, given that the peer effect among SC/ST children is found to be larger than for children of other castes, enrollment rates among SC/ST children may in fact 'catch up' to the enrollment rates of higher caste groups.

The rest of the paper is organized as follows: Section 2 provides a brief discussion of the data, educational policy in India, and the role that caste plays in social interactions. Section 3 outlines the model and estimation strategy used to identify the two social effects. Section 4 presents the key findings, including whether social effects matter for enrollment and whether peer enrollment affects time spent on various activities such as study and work. The paper is concluded in Section 5.

1.2 Data

The data used for this study comes from the 1998-99 and 2005-06 (henceforth referred to as 1999 and 2006) rounds of the Rural Economic and Demographic Survey (REDS).[6] The survey is designed to be representative of rural India, though it excludes some areas that were experiencing significant conflict or unrest at the time the survey was conducted.[7] Data are drawn from the household and village surveys. The household survey was administered to 7,474 and 8,658 households in the 1999 and 2006 rounds respectively across 250 villages; however, not all households have school-aged children and others have missing values for key variables. The household survey includes level of education and current enrollment status for each household member, along with demographic information such as age, gender, and relationship to the head of household. It also contains variables on the household's religion, caste, and consumption. The village survey provides data on village population, location, existing facilities, and infrastructure, as well as the availability and quality of

[6] REDS is conducted by the National Council for Applied Economic Research (NCAER). The first round of this survey, known as ARIS-REDS, was collected in 1970-71. Another round was collected in 1981-82. These rounds are not included as key variables needed for the analysis are not available.

[7] Assam is omitted from the 2006 rounds due to conflict in the region. Jammu and Kashmir is similarly omitted from all rounds.

schools within the village.

The REDS is designed as a panel, tracking the same households over time.[8] Additional randomly sampled and split off households are also added in each round, yielding the larger sample size in 2006.[9] For this paper, the 1999 and 2006 rounds are pooled together with a common village identifier. That is, the data can be thought of as a panel at the village level, though there are some villages that appear in only one of the two rounds either because they were added or not re-surveyed in 2006.[10] 253 villages from 16 states in 1999 and 241 villages from 17 states in 2006 are included in the final sample, with an average of 19 and 26 households per village in 1999 and 2006 respectively.[11]

1.3 School enrollment and educational policy

This paper looks at the enrollment decisions of school-aged children, where a school-aged child is defined as between 5 and 16 years of age at the time of the survey.[12] The enrollment variable constructed is a binary indicator that takes on a value of one if the child is reported as currently enrolled in school or if student is listed as their primary activity and zero otherwise.[13] Table 1 shows the proportion of children enrolled in school by state. On average, the proportion of children enrolled in school has increased by 10 percentage points over the seven-year period considered. While southern states such as Kerala, Tamil Nadu, and Andhra Pradesh have higher average enrollment rates than the traditionally poorer states in the northern part of the country (for example, Bihar and Uttar Pradesh), the

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[8] The sampling for this survey was done in 1970-71. As such, subsequent rounds may not accurately represent rural India.

[9] Split off households are new households that originated from an existing household in the previous round. Less than 5 percent of the households in the 2006 round are split off households. There is also some attrition, with approximately 20 percent of households form the 1999 round not appearing in the subsequent 2006 round.

[10] 81 children are from villages that were not re-surveyed in 1999. 402 children are from villages that were added in the 2006 round. 12 new villages in total from 8 different states were added.

[11] The 2006 round includes new states Jharkhand and Chattisgarh, which were formed in 2000 from parts of Bihar and Madhya Pradesh respectively.

[12] Results are also robust to different definitions of a school-aged child, for example 6 to 16 years or 5 to 18 years.

[13] 27 children had missing values for this variable and were dropped from the final sample.

gap has narrowed over time. Nonetheless, more than 20 percent of school-aged children in 2006 remained unenrolled in school, with several states showing only slight increases, and even decreases, in average enrollment between 1999 and 2006.

The increase in average enrollment is in part due to large investments in education by the Indian government over the past three decades. The 2011 Public Report on Basic Education (PROBE) report found that one out of every four government schools surveyed in 2006 had been set up in the last ten years, and the majority of schools surveyed had more than two rooms, drinking water facilities, and toilets. Following the 1986 National Policy on Education (NPE), the government introduced a number of different initiatives aimed at improving educational access and quality throughout India. Sarva Shiksha Abhiyan (SSA), or "Education for All Movement," is the largest program. Its goal is universal primary education for all children 6 to 14 years of age and includes improved infrastructure and teacher training, as well as free educational materials for girls and lower caste children (Kainth, 2006). The Mid-Day Meal (MDM) scheme, the largest school meal program in the world, provides each child in every government primary school with a mid-day meal each day of school (Kingdon, 2007).

Non-government organizations (NGOs) are also active in India, providing more micro-level funding for educational projects aimed at improving educational quality. Many of these projects are small- scale and are targeted at the school or household level; see, for example, projects evaluated by Banerjee et al. (2007); Muralidharan and Sundaraman (2011); Duflo et al. (2012) and Muralidharan and Prakash (2013).[14] Several field experiments using a CCT intervention aimed at encouraging enrollment at the household level have also been implemented in specific regions (Sinha and Yoong, 2009; Berry, 2015).

[14] Asim et al. (2015) provides a review of the literature on impact evaluations in South Asia.

While existing programs that encourage enrollment have reported some success, the existence of social effects can lead to additional impacts: Bobonis and Finan (2009) and Lalive and Cattaneo (2009), for example, show that the conditional cash transfer program PROGRESA in Mexico increases school enrollment not only among children in eligible households, but also among children in non-eligible households. Furthermore, the presence of peer effects implies that there is a social multiplier - an increase in school enrollment encourages unenrolled peers to enroll, which encourages unenrolled peers of the peers to enroll. The aggregate effect of a program may therefore be much larger than sum of the individual effects (Glaeser et al., 2003).

Social effects also have implications for how educational programs are targeted. If the aim of a program is to increase overall enrollment within a village, then the targeting method should ensure that children in each social group (in this context each caste group) receive the program, enabling the benefits to spillover to other, non-recipient children. For example, a program that targets all SC/ST children may have a smaller overall impact on enrollment rates than a program that targets children across caste groups and allows the social multiplier to distribute the benefits among children who did not receive the program.

1.4 Women as role models

The focus on female role models stems from existing research that finds mother's education has a larger effect on child's educational attainments than the education of the father (Hill and King, 1995; Thomas et al., 1996; Dauber et al., 1996; Ermisch and Francesconi, 2001; Schultz, 2002). Within India, children of literate mothers are found to spend more hours per day studying, while father's schooling has little impact (Behrman et al., 1999). This influence could potentially spillover to other children within the household's social circle, either directly through the child or indirectly through the child's mother.

For example, an educated mother might encourage other mothers to enroll their children in school, or teach children other their own the value of education. Children, and particularly girls, may look up to their friends' mothers.

Women who do not have young children can also serve as role models: a child may be influenced by older sisters of her peers, or a woman who is in a position of power. Indeed, there is evidence that women can serve as role models for young women, particularly when it comes to career choice (Smith and Erb, 1986; Nauta et al., 1998; Campbell and Wolbrecht, 2006; Lockwood, 2006; Quimby and Santis, 2006). Within India, Beaman et al. (2012) find that female leadership significantly influences adolescent girls' career aspirations and improves their educational outcomes. Exposure to female leaders has been found to improve the chances that women will both stand for and win elections in the future, and improve perceptions of female effectiveness among men (Beaman et al., 2009).

The role model effect may also operate through women's influence on the distribution of public goods: if women in power are able to improve access to education for children within their caste, this may increase enrollment among these children.[15] Chattopadhyay and Duflo (2004), for example, found that female political representation leads to greater investments in drinking water, while Clots-Figueras (2011) shows that female politicians invest more in health and early education, and favor laws friendly to women.[16]

1.5 Caste and social effects

For the purpose of this paper, both role models and peer groups are defined within village by

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[15] Note, however, that this influence needs to vary within village. If women influence access to education for all children within a village, this will be absorbed by the village fixed effects.

[16] Nonetheless, in the patriarchal setting of rural India it may be a child's father or the head of the household (who is typically male) that makes the enrollment decisions, and boys in particular are likely to look to male role models rather than female ones. Section 4 therefore considers the impacts of the role model effect both for all children and separately by gender.

caste group. Caste in this case refers to the household's *jaati*, which are categorized into three primary groups: Scheduled Caste/Scheduled Tribe (SC/ST); Other Backwards Caste (OBC); and General Castes, which includes Brahmin, Other Upper and Non-class children.[17] An additional category for Muslim children is also created, giving a total of four caste-based peer groups and role model pools.[18] For example, an SC/ST child's role models are every SC/ST woman in the village excluding her mother, and her peer group is every other school-aged SC/ST child within her village.

There are several advantages to using caste as an indicator of one's potential influences. Firstly, caste plays an important social role in rural India. Caste is hereditary, and often determines where households live within a village and what occupations they hold (Jodhka, 2002; Munshi and Rosenzweig, 2006; Luke and Munshi, 2011; Desai and Dubey, 2012). It regulates social interactions, marriage, and conduct towards women and members of other castes (Deshpande, 2001). Within-caste marriages are (still) strongly preferred by both men and women, particularly in rural areas (Anderson, 2003; Banerjee et al., 2013).

Secondly, caste affects how children socialize, particularly in educational settings. SC/ST children (also known as Dalits, or 'untouchables') are commonly discriminated against; for example, they are often made to sit separately from other children and are prevented from using the same drinking water facilities (Nambissan, 1996; Sarkar, 2014). Eating food cooked by Dalits is typically forbidden by upper caste members, and parents have been known to send their children to school instead with packed lunches or even withdraw them from school (Throat and Lee, 2005). Caste can also impact how children learn: Hoff and Pandey (2006) showed that children perform worse at tasks when their caste is revealed

[17] Caste can also refer to *varna*, which is also used to classify households.
[18] While Muslim households often have a traditional caste category listed, many surveyed are listed as Non-class. It is likely that these children do not interact often with children in the General caste group, so a separate category seems appropriate.

to other students.

Table 2 shows that enrollment rates for lower caste (SC/ST and Other Backwards Caste) and Muslim children continue to fall well below enrollment rates for children in the General caste category - around 25% of children from lower castes and 33% of Muslim children are unenrolled in 2006.

Table 3 provides summary statistics by caste group for various individual and household level variables. SC/ST and Muslim children in particular continue to be disadvantaged in a number of key areas: their parents are less educated, and their households are less likely to own land and have lower average levels of per capita consumption. There are also some differences among the village level characteristics, with lower caste children living in smaller, and for SC/ST children more remote, villages than children of other castes.

1.6 Estimation of social effects

This section presents the estimation strategy for the peer and role model effect. Formally, a child's peer group is defined as every other school-aged child in caste group c within village v in survey year t. Each caste group has a total of n_{cvt} children. An individual child i's enrollment decision is given by y_{icvt}, which is equal to 1 if a child is enrolled in school at the time of the survey and zero otherwise. Her peer group enrollment rate is given by $\bar{y}_{icvt}^{p}$, where $\bar{y}_{icvt}^{p} = \frac{1}{n_{cvt}-1} \sum_{j=1,j\neq i}^{n_{cvt}} y_{jcvt}$. In other words, $\bar{y}_{icvt}^{p}$ is the leave out mean for average enrollment within caste group c in village v.

Two variables are used to estimate the role model effect. The first is the average education of the peer group mothers for child i: $\overline{m}_{icvt}^{p} = \frac{1}{n_{cvt}-1} \sum_{j=1,j\neq i}^{n_{cvt}} m_{jcvt}$, where m_{jcvt} is the years of education of the jth mother within the village-caste group (the leave out mean for average mother education). Own mother education is not included here, but is controlled for in all regressions. The second variable is the average education of women within a child's caste group who do not have a school-aged child.[19] This is denoted by $\overline{w}_{cvt}^{p} = \frac{1}{k_{cvt}} \sum_{j=1}^{k_{cvt}} w_{jcvt}$, where w_{jcvt} is the years of education for the jth woman without a school-aged child and k_{cvt} is the total number of these women in caste c in village v at time t. Note that this variable is a straightforward mean that does not vary among children within caste group c in village v.

To test the influence of peers and role models on individual enrollment, one could estimate the following equation:

$$y_{icvt} = \alpha + \beta\, \bar{y}_{icvt}^{p} + \gamma\, \overline{m}_{icvt}^{p} + \delta\, \overline{w}_{cvt}^{p} + \theta x_{icvt} + \eta\, \bar{x}_{icvt}^{p} + z_v + \mu_t + \varepsilon_{icvt} \qquad (1)$$

The peer effect is given by β; the role model effect by γ and δ. Additional individual- and household-level control variables are included in x_{icvt}. Peer characteristics such as average peer age and gender are found in $\bar{x}_{icvt}^{p}$. Time-invariant characteristics of the child's village are included in z_v, and μ_t is a time fixed effect for the survey year.

There are several issues with using OLS to estimate (1). The first is that there may be selection in peer groups. There may also be unobservables at the village level which are influencing both peer and individual enrollment decisions. Finally, there is what Manski (1993) refers to as the 'reflection problem': there is simultaneity between y_{icvt} and $\bar{y}_{icvt}^{p}$. Empirically, it is impossible to identify the direction of the causal relationship; that is, whether a child's peer group is affecting her enrollment

[19] Any woman over the age of 16 without a child between 5 and 16 years of age in caste group c is included here. This reflects the fact that there may be other women outside of the peer group mothers who may influence a child's enrollment decision. For example, older girls who are not yet married, or women whose children are either younger than 5 or older than 16. Peer group mothers may have a different impact on other children's enrollment decisions than other same-caste women within the village, as they likely interact regularly with children other than their own. As a result, peer group mothers are considered separately from other women within the caste group.

decision, or she is affecting theirs. This poses a problem not only for the identification of β, but also for γ and δ.

1.7 Caste and peer group selection

As noted in previous sections, peer groups are defined within village and by caste group. In addition to the role that caste plays in regulating social interactions, and therefore serving as an indicator of a child's social influences, caste-based peer groups avoid selection issues which arise in more traditional peer group definitions.[20] The hereditary nature of caste implies that children are born with the caste-based social codes already in place, and these codes are largely unchanging over time. Caste is also well-defined – in rural India, households know what caste they are, and the caste of most other households in the village.

Nevertheless, there still may be some selection if children (or their parents) choose the village they live in. For example, a household may move to a village that has a certain preference for education or educational amenities. The peer group of the child will then consist of children from families (of the same caste) with similar preferences. However, household migration between villages in rural India is unusual, even rare; see, for example Munshi and Rosenzweig (2009, 2016). It is unlikely that families move from one village to another for, say, better schools. Urban-rural migration is similarly uncommon. Less than 2 percent of household heads in each of the two survey rounds considered report having migrated from elsewhere. Similarly, less than 2 percent of heads report their fathers as migrants.

[20] That is, individuals choosing their peer group because they are similar (or different) in some aspects that may be unobservable to the researcher. With endogenous peer group formation, it is impossible to separate the peer effect from a selection effect. Some researchers have exploited some type of random assignment in peer group formation to overcome this issue; for example, Sacerdote (2001) and Zimmerman (2003) use random college dorm assignment to estimate the effect of one's roommates on individual student outcomes. Ammermueller and Pischke (2009) consider peer groups at the classroom level, arguing that class assignment within a particular school and a particular grade are randomly assigned.

Defining peer groups at the caste-village level also avoids selection within village; for example, if a household chooses to live in some part of a village for reasons related to enrollment.[21] Even if households sort within villages in terms of where they reside, it does not change their caste group. Similarly, it also does not matter for the peer group definition whether currently enrolled children are attending a school within or outside the village.[22]

1.8 Correlated effects

Another potential issue with estimating (1) concerns unobservables which may be affecting enrollment decisions within the village (these effects are often known as 'correlated effects'). For example, though household-level migration rates are relatively low, there may still be some selection in migration patterns. Variation in school quality and preferences, along with policies regarding education, are also going to differentially affect enrollment decisions within villages.

To address this, two types of specifications utilizing fixed effects are presented. The first uses village fixed effects, which account for any unobservables at the village-level, for example, school resources, village-level preferences for education, and seasonal factors. They also account for account for variation in the distribution of caste groups between villages, as well as the enrollment rates of other caste groups.[23]

[21] It is difficult to determine the household's precise location within the village, and therefore each household's neighbors. GPS coordinates are not available for either the 1999 or 2006 round. In the 2006 survey there is some information on where the household is located within the village, though this information is typically not detailed enough to reliably determine a household's exact location or its neighbors.
[22] Children who are recorded as attending school outside the village are still considered part of the household, and will still interact (perhaps infrequently) with other children in the village. Furthermore, the child's enrollment decision is seen by other households within the caste group, and can influence the decisions of other children or households.
[23] On average, 23% of children in the sample are Scheduled Caste/Scheduled Tribe, 40% of children are Other Backwards Caste, and the remaining 37% of children fall into the General caste category. However, twenty-two villages in the final sample have only one caste, and almost half of the villages have just two caste groups.

However, the inclusion of village fixed effects could potentially absorb much of the variation in the data and effect the estimates. Only two survey rounds are used, and variation is derived from differences in enrollment across time and within each child's peer group. For smaller villages and villages that only appear in one survey round, this may be quite demanding. For this reason, the second specification uses tehsil-level fixed effects combined with village-level control variables to account for observable village characteristics. Tehsils (also known as blocks) are relatively small administrative areas containing multiple villages – each tehsil sampled includes between two to three sampled villages. Given the administrative nature of the tehsils, the inclusion of a tehsil fixed effect accounts for educational policies, as well as formal policies regarding wages and employment. Tehsils are also sufficiently small such that factors like weather conditions and local prices are controlled for. Village-level control variables such as the number and types of schools in a village are also included (see Table 3 for a list of village-level variables that are included as controls).

1.9 The 'reflection problem'

The final issue to be addressed is the 'reflection problem'; that is, disentangling an individual's influence on her peers from the peer's influence on the individual. To deal with this problem, an instrumental variables approach is used.[24] The instrument employed in this setting is the average education of the maternal grandmothers of the peer group. First stage results will reveal whether this variable is sufficiently correlated with $\bar{y}_{icvt}^{p}$, though intuitively we can think of grandmothers affecting their grandchildren's education either through their influence on the child's mother (or father) or through interaction with their grandchildren directly.

[24] In existing work on peer effects, researchers have often exploited some type of exogenous variation: Bobonis and Finan (2009) and Lalive and Cattaneo (2009), for example, use the randomized placement of a conditional cash transfer scheme that encourages recipients to attend school to identify the peer effect of children in recipient households on the schooling decisions of children from ineligible households.

For the exclusion restriction to hold, it must be that average peer maternal grandmother education is not related to individual child enrollment decisions. This could be violated with either a direct connection between peer maternal grandmother education and individual child enrollment, or an indirect connection through, say, the child's family. For example, a grandmother could encourage (or discourage) children other than their own grandchildren to attend school directly, or discuss educational decisions with other children's mothers or grandmothers.

The argument for the exclusion restriction is based upon the prevalence of marriage migration among women: the majority of women in rural areas migrate for marriage. Rural India is patrilocal, such that women typically join their husband's household after marriage, which is often in another village, district, or even state. Almost 50% of women were found to be migrants in the 2007-08 National Sample Survey (NSS), with 91% of these women reporting marriage as the reason for migration.[25] Fulford (2015) finds that almost 75% of women in rural areas have migrated for marriage, rising to over 95% for women in northern states.

If women migrate for marriage, then a child's maternal grandmother is likely to be living in a different village to the one her grandchild is currently residing in. In 2006, less than 5% of children lived in the same household as their maternal grandmother.[26] In addition, care of elderly parents has historically fallen to a son, who often lives in the same household or another nearby household as the parent, rather than a daughter (Vlassoff and Vlassoff, 1980; Caine, 1991; Rajan and Kimar, 2003).

Unfortunately, there is little information in either survey round regarding the current location of a child's maternal grandmother, and it could be the case that maternal grandmothers are living in another

[25] The NSS includes urban areas, where marriage migration is less prevalent.
[26] It is not possible to distinguish maternal and paternal grandparents in the 1999 survey.

household within the same village. Nonetheless, other information pertaining to other family members such as wives and daughters of the household head suggests that marriage migration among the sample households is extensive. In both survey rounds, spouses of male heads of household migrated an average of 32 kilometers from their birth household to their marriage household. Around 66 percent of spouses in 1999 and 77 percent of spouses in 2006 are from more than 5 kilometers away. Non-resident daughters of the household head in 2006 migrated 52 kilometers on average, and more than 80 percent were currently living in villages that were more than 5 kilometers away. In the 1999 survey, only 10 percent of non-resident daughters of the household head reported living in the same village. More than 60 percent of daughters moved to a different district, and 50 percent to a different state.[27]

Given that maternal grandmothers are unlikely to live in the same village as their grandchildren, average education of peer grandmothers will also be uncorrelated with out-of-sample peers and role models. For example, there may be influential women who affect child enrollment decisions, but are not included in the survey and therefore fall into the error term. If these women are correlated with the instrument, this would violate the exclusion restriction. Given that they are likely to be uncorrelated, however, the incidence of unsampled peers and role models will simply cause the estimates to be weaker than their true values.

Table 4 reports summary statistics for average grandmother years of education by state and by survey year. The number of observations in 2006 is, as expected, much lower across all states, though the decline does not seem to be biased towards one specific state or region (on average, at least). The

[27] While there is evidence to suggest that the majority women migrate some distance for marriage, and to geographically disperse places, it may still be the case that women are selectively moving to a type of village. For example, Rosenzweig (1988) and Rosenzweig and Stark (1989) argue that by sending daughters to different locations within the familial network, marriage can be used by the household to share spatially covariate risk. Women may also selectively migrate to villages with certain characteristics such as good schools or employment opportunities. The inclusion of village fixed effects account for this possibility.

average years of schooling for all grandmothers of school-aged children is similar in 1999 and 2006, with some variation between states. In some cases, average years of schooling for grandmothers has decreased. Standard deviations are also reported.

An important drawback of this instrument is that nearly half of children cannot be linked with grandmother education in the 2006 round (less than 10% of children have missing grandmother education for the 1999 round). The 2006 survey does not ask about mother education for any household member, though mother ID is given if the mother lives in the household. To link mother education to each child, information on women from the 1999 survey is used (which does ask about mother education). The missing values are therefore concentrated among children whose mothers were not included in the 1999 round, either because their household was newly added or they were not part of the household at the time of the survey.

1.10 Evidence of social effects

Equation (1) is estimated for all children between 5 and 16 years. Children with less than three peers are excluded from the analysis.[28] Standard errors are clustered by village and year. Three specifications of (1) are considered: the first is the most basic with village-level controls (columns (1) and (2)), the second has tehsil fixed effects and village controls (columns (3) and (4)), and the third is the most conservative with village fixed effects (columns (5) and (6)). All models have individual- and household-level controls including child age, gender, own mother, father, and grandmother education. Household caste, religion, demographic composition, and consumption per capita are controlled for, as are various characteristics of the head including age, marital status, and education.[29] Peer characteristics

[28] 98 observations in total were dropped due to small peer group sizes. These children were exclusively from villages with very few observations.
[29] A full list of the controls included in the regression can be found in Table 3. Full regression results can be found in the online Appendix.

(also known as contextual effects) such as average peer age and gender are also included. A linear probability model is used for estimation.[30]

The results are shown in Table 5 (see the online Appendix for the full regression results). Peer enrollment is found to significantly influence individual child enrollment decisions: under the most conservative estimate, a one percent increase in peer enrollment leads to an increase in the probability of a child enrolling in school by 0.58 percent. The role model effect among both the peer group mothers and other women within a child's caste is close to zero and statistically insignificant for almost all specifications (and insignificant for all IV estimates).

One interesting finding is that the IV estimates in Table 5 are larger than the OLS estimates. There are several possible reasons for this. Firstly, there could be bias in the IV estimate if the exclusion restriction is violated. Though unlikely given the patrilocal norms in rural India, this cannot be ruled out. Alternatively, there may be measurement error in the enrollment variable which is causing attenuation bias in the OLS estimate. This is the conclusion reached in Ammermueller and Pischke (2009) and Helmers and Patnam (2014) who similarly report larger IV estimates than the OLS estimates. If peer grandmother education is uncorrelated with the measurement error, then the IV estimates are likely to reflect the true value of the peer effect.

Finally, it is certainly plausible that the true IV estimate is larger than the OLS estimate. OLS is an estimate of the average treatment effect, while IV estimates the local average treatment effect (Imbens and Angrist, 1994). If there are heterogeneous subpopulations, then the LATE may be larger than the ATE. For example, a high education grandmother is going to have a stronger effect amongst

[30] Results are also robust to using a probit model for estimation.

kids who are more influenced by peers than a low education grandmother.

1.11 Social effects by caste group and gender

Table 6 considers the impact of peers and role models separately for lower (specifically SC/ST, and OBC) and General caste children (the Muslim category was omitted here due to its relatively small size). OLS and IV results for the model with both village controls and tehsil fixed effects can be found in columns (1), (2), (5) and (6), while the more conservative estimates with village fixed effects are in the remaining columns. As in Table 5, each model contains the full list of individual- and household-level controls (full regression results can be found in the online Appendix). The results show that lower caste children are more strongly influenced by their peers than children in the General Caste category, though this result is only significant in the model with tehsil-fixed effects (column (4)). As in Table 5 the role model effect is negligible.

One explanation for the muted role model effect is that women may not be appropriate role models for boys. Children may also be more influenced by peers of the same gender - for example, boys' enrollment decisions may have little to do with girls' enrollment once factors such as educational quality and village-level social norms are controlled for. There may be a stronger effect when equation (1) is estimated separately by gender. Peer groups are redefined as all other school-aged children within the village who are of the same caste group and the same gender. As before, children with fewer than two peers are excluded. The results for the model with village fixed effects are presented in Table 7 (full regression results can be found in the online Appendix).

Peers significantly influence enrollment decisions for both genders. The effect is strongest both in significance in magnitude for girls. There is also a significant role model effect for the OLS estimates, though this significance disappears once any endogeneity caused by the peer enrollment variable is instrumented for.

The next set of results separate peer groups within caste by age group. Two age groups are considered: children aged 5 to 10 and age 11 to 16.[31] Peer groups are therefore defined as all other school-aged children in the village of the same caste and age group. The right panel of Table 7 reports the results. Younger children appear to be more strongly influenced by their peers than older children and as in previous results, the role model effect is small and insignificant.

1.12 Social effects and schoolwork

A common concern with enrollment rates is that enrollment does not necessarily imply attendance. Supply side problems abound, with many schools suffering from poor infrastructure, facilities, and lack of teaching input (PROBE, 2011). Children may enroll in school, only to find that their teacher is absent. Children of certain caste, religion or gender may be discriminated against both outside and inside the classroom, potentially discouraging them from attending class each day. In either of these cases, a child may be enrolled, though not attending school (or attending irregularly).

Though neither survey round asks specifically about school attendance, the number of hours a child spends studying each day is available. Given that peers have been found to positively influence child enrollment, we should expect peer enrollment to increase the amount of time per day a child spends studying. If this is the case, it is of interest to know the effect of peer enrollment on other activities such as farm employment (non-farm employment is relatively uncommon among the children surveyed), housework, and leisure time, with the expectation that at least one of these categories will be

[31] Finer delineations of age are not possible given the relatively small sample sizes for each village-caste group. A comparison of gender- and age-based is problematic for similar reasons.

negatively related to peer enrollment. We might also expect role models to have a greater impact on how a child spends his or her day, particularly in regards to study and work hours.

Table 8 lists the average time a child spends per day on these activities by gender, age group, and by survey year. Time is measured in hours and outside of leisure time, the categories are roughly comparable between survey rounds (leisure in 2006 includes time spent sleeping while leisure in the 1999 questionnaire does not). Time spent studying across all caste groups has increased between the two rounds, though children from lower caste groups still spend less time studying than other children, similar to what was found with school enrollment. SC/ST children in particular spend more time working on farm employment, and also on housework (which includes both household chores and fuel collection). Lower caste children also spend more time on leisure activities.

To determine the effect of peers and role models on time allocation, the following equation is estimated:

$$h_{a,icvt} = \alpha_a + \beta_a \bar{y}^p_{icvt} + \gamma_a \overline{m}^p_{icvt} + \delta_a \overline{w}^p_{cvt} + \theta_a x_{icvt} + \eta_a \bar{x}^p_{icvt} + z_v + \mu_t + \varepsilon_{a,icvt} \qquad (2)$$

where $h_{a,icvt}$ is the number of hours per day child i spends on activity a. The control variables included in x_{icvt} and $\bar{x}^p_{icvt}$ are the same as in previous sections. The instrument for peer enrollment is again the average education of the peer group maternal grandmothers. The three different specifications used in the previous section (namely, village controls; tehsil fixed effects with village controls; and village fixed effects) are estimated. Time fixed effects are included in all models and will control for the difference in measurement of leisure time between the two years.

Table 9 lists the results for all school-aged children. Peer enrollment is shown to significantly increase the number of hours per day spent studying, suggesting that children are not simply enrolling in school but also attending. Under the most conservative specification, (6), a one percentage increase in peer enrollment leads a child to study 2.4 more hours per day (though this result is statistically insignificant). A slightly smaller in magnitude but statistically significant results is found in the estimates without village fixed effects.

Interestingly, the role model effect arises when other activities are considered. Both peer mother education and other women education significantly decrease the time a child spends on farm and non-farm employment. Peer mother education is also negatively related to the amount of time a child spends on housework. No significant effects are found for time spend on leisure.

1.13 Conclusions

This paper uses unique features of rural Indian villages to estimate social effects in child schooling decisions, considering specifically a role model effect and a peer effect. The prevalence of marriage migration implies that maternal grandmothers are unlikely to live in the same village as their grandchildren, and the average education of the maternal grandmothers of a child's peer group is used as an instrument for the peer effect. The role model effect was found to be small and insignificant in the most conservative specification, suggesting that women within a child's village and caste group appear to have little impact on individual child enrollment decisions. However, the peer effect is found to be strongly significant. The magnitude of the peer effect varies depending child's caste group, age group and gender.

While this paper finds that social effects, and specifically peer effects, impact enrollment decisions of children in rural Indian households, it is unable to determine the mechanism through which these effects are operating.[32] It may be that children are learning about the value of education as their

[32] Young (2009) provides a summary of possible channels in the context of innovation and technology diffusion.

peers enroll, prompting a child to also enroll. Alternatively, children may feel some type of social pressure to enroll in school if other children of the same caste are enrolled. Relatedly, it is unclear whether these possible channels occur through the child or their parents; rather than the child, it might be the child's mother who is learning about the benefits of enrollment, or feeling pressure from other mothers to enroll her child.

The existence of the peer effect suggests that the overall impact of a policy aimed at increasing enrollment may be larger than the individual impacts suggest. Following Glaeser et al. (2003), the social multiplier in this context for a caste-based peer group size of 30 is equal to 2.6.[33] That is, the long run impact of the policy will be more than double what the individual effects suggest. The social multiplier also has important implications for how educational programs are targeted - spreading resources across caste groups suggests that the program will have a larger overall impact on the long run in comparison to a program that targets a single caste group. Policies that utilize the peer effect and the accompanying social multiplier may have a role in achieving universal education for all children.

[33] The average peer group size when peer groups are defined by caste is 35. The average peer group size for children in SC/ST castes is 25, 40 for OBC children and 37 for children in the Gneral caste group. The larger the peer group size, the larger the social multiplier.

Chapter 2: Are Poor Individuals Mainly Found in Poor Households? Evidence Using Nutrition Data for Africa[34]

2.1 Introduction

While it is widely appreciated that poverty is an individual deprivation, household aggregate data are almost invariably used to infer individual poverty. It is almost always assumed that each individual within the household has the same level of economic welfare as measured by household aggregate consumption per person (or per equivalent single adult). An array of antipoverty programs, now found almost everywhere, are targeted on this basis, though typically using readily available proxies for household consumption or income per person.[35] Partly in response to concerns about high chronic undernutrition in certain regions, including Africa, there is an expanding effort at social protection in developing countries and this effort is typically focused on transfers targeted to poor families.[36] For its part, the World Bank has made reaching poor families—as often identified by the poorest two quintiles of people based on household consumption per person—the main objective of its social protection operations.

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[34] This chapter is coauthored with Martin Raallion and Dominique van de Walle. Martin Ravallion is with the Department of Economics at Georgetown University and Dominique van de Walle is with the Development Research Group at the World Bank. The authors are grateful to the World Bank's Strategic Research Program for funding assistance. Helpful comments were received from Harold Alderman, Arthur Alik-Lagrange, Emanuela Galasso, Adam Wagstaff and seminar participants at the University of California Riverside and the Paris School of Economics. These are the views of the authors and need not reflect those of their employers.

[35] On these programs in developing countries see Coady et al. (2004), Fiszbein and Schady (2010), Ruel et al. (2013), Del Ninno and Mills (2015), and Ravallion (2016, Chapter 10).

[36] For evidence on the expansion in social protection programs in developing countries see Ravallion (2016, Chapter 10). Various case studies of these programs in Africa are found in Del Ninno and Mills (2015).

Reaching deprived individuals using antipoverty programs that explicitly target poor households is an attractive option for three reasons. First, there is a data constraint, namely that standard data sources do not allow us to measure individual consumption. Second, interventions at the individual level may be seen to be paternalistic and intrusive (as they require intervention within families) and may well be costly (to the extent that they rely on fine targeting, constrained by the fact that individual deprivations are not comprehensively observed in large populations). Third, a large literature has documented that poorer households in terms of consumption, income or wealth are more likely to include deprived individuals.[37] Aggregate household resources constrain consumption for all household members. For these reasons, it is not surprising that, in practice, many social policies hope to reach deprived individuals by targeting poor households, or (more commonly) households with characteristics known to be associated with poverty.

However, the existence of a household wealth effect on individual welfare does not imply that targeting poor households will be very effective in reaching poor individuals. A growing body of empirical evidence casts doubt on that assumption. Relevant evidence includes:[38] (i) evidence that rejects a unitary model of the household, suggesting new sources of inequality within households; (ii) studies explaining the 'missing women' phenomenon; (iii) evidence of discrimination against certain household members such as orphans and widows; and (iv) evidence of unequal exposure to transitory shocks. Heterogeneity in factors influencing individual poverty can also mean that transfers to poor households often miss deprived individuals. It is important for policy makers to know whether standard household data sources can be relied upon to also reach poor individuals.

This paper tries to throw light on how well widely-used household-based measures perform in identifying disadvantaged individuals. Are we reaching such individuals adequately by simply targeting

[37] The evidence is reviewed in Ravallion (2016, Chapter 7). The present paper will return to the literature.
[38] We provide references on these points later.

"poor" households? Or do many of them live in households that are not identified as poor? Is it harder or easier to reach vulnerable women and children using household data in settings in which the incidence of individual level disadvantage is high or average income is low?

Missing data on individual-level poverty present a significant hurdle to examining these issues. However, there is one dimension of individual welfare that can be observed in many surveys, namely nutritional status as indicated by anthropometric measures. Undernutrition can stem from inadequate caloric intakes or deficiencies in protein or micronutrient intakes, or from illness that impedes nutritional absorption. Such nutritional deprivations are of direct and immediate concern, and there is also evidence of longer-term social and economic costs, especially of low-birth weight and chronic undernutrition in childhood. Although nutritional status admittedly represents only one dimension of individual poverty there can be no doubt that it is an important dimension. It is also frequently used as a proxy for individual welfare.

The paper uses undernutrition as the measure of individual welfare to explore the questions posed above. We use data for 30 countries in sub-Saharan Africa (SSA), where chronic undernutrition among children is a major policy concern. The latest data at the time of writing indicate that the count of stunted children in SSA has risen by 12.5 million since 1990. The incidence of child stunting in SSA today is probably the highest of any of the standard geographic groupings of countries.[39] We draw on anthropometric data for 390,000 women and children from the Demographic and Health Surveys (DHS). These data can be used to identify nutritionally vulnerable women and children. The DHS also include a household wealth index based on a household's assets and living conditions. We use this index as a proxy for household wealth. However, aggregate consumption may well be a better predictor of

[39] These observations are from the World Bank's website on nutrition and the latest available estimates compiled by UNICEF. Historically, South Asia has been the region with highest incidence but that region has been making greater progress than SSA in this respect. Also see the discussion in Smith and Haddad (2015). Differences in population growth also affect this shift to SSA of the global share of the undernourished.

individual welfare (and nutritional status) than the DHS wealth index, which (for example) may not respond quickly to shocks. Wherever possible, we complement the DHS data with good-quality nationally-representative household consumption surveys from the World Bank's Living Standards Measurement Study (LSMS).

We acknowledge that nutritional status is not all that matters to individual welfare; our findings may not hold for other dimensions of individual poverty. Yet, in the absence of better individual poverty measures, and given considerable evidence of unequal intra-household allocation of resources, it is important to investigate this issue. Our results are also relevant to policy makers who are specifically interested in reaching undernourished individuals viewed as a health deprivation. There are various forms of direct interventions with the aim of improving nutrition, including direct nutrition supplementation and promoting better health practices.[40] Many of these are implemented through health clinics and delivery points other than the household. However, there is a growing interest in doing so more through household-based policies—by integrating nutrition programs within anti-poverty policies more broadly. We throw light on whether this might work.

Our principle finding is that, although the incidence of undernutrition tends to be higher in poorer households, the nutritional deprivations are spread quite widely through both the wealth and consumption distributions, such that the joint probability of being an underweight woman or child and living in the poorest household wealth quintile is low. This also holds when we use an augmented regression to control for various individual- and household-level factors which may influence nutritional outcomes. Our results point to the need for broad coverage in efforts to address undernutrition and, by extension, individual poverty, rather than subsuming this problem within household targeted antipoverty

[40] See for example the package of nutritional interventions described in Bhutta et al. (2013).

interventions. Data availability limits how far we can go in explaining our findings, but we point to evidence suggesting that intra-household inequality may well be a major factor.

The following section considers relevant arguments and evidence from the literature. Section 3 outlines a simple theoretical model to help understand the relevant aspects of the joint distribution of household poverty and individual undernutrition. Section 4 then reviews the data we shall be using. Section 5 presents the main findings, while Section 6 tests robustness to allowing for a wider range of covariates. Section 7 concludes.

2.2 Insights from the literature

Several strands of the literature have bearing on how effective household poverty data can be expected to be in identifying poor individuals. Here we summarize relevant arguments and evidence.

A body of research on the economics of the household has focused on the wealth effect on nutritional status, i.e., how much nutrition improves as a household's economic welfare—income, consumption or wealth—rises. One strand of this literature has estimated income elasticities of demand for food and (hence) nutrition; an influential early example is Behrman and Deolalikar (1987). Rather than focus on food consumption, as in consumer demand studies (such as Pitt, 1983), other work has instead studied the income effect on nutritional adequacy, taking account of requirements for good health and normal activities in society. A low income elasticity of demand for food can be consistent with a high responsiveness of nutritional adequacy to income gains, since even small gains in nutritional intakes can make a big difference at low levels (Ravallion 1990, 1992).[41]

[41] While it is not an issue taken up here, it is now well recognized that nutritional intakes can also be too high from the point of view of good health and normal activity levels. A strand of the literature has focused on obesity and its relationship to wealth in both rich and poor countries; for a review see Ravallion (2016, Chapter 7).

New evidence on this topic has emerged from analyses of the many micro data sets (including the DHS) that have become available to researchers over the last 20 years or so. A limitation of the DHS is that the surveys have not included the questions needed to measure consumption or income. (At the same time, most surveys of the LSMS-type have not included anthropometrics.) The DHS wealth index was developed to help address this deficiency (Filmer and Pritchett 2001). Some studies have argued that the DHS wealth index is a good predictor of various human capital and other outcomes (Filmer and Pritchett 1999, 2001; Filmer and Scott 2012; Sahn and Stifel 2003; Petrou and Kupek 2010). For example, on comparing DHS wealth indices, Filmer and Scott (2012, p. 359) conclude that "…inferences about inequalities in education, health care use, fertility and child mortality, as well as labor market outcomes, are quite robust." Similarly, Sahn and Stifel (2003, p. 463) argue that their version of the wealth index "…is a valid predictor of a crucial manifestation of poverty—child health and nutrition." However, other studies have been less supportive and have found only seemingly modest correlations between nutritional, health and other outcomes and wealth indices (Hong and Hong 2007; Zere and McIntyre 2003; Howe et al. 2009). Different data sets can tell different stories here, so a comprehensive look at the evidence across multiple countries is needed.

A strand of the literature has used the DHS wealth index to measure inequalities in child nutritional status, mainly using the concentration curve which gives the share of undernourished children living in the poorest x% of households based on the wealth index (Kakwani et al., 1997; Wagstaff and Watanabe 2000; Wagstaff et al., 2014; Bredenkamp et al., 2014).[42] A widely-used measure based on this curve is the concentration index, given by twice the area between the curve and the diagonal (analogous to the Gini index). A key finding from this literature of relevance here is that the concentration indices for child stunting and wasting in developing countries are almost invariably negative. A typical

[42] There has been far less focus on inequalities in malnutrition among women.

conclusion found in this literature is that "Unsurprisingly, in all countries, undernutrition is concentrated among the poor." (Bredenkamp et al., 2014, p.1330). Such assessments appear to support the common, but often implicit, assumption among social policy makers that targeting poor households will be effective in reaching undernourished individuals. However, the concentration indices are rarely more negative than -0.3, with median values typically around -0.15 to -0.10 (depending on the measure of undernutrition).[43] While this confirms that children from wealthier households tend to be better nourished (given that the index is negative), it also suggests that there is quite wide dispersion of undernutrition across wealth strata. We study this dispersion, focusing on its implication in the context of efforts to use household poverty data to target undernourished individuals.[44]

A number of recent papers review the existing evidence on the nutritional impacts of income growth and income support to poor households. On the first, as already noted, several papers find low income effects, particularly in the short-term (Grogan and Moers 2016; Haddad et al. 2003; Smith and Haddad 2015). With respect to the second, Alderman (2015) and Ruel and Alderman (2013) conclude that social safety nets targeting poor households with food or cash transfers (whether conditional or unconditional) have generally had limited impacts on children's nutritional status. The papers speculate that this may be because the targeted households are not those that have young children in the right age range. They do not question the practice of targeting poor households to reach undernourished individuals. Manley et al. (2013) undertake a systematic review and meta-analysis on conditional and unconditional cash transfers and child nutrition and come to similar conclusions.

The existence of intra-household inequality is clearly relevant. The unitary model of the household (characterized by a single utility function) has found little support empirically, and various

[43] The online addendum to Bredenkamp et al. (2014) provides concentration indices across 80 developing countries for child undernutrition using the wealth index as the ranking variable. The median for stunting is -0.15.
[44] We do not use the concentration index here as there is greater interest in this context in points on the concentration curve.

alternatives have been proposed (as reviewed by Chiappori and Mazzocco, 2015, and Baland and Ziparo, 2017). These models permit new sources of inequality within households, such as in reservation utility levels. An extensive literature details intra-household inequalities in resource allocations and outcomes (as reviewed in World Bank 2012). There are two policy implications: targeting poor households may well miss some significantly disadvantaged individuals and targeted households may not allocate the benefits to the neediest within the household. This paper only addresses the first.

It is well recognized in principle that household-level consumption or income-based measures don't allow for inequality within the household. There is also (largely qualitative) evidence that certain individuals are poor and/or vulnerable, but do not live in households that would normally be considered poor and so are hidden from view in standard data sources on poverty. Differentiation between men and women has been widely documented in human capital, legal protection, constraints stemming from social norms, roles and responsibilities, and control over resources (Ezemenari et al. 2002). For Africa, there is evidence that household shocks affect men and women differentially, with women bearing the brunt of negative shocks (Rose 1999; Dercon and Krishnan 2000). Such differentiation can be expected to have consequences for measures of poverty and inequality. In an important early example, Haddad and Kanbur (1990) find that such measures for the Philippines are appreciably underestimated using standard household-level data, although the "profiles"—the comparisons of these measures across sub-groups such as urban and rural areas—were found to be quite robust. Using a survey for Senegal that (unusually) collected a relatively individualized measure of consumption, Lambert et al. (2014) find significant inequalities within the household and a sizeable gender gap in consumption. Using the same data, De Vreyer and Lambert (2016) estimate that about one in eight poor individuals live in non-poor households. Using anthropometric data, Sahn and Younger (2009) find that about half of country-level inequality in the Body-Mass Index is <u>within</u> households rather than between them.

Other work has emphasized the poverty of specific types of individuals. Recent research on Mali confirms that widows—most of whom are absorbed into male headed households and can be quite young—experience significantly lower levels of individual (non-income) welfare indicators than women of other marital statuses, and that the disadvantage persists through remarriage (van de Walle 2013). There is also a large literature on orphans in the context of AIDS deaths, and the disadvantages they may face, particularly in schooling (Bicego et al. 2003; Case et al. 2004; Evans and Miguel 2007). While it may well be more likely that these disadvantaged groups live in relatively poor households, they may also be spread quite widely across the wealth distribution.

There are other sources of heterogeneity in individual health and nutrition at given levels of household wealth. Wagstaff (2003) finds large differences across developing countries in the incidence of underweight and stunted children even if one controls for wealth as best one can. Wagstaff found in addition that these differences are negatively correlated with public health spending per capita. This is consistent with other findings suggesting that cross-country differences in public health spending matter more for the poor than for others (Bidani and Ravallion 1997). The well-off are better able to protect their children's nutrition and health status from weak public provisioning and poor health environments. However, the powerful role of complementarities and externalities in water, sanitation and hygiene means that the better off also remain vulnerable to these deficiencies (Duflo et al. 2015; Ngure et al. 2014). Cross- country comparisons of stunting incidence have also pointed to the role played by access to health-related infrastructure (such as water and sanitation facilities) in addition to household characteristics such as food availability and maternal schooling (Smith and Haddad 2015).

In the light of these studies, prevailing methods of measuring poverty and designing antipoverty policies using the household as the unit of observation may be inadequate. Economists and policymakers have traditionally looked at poverty and vulnerability using the household as the unit of observation. The gold standard for measuring poverty has long been household-based consumption normalized for household size and (possibly) demographic composition. In the absence of data on such poverty indicators and the costs of collecting them for the whole population, it has become common in policy making to use proxy-means-testing (PMT) and other methods such as community-based targeting to target anti-poverty programs.[45] A number of studies have assessed how well PMT does in targeting poor households (Brown et al. 2016; Alatas et al. 2012; Kidd and Wylde 2011). But there has been little attention to how well such methods identify disadvantaged individuals.

2.3 An expository model

An important point that has not received adequate attention in the literature on antipoverty policies is that heterogeneity in individual economic welfare at any given level of aggregate household welfare can restrict the scope for reaching vulnerable women and children using household poverty data. And this is the case even when there is a strong household income effect on individual welfare. To anticipate our empirical work, we shall identify individual welfare by nutritional status.

We elaborate this point in a simple expository model. The nutritional attainments of an individual are denoted n, while the wealth of the household to which the individual belongs is w. To keep notation simple, we take n and w to be normalized by appropriate cut-off points (stipulated nutritional thresholds or poverty lines) such that a person is undernourished if (and only if) $n<1$ and a household is poor if $w<1$. These two random variables have a (continuous) joint density $f(n, w)$.

The relationship between the two variables depends on a number of factors, including intra-household inequality, the local health environment (including water and sanitation), access to relevant health and nutritional knowledge, and child care. To keep our model simple, we collapse the

[45] Using more easily observed correlates of consumption or income such as assets and household characteristics, PMT uses the predicted values from multivariate regressions for consumption or income.

heterogeneity into one composite factor denoted ε, which we can take to be scaled such that it is bounded below by zero and above by unity. For concreteness, we might suppose that ε is the share of the household's total nutritional intake devoted to other household members. The expected value of individual nutritional status given *w* and ε is:

$$E(n|w, \varepsilon) = n(w, \varepsilon) \tag{1}$$

It is assumed that the function *n*(.) is strictly increasing in *w*—the slope of this function with respect to *w* is the aforementioned wealth effect on undernutrition—and that the function is strictly decreasing in ε at given *w*. (Continuing the previous example, we can have the special case $n(.) = (1-\varepsilon)\phi(w)$ where $\phi(w)$ is aggregate household nutrition when wealth is *w*.)

Motivated by the existence of a wealth effect on nutritional attainments, it is understandable that a policy maker may be drawn to targeting wealth-poor households so as to reach nutritionally-deprived individuals. However, the common finding in the literature reviewed in Section 2 that the expected value of nutritional status rises with wealth does not necessarily mean that household wealth will provide a reliable indicator of individual outcomes for the purposes of policy. It makes more sense to focus on the conditional probability distribution $\Pr(w < 1|n < 1)$, i.e., the probability of living in a wealth poor household given that one is undernourished. (For example, if an antipoverty policy made a transfer payment in a fixed amount to every poor household ($w < 1$) then the proportion that reached poor individuals will be $\Pr(w < 1|n < 1)$.) By well-known properties of conditional probabilities:[46]

$$\Pr(w < 1|n < 1) = \frac{\Pr(n < 1, w < 1)}{\Pr(n < 1)} \tag{2}$$

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[46] Alternatively, one might calculate $\Pr(n < 1|w < 1)$. However, focusing on $\Pr(w < 1|n < 1)$ seems to accord more directly with the relevant question for policy purposes. Of course the two conditional probabilities are linked by Bayes' theorem. Readers can back out $\Pr(n < 1|w < 1)$ from our results below.

The numerator is the joint probability of being both undernourished and living in a poor household, and the denominator is the overall rate of undernutrition.[47] In other words, we ask: among those who are undernourished, what share also live in wealth-poor households?

We can now readily see how heterogeneity can confound a policy maker's ability to reach undernourished individuals using only household data. Let w^* denote the minimum level of wealth that is needed to not be undernourished given ε, i.e., $n(w^*, \varepsilon) = 1$. Plainly, w^* is a strictly increasing function of ε, which we write as $w^*(\varepsilon)$.[48] Then we have:

$$\Pr(w < 1 | n < 1) = \Pr[w < 1 | w < w^*(\varepsilon)] \tag{3}$$

Now consider the lower and upper bounds of ε. We assume that the wealth-poverty line is set such that nutritional status is deemed to be adequate for someone at that line when $\varepsilon = 0$. For example, when intra-household inequality is the source of heterogeneity, a fair division of food should allow all those living in households around the poverty line to be adequately nourished. Then $w^*(0) \le 1$ and $\Pr(w < 1 | w < w^*(0)) = 1$. That is, targeting the wealth poor when there is no intra-household inequality assures that one reaches all those households with undernourished individuals. By contrast, given that w^* is an increasing function of ε, when ε approaches its maximum value, a high level of household wealth will be needed to assure that enough of the household's resources "trickle down" to avoid undernutrition in women and children. (This is clear if one considers again the example when ε represents intra-household inequality.) Specifically, $w^*(1) = w^{\max}$ and $\Pr(w < 1 | w < w^{\max}) = \Pr(w < 1)$. By invoking continuity, it is clear that $\Pr(w < 1 | n < 1)$ must be a non-increasing function of ε over (0, 1)

[47] More precisely $\Pr(n < 1, w < 1) \equiv \int_0^1 \int_0^1 f(n, w) dndw$ and $\Pr(n < 1) \equiv \int_0^1 \int_0^\infty f(n, w) dwdn$.

[48] For example, if $n = (1-\varepsilon)\phi(w)$ then $w^* = \phi^{-1}[(1-\varepsilon)^{-1}]$.

and strictly decreasing for some sub-intervals. As ε approaches its upper limit, the probability of reaching undernourished individuals by targeting poor households is no higher than the overall poverty rate.

We will study how the conditional probability varies across countries with the overall poverty rate. Intuitively, the higher the household poverty rate the more likely it is that a policy that successfully targeted poor households will reach poor individuals. But how much better will it be? To answer this we assume that the empirical relationship can be written as:

$$\Pr(w<1|n<1)=\psi[\Pr(w<1)]+\upsilon \tag{4}$$

where ψ is a continuous regression function and $E[\upsilon|\Pr(w<1)]=0$. If we again consider a uniform transfer to the poorest p% of households then the function ψ can be interpreted as the share of those transfers going to the poorest individuals, as assessed by their nutritional status. There are obvious boundary conditions to impose on the function ψ. When nobody is wealth poor none of the undernourished will be wealth poor, yet when everyone is wealth poor, this must of course also hold for the undernourished; in terms of equation (4) we expect that $\psi(0)=0$ and $\psi(1)=1$. So the empirical relationship must be increasing, although not necessarily monotonically. We can also expect that $\psi[\Pr(w<1)]\geq\Pr(w<1)$ (given a positive wealth effect on undernutrition).

In our empirical implementation across 30 countries in SSA we will assume that ψ is a quadratic function, which (on imposing the boundary conditions, $\psi(0)=0$ and $\psi(1)=1$) implies that (4) can be written as:

$$\Pr(w<1|n<1)-\Pr(w<1)=-\beta\Pr(w<1)[1-\Pr(w<1)]+\upsilon \tag{5}$$

Where $\beta>0$ is a parameter to be estimated using the estimates we obtain across countries. To help interpret β it can be noted that its value is directly proportional to a version of the concentration index:

$$C = \int_0^1 \psi(x)dx - 0.5 \tag{6}$$

For a quadratic ψ function (implying (5)), it is readily verified that $\beta = 6C$. Thus β can be interpreted as an overall measure of how much better targeting poor households works for reaching poor individuals when the overall household poverty rate is higher. When $\beta = 0$, the conditional probability of an undernourished person living in a wealth-poor household is no different (in expectation) to the overall wealth-poverty rate as one varies the latter from 0 to 1. This will be the case if there is no wealth effect on undernutrition. At the other extreme, when $\beta = 6$ ($C = 1$), one finds that all undernourished individuals are found in wealth poor households at each and every point.

It is less clear how the conditional probability in (2) varies with the overall rate of undernutrition, $\Pr(n < 1)$. In countries in which the rate of undernutrition is higher do we find that a higher proportion of the undernourished also live in wealth-poor households? The value of $\Pr(w < 1 | n < 1)$ is undefined at the lower bound of undernutrition, $\Pr(n < 1) = 0$. Comparing strictly positive values, a higher $\Pr(n < 1)$ can come with a change in the numerator of (2), so that it cannot be presumed that the conditional probability will fall. To see why, suppose that there is a change in the joint distribution $f(n, w)$, such that $\Pr(n < 1)$ increases. Furthermore, suppose that the joint probability increases for all points with n<1 and w<1, while the opposite happens at all other points in the (n, w) space. In this case it is clear that the joint conditional probability must also increase along with the marginal, with a theoretically ambiguous implication for the conditional probability. (A similar argument can be made with respect to how $\Pr(w < 1 | n < 1)$ varies with $\Pr(w < 1)$.)

2.4 Data

The model in the previous section formalizes the intuition that heterogeneity, such as due to intra-household inequality or the local health environment, diminishes the scope for reaching poor individuals by targeting poor households. But how much does this matter empirically? Is the wealth effect on individual nutritional status strong enough to allow satisfactory targeting of vulnerable women and children? The rest of this paper addresses these questions, also using some of the measurement concepts from the previous section.

Our data are drawn from the Demographic Health Surveys (DHS) and the LSMS. We use the most recent DHSs available.[49] Table 1 lists the countries included in our analyses and the year of each survey.

We study the nutritional outcomes of women and children. For women, the two variables we employ are the body mass index (BMI) (also known as the Quetelet index), defined as a woman's weight (in kilograms) divided by her height (in meters) squared, and an indicator for being underweight, which is set equal to one if a woman's BMI is lower than 18.5 and zero otherwise. The DHS excludes values of BMI that are smaller than 12 and greater than 60 on the grounds that these are almost certainly measurement errors. We do the same for the consumption surveys. BMI is computed by the DHS for samples of women aged 15 through 49. For the LSMS surveys we restrict women to the same age range. We exclude all women who report being pregnant at the survey date.[50, 51] On average, pregnant women

[49] Several countries had to be excluded due to older survey data that did not contain many of the key variables needed, namely the Central African Republic, Chad, Comoros, Madagascar, Sao Tome and Principe, and South Africa.

[50] Unfortunately, we are unable to exclude pregnant women for Tanzania's consumption survey as it did not ask women whether they were pregnant.

[51] We also dropped observations with missing values for any variables used in the paper, such that sample sizes are consistently the same and comparable throughout the paper. However, we tested the effect of relaxing this constraint and found that it makes negligible difference to the results.

represent approximately 10 percent of all women aged between 15 and 49. The addendum gives the pregnancy incidence for each country in the DHS dataset.

For children, we use the z-scores for height-for-age (stunting) and weight-for-height (wasting).[52] These anthropometric data are measured for all children aged under 5 in the DHS and LSMS surveys. We then create our measure for stunting (low height-for-age) and wasting (low weight-for-height). A child is deemed to be stunted if his height-for-age z-score is two standard deviations below the median of the reference group; wasting is defined similarly using weight-for-height. Stunting and wasting, while both considered indicators of undernutrition, have different causes and effects. Stunting is an indicator of persistent, longer-term, chronic undernutrition from which it is much harder for a child to recover. Compared to wasting, it is known that stunting has adverse longer term consequences for child development.[53] Wasting tends to be more responsive to short-term (possibly seasonal) food deprivations or illnesses.

Tables 2 and 3 give the summary statistics for the nutritional outcomes for women and children using the DHS and LSMS.[54] Focusing on the larger sample of countries available in the DHS and taking population-weighted averages, we find that 11% of adult women are underweight, while 32% of children are stunted and 9% are wasted (similar numbers are found for children in the LSMS). Across countries, a higher incidence of underweight women is associated with a higher incidence of wasted children (r=0.40, significant at the 5% level[55]). The correlation between women's and children's nutritional status is weaker for stunting (r=0.14).[56] This is what we would expect if a woman being

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[52] These variables are already constructed in the DHSs. For the consumption surveys we use the Stata command zscore06 to convert height and weight values into a standardized value.
[53] See, for example, Walker et al. (2007) and Hoddinott et al. (2008).
[54] There are some discrepancies in the means between the two datasets, much of which is likely to do with the timing of the surveys, although differences in sample selection and measurement may also be contributing.
[55] For prob.= 0.05, the critical value of the correlation coefficient is 0.306.
[56] This weak correlation between wasting and stunting is not surprising (Victora 1992). Although there is some evidence that wasting in early childhood can cause subsequent stunting (Richard et al. 2012), the fact that stunting is a longer-term condition while wasting tends to be more transient points to different causative factors.

underweight and her children being wasted are caused by similar short-term shocks, while stunting is a more long-term condition.

Table 4 provides summary statistics for selected other indicators that have been identified in past work as relevant to nutritional outcomes, specifically GDP per capita, the national poverty rate, the female literacy rate, and access to improved water and sanitation facilities. Table 5 gives the correlation matrix for the three nutritional indicators from Table 2 and the five country-level indicators from Table 4. GDP and FLR are both negatively correlated with the nutritional indicators, as is access to water and sanitation. For GDP, the correlation is only statistically significant for stunting (r= -0.54). The FLR has a large and significant negative correlation with the wasting rate (r= -0.73), but the correlations are not statistically significant for underweight women or stunted children. The poverty rate is strongly correlated with stunting (r= 0.71). Water access is correlated with stunting (r= -0.33), while sanitation access is correlated with wasting (r= -0.31). Of course, these are only simple (pair-wise) correlations and may be deceptive. For example, if one regresses the stunting rates in Table 3 on both GDP and the poverty rate, only the latter is statistically significant.[57] In other words, the negative correlation between stunting incidence and GDP is due to an omitted variable bias, given that GDP is (negatively) correlated with poverty incidence, which is a strong covariate of the incidence of stunting.

For a subset of countries the DHS also collected data on adult male anthropometrics which provide an insight into the extent of intra-household inequality. Table 6 provides summary statistics on the incidence of undernutrition for women and children stratified according to whether the male head of household is underweight or not. We see that the incidence of undernutrition among women and children is lower when the male head is adequately nourished. However, substantial inequality in nutritional status is also evident, and the gender inequality goes in both directions. The majority of

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[57] Using our estimates from Tables 2 and 4 to regress the log of the stunting rate on the log of GDP plus the log of the poverty rate, it is readily verified that the regression coefficients are -0.09 (s.e.= 0.06) and 0.42 (s.e.= 0.12) respectively.

women in households where the male head is underweight are not undernourished, and there is a high incidence of undernutrition among women and children in households where the male head is not underweight. Table 6 also gives (in parentheses) the proportions of undernourished women and children found in the two groups of households, identified by whether the male head is underweight or not. (Note that the proportions sum to unity horizontally.) We see that the bulk of underweight women (74%) are found in households where the male head is not underweight and similarly for stunted (80%) and wasted (53%) children.

When we say that a household is "wealth-poor" we are referring to the DHS wealth index within a given country. The wealth index is constructed by taking variables relating to a household's assets (including consumer durables) and amenities, including materials used for housing construction and its access to water and sanitation. These variables are then aggregated into an index using factor-analytic methods, with the wealth index being identified as the first principal component of the data. The DHS wealth index comes as a z-score, i.e., standardized with mean zero and standard deviation of unity. So the index is country specific—not intended to be comparable across countries.

We focus on the poorest 20% and 40% of households based on the wealth index. These are arbitrary choices, although the 40% figure does coincide fairly closely with the overall poverty rate found for SSA using the World Bank's international line.[58] The 20% figure allows us to focus more on the lower part of the wealth distribution. We also provide key results for the full range of the distribution.

It should not be forgotten that the DHS wealth index is a proxy, not a direct measure of wealth. The index focuses on durable and productive asset wealth rather than labor or education wealth, arguably the main assets of many among the poor. When compared to the results of a full-blown

[58] 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).

consumption survey, the DHS index will undoubtedly count as poor some who are not (often called "inclusion errors") and count as non-poor some of those who are in fact poor ("exclusion errors"). In practice, policy makers targeting poor households almost never have access to accurate measures of wealth or consumption for the population as a whole, and must rely instead on a relatively small number of indicators, such as those embodied in the DHS index. Nonetheless, we also conduct the analysis using household consumption per capita for the sub-set of countries for which this is feasible. Surveys that contain detailed household consumption data as well as anthropometrics for women and children are not common, but some do exist including within the LSMS (specifically the LSMS Integrated Surveys on Agriculture) as listed in Table 1.[59] The consumption variable is spatially deflated and expressed in per capita terms.

In an attempt to test whether controlling for additional information, including education and labor assets, enhances predictive power, we draw on household and individual covariates from both surveys. Variables based on the consumption surveys are constructed to be as similar as possible to those used in the DHS data.

The statistical addendum provides summary statistics for the wealth index and other key variables that are typically included in the index or are standard in proxy-means-testing for each country. Descriptive statistics for the variables from the consumption surveys are also shown in the addendum. Overall, means match reasonably well between the two datasets, though with some differences among the asset variables.

[59] Only the consumption survey from Ghana is not one of the Integrated Surveys on Agriculture within the LSMS.

2.5 Individual outcomes and household wealth

Figure 1 plots the incidence of the three anthropometric indices of undernutrition against percentiles of the household wealth-index distribution. For women, we plot incidence for all women 15 to 49 years of age, and for women 20 to 49 years of age, given that younger women typically have a lower BMI. The wealth effect—whereby nutritional status improves with a higher DHS wealth index—is generally evident. However, aside from child stunting, the wealth effect is clearly weak in most countries. The incidence of being underweight is slightly higher for younger women, although the relationship with household wealth is very similar. Child wasting in some countries shows little or no sign of the wealth effect (notably Gabon, Gambia, Senegal, Sierra Leone and Swaziland). Figure 2 gives the corresponding graphs using household consumption per capita. Similar comments apply.

The overall strength of the household wealth effect for each country can be assessed by regressing the standardized values for nutritional status (that is, the z-score for women's BMI and height-for-age and weight-for-height z-scores for children) on the wealth index, which (as noted) is also a z-score. The regression coefficient gives the number of standard deviations of the nutritional indicator attributed to a one standard deviation increase in wealth. Table 7 gives results using the DHS, and also the analogous results using standardized consumption z-scores from the LSMS. Although the estimated wealth effects are statistically significant in almost all cases (the exceptions are for child wasting in a few countries), the coefficients appear to be generally quite low; for women's BMI the mean regression coefficient is 0.26, while it is 0.29 for the height-for-age z-score and only 0.09 for weight-for-height. Even for the countries where the wealth effect on child stunting is highest (Burundi, Cameroon and Nigeria), a one standard deviation increase in wealth is only associated with a 0.5 standard deviation increase in the incidence of child stunting. And for about half the countries, the wealth effect on stunting is less than 0.3 standard deviations.

However, these results cannot tell us much about the efficacy of household wealth in predicting the incidence of undernourished individuals. Low wealth effects such as evident in Table 7 need not imply that the incidence of undernutrition is unresponsive to income or wealth differences (as demonstrated in Ravallion, 1990). Also, as shown in Section 3, even if household wealth and individual nutritional status are correlated it does not follow that a large proportion of undernourished individuals will be found in the lower ends of the wealth distribution.

Figure 3 gives the cumulative share of undernourished individuals by cumulative household wealth percentile ranked from the poorest up, i.e., the concentration curves. The greater the degree of concavity (meaning that the concentration curve is further above the 45-degree line) the more undernourished individuals tend to be concentrated in the poorer strata of household wealth. Similarly, Figure 4 displays the concentration curves using household consumption per person as the ranking variable.

We see in Figure 3 that there is marked concavity for some countries, notably Cameroon (for all three indicators), Congo, Gabon and Ghana (for stunting), Gabon, Kenya, Uganda, Zambia and Zimbabwe (for underweight women). However, in most cases the curves tend to be fairly close to the diagonal line. The curve for underweight women tends to be above that for children in about half the countries, though otherwise there is little sign of a clear ranking of the three indicators.

For the rest of this discussion we focus on the points on the concentration curves corresponding to the poorest 20% and 40% of the household wealth index. Table 8 presents the proportion of undernourished women and children who fall into the bottom 20 and 40 percent. Given the wealth effect on nutritional status, the values for underweight women and stunted children are generally bounded below by $\Pr(w<1)$ (either 0.2 or 0.4). The only exceptions are for child wasting in Gambia, Senegal, Sierra Leone and Swaziland, where the wealth effect is not evident (Figure 1).

What is striking about the results in Table 8 is how close the conditional probabilities are to $\Pr(w<1)$. For 20 of the 30 countries less than 30% of underweight women are found in the poorest 20% of households. This is true for 25 and 26 countries with regard to stunted and wasted children (respectively). On average, roughly three-quarters of underweight women and undernourished children are not found in the poorest 20% of households when judged by household wealth. And about half of underweight women and under-nourished children are not found in the poorest 40% of households.

The countries with a higher percentage of undernourished women in the poorest strata of households tend to also have a higher proportion of wasted children in that group; the correlation coefficients are 0.50 and 0.41 for the poorest 20% and 40% respectively. However, this is not the case for stunted children; the corresponding correlation coefficients are -0.01 and 0.07. There is only one country (Cameroon) where more than 30% of individuals are found in the poorest 20% for all three nutritional indicators.

Table 9 provides the same statistics using the consumption indicator, with very similar results. Overall, about two thirds of undernourished women are not found in the poorest 20% of households based on consumption per person, while about half of them are not found in the poorest 40%. For children, we find that about three-quarters of stunted children are not found in the poorest 20%, and similarly for wasted children. More than half of wasted and stunted children are not found in the poorest 40%.

On combining Tables 2 and 8, we can use equation (2) to infer the joint probabilities of being both undernourished and wealth-poor, $\Pr(n<1, w<1)$. The empirical values for the DHS data are given in Table 10. For underweight women and the poorest 20%, the joint probability is under 0.04 for 22 countries. The mean joint probability of a woman being underweight and living in the poorest 20% of households is only 0.03, rising to 0.06 for the poorest 40%. For child wasting the probabilities are even lower than for underweight women, at under 0.02 for two thirds of all countries. The joint probabilities are higher for stunting, with a mean of 0.08 and 0.16 for the poorest 20% and 40%, respectively. While for child stunting the probabilities span a wider range, it remains that all but two are under 0.1 for the poorest 20%.

As expected, the joint probabilities tend to be positively correlated with the marginals; the bottom row of Table 10 gives the correlation coefficients. The table also gives the OLS elasticities across countries (regression coefficients of the log joint probability on the log marginal probabilities). The elasticities are all less than unity. So a higher rate of undernutrition should reduce the conditional probability. On balance, we find that countries with a higher overall incidence of women's undernutrition or a higher incidence of child undernutrition (whether stunting or wasting) tend to have a higher share of these disadvantageous outcomes among the "non-poor" based on wealth. Table 11 shows the correlations between the conditional probabilities. For women's undernutrition, the correlation coefficient between the share of undernourished women in the poorest 20% of households and the overall incidence of underweight women is -0.31, while for the poorest 40% it is -0.22. For child stunting the corresponding correlations are -0.47 and -0.56, while for wasting they are -0.24 and -0.26. However, not all of these correlations can be considered statistically significant at a reasonable level. The correlations are only significant at the 5% level for the share of underweight women in the poorest 20% and for stunting. Figures 5, 6 and 7 plot the values from Tables 2 and 8 for the incidence of underweight women, stunting and wasting respectively, highlighting the negative relationship between the joint and marginal probabilities.

These results suggest that when relatively few women or children are undernourished in a country one tends to find them more concentrated in relatively poorer households. Conversely, when there are many undernourished women and children one tends to find them more widely spread across the household wealth distribution. From a policy perspective, these results suggest that targeting relatively poor households will tend to work less well in reaching vulnerable women and children in countries where the overall problem of undernutrition is greater.[60]

We examine the correlations with three variables, GDP per capita, the female literacy rate (Table 2) and the wealth-index effect (Table 7). We can think of these as shift parameters of the joint probability density of wealth and nutrition. GDP and the FLR are of obvious interest. The wealth effect is less obvious. In this context, the wealth-index effect can be interpreted as a measure of the extent of nutritional inequality by wealth, and the expectation is that a steeper wealth effect would be associated with a greater concentration of undernutrition in wealth-poor households.

Table 11 also gives the correlation coefficients among the conditional probabilities as well as those with the other social and economic indicators from Table 4. The conditional probabilities are positively correlated with the relevant wealth effects. For underweight women we find that r= 0.64 and 0.71 for 20% and 40% respectively. For stunting, the corresponding correlations with the wealth effects for height-for-age are 0.39 and 0.44, while for wasting they are 0.47 and 0.64. The positive correlation of the conditional probabilities with the wealth effects is also found on using regressions to control for the other summary statistics in Table 4, and these partial correlations are statistically significant in most cases.[61]

We find that the shares of stunted children found in wealth-poor households are quite strongly positively correlated with GDP per capita (r=0.77 for the poorest 20% and r=0.67 for 40%), but this is not the case for underweight women (r=-0.01 and r=-0.17 respectively) or wasted children (r=-0.20 and

[60] This is also evident in the data for stunting in Africa assembled by Bredenkamp et al. (2014) (see the Africa data points in their Figure 1), although across all developing countries Bredenkamp et al. find that inequalities in stunting are greater in countries where stunting is more prevalent. Evidently Africa is different in this respect, though the reason is unclear.
[61] For the shares of underweight women and stunted children in the poorest 20% the partial correlations are only significant at about the 10% level, while they are significant at the 5% level in all other cases.

r=-0.21 respectively). The conditional probabilities are all negatively correlated with the poverty rate; as one would expect, when a higher share of the population is absolutely poor (by a fixed international line) the incidence of undernutrition tends to be spread more widely. However, the correlation coefficients are only significant for child stunting. All six measures of the shares of nutritionally vulnerable women and children are positively correlated with the female literacy rate, though not all are statistically significant; r=0.31 and 0.30 for underweight women and the poorest 20% and 40% respectively, while r=0.42 and 0.34 for stunted children and r=0.20 and 0.15 for wasted children. There are no significant correlations with access to water and sanitation. Nor did the regressions reveal any sign of significant partial correlations with water and sanitation, holding constant either the marginal probability, the wealth-index effect or the combination of the other non-nutritional variables in Table 4. This suggests that other factors besides the health environment may well be playing a more important role, including intra-household inequalities.

2.6 Augmented regressions

Introducing other household-level factors may enhance power for predicting individual outcomes. There may also be a problem with the weights used in constructing the wealth index; for example, the index may not adequately adjust for economies of scale in consumption. Finally, adding basic individual-level variables such as age and marital status for women may enhance targeting capability.[62] To test these conjectures we augment wealth with such household- (and individual-) level variables. The augmented regressions can be expected to perform similarly to the widely-used PMT method based on the predicted values of regressions calibrated to survey data (Section 2).

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[62] Recent research has argued that widows and remarried women often fare poorly when compared to married once women (Anderson and Ray 2016; Djuikom and van de Walle 2017).

To motivate the augmented regressions, we can start by thinking of a simple regression of nutritional outcomes on the wealth index:

$$y_{ijm} = \alpha_m + \beta_m w_{jm} + \varepsilon_{ijm} \tag{4}$$

where y_{ijm} is the nutritional indicator for individual i in household j in country m and w_{jm} is the household wealth index. Call this Model 1. Since the expected value of nutritional status tends to improve with wealth (the aforementioned wealth effect) rankings in terms of the predicted values from these regressions are very similar to those we have seen already. Model 2 augments (4) to contain household-level variables x_{jm}, giving:

$$y_{ijm} = \alpha_m + \beta_m w_{jm} + \gamma_m x_{jm} + \varepsilon_{ijm} \tag{5}$$

The vector x_{jm} includes the separate components of the wealth index (essentially to allow a re-weighting of the index), as well as other household-level variables such as size and composition, and characteristics of the head. Dummies for survey month and region of residence are also entered as controls. Finally, Model 3 adds the observable individual-level variables, z_{ijm}:

$$y_{ijm} = \alpha_m + \beta_m w_{jm} + \gamma_m x_{jm} + \delta_m z_{ijm} + \varepsilon_{ijm} \tag{6}$$

For the incidence of underweight women, the individual-level attributes include the woman's age (BMI tends to increase as women age), education and marital status. For children, age, gender, and characteristics of the child's mother are included. To avoid *ad hoc* functional form assumptions, age and education variables as well as household size are broken into categories each of which is entered as a dummy variable. OLS is used to estimate each model, with standard errors clustered at the PSU. (The Addendum gives the actual regressions.)

As discussed above, in the event that household wealth is simply a poor indicator of nutritional outcomes, we also use household per capita consumption. For the relevant subset of countries we estimate the regressions using household consumption per person (with, as noted in Section 4, some slight variations in the variables included in x_{jm} and z_{ijm}). The results were similar; details are found in the Addendum.

Tables 12 and 13 present the results for Models 2 and 3 for underweight women and undernourished children respectively. The tables give the proportion of undernourished individuals who fall into the poorest 20% and 40% of the distribution of the predicted values based on wealth and (unlike prior tables) the additional covariates. We find that, on average, 32% of underweight women are found in the poorest 20% based on the predicted values from Model 2 (Table 12), as compared to 28% using only the household wealth index (Table 8). Focusing instead on the poorest 40%, the proportion rises to 56% using Model 2, as compared to 51% using wealth alone. Adding the individual variables (Model 3) we now find that (on average) 37% of underweight women are found in the poorest 20% in terms of the predicted values, rising to 61% for the poorest 40%. Similar improvements are evident for both stunting and wasting in children (comparing Tables 13 and 8).

However, it is clear that these augmented regressions still do a poor job at identifying undernourished individuals within households. While the predictive power is improved, it is not enough to change our conclusion that targeting based on the available household poverty data misses a large share of undernourished women and children.

2.7 Conclusions

There are multiple constraints on effective policy interventions in practice. Here we have focused on a key informational constraint, and asked whether household poverty might provide a reliable guide for policy efforts trying to reach deprived individuals, as indicated by anthropometric measures of undernutrition, recognizing that poverty is an individual characteristic. We do not claim that information is the only constraint. Even if undernourished women and children are almost solely found in wealth- or consumption-poor households, other factors such as the local health environment can play an important role in determining policy effectiveness.

We have focused on just one dimension of individual deprivation. Individual welfare clearly depends on more than nutritional status, and we cannot rule out the possibility that household-level data are more revealing for other non-nutrition dimensions. That said, undernutrition is an undeniably important dimension of individual poverty and it has long played a central role in the measurement of poverty using aggregate household data. This dimension of welfare is also emphasized by policy makers concerned with reducing both current and longer-term poverty. The mounting evidence on the longer-term costs of stunting in young children adds force to that emphasis.

A great deal has been learnt about the socioeconomic differentials in individual health and nutrition from micro data, typically using cross-tabulations or regressions. This knowledge is valuable. However, there is a risk that the differentials in mean attainments often found between rich and poor households lead policy makers to be overly optimistic about the scope for reaching vulnerable individuals using only household-level data. Standard poverty data make *ad hoc* assumptions about equality within households. Persistent effects of intra-household inequality on health and nutrition may not be evident in these measures. Just how adequate household-level data are for the policy purpose of reaching vulnerable women and children has been unclear.

To help improve our knowledge about this constraint on policy, the paper has provided a comprehensive study for 30 countries in Sub-Saharan Africa. We find a reasonably robust household-wealth effect on individual undernutrition indicators for women and children. Nonetheless, on aggregating across the 30 countries studied here, about three-quarters of underweight women and under-nourished children are not found in the poorest 20% of households when judged by the household wealth index in the Demographic and Health Surveys. A similar pattern is found in the available household surveys that allow a comparison of individual nutritional measures with an estimate of the household's consumption per person, which is clearly the most widely used welfare metric in measuring poverty in developing countries. Adding other household variables—interpreted as either a re-weighting of the DHS wealth index or as supplementary variables—improves the performance of household data in this respect, but we still find that a large share of undernourished individuals are not among those predicted to be undernourished based on household variables. It is clear from this study that to have any hope of reaching undernourished women and children, policy interventions in this setting will either require much more individualized intra-household information or they will need to be nearly-universal in coverage.

This dispersion of undernourished individuals across the distributions of household wealth and consumption entails that countries with a higher overall incidence of undernutrition tend to be countries where a larger share of the undernourished are found in non-poor families. This suggests that the need for broad coverage in social policies (rather than policies finely targeted to poor households) is especially great in countries with a high incidence of undernutrition. Rather than folding nutrition schemes into household-targeted antipoverty programs in such countries, emphasis should be given to nutritional interventions with near universal coverage, such as comprehensive school feeding (with explicit nutrition supplementation), maternal health care and universal sanitation services.

In addition to documenting the limitations of relying on household poverty data to reach nutritionally deprived individuals, we throw some light on why those limitations are so severe. For the subset of countries for which we also know adult male BMI, we have shown that the extent of intra-household inequality entails that the bulk of underweight women and undernourished children are found in households where the male head appears to be adequately nourished. In exploring the cross-country patterns we find that richer countries (in terms of GDP per capita) within Africa tend to have child stunting more concentrated among the wealth-poor, suggesting greater scope in those countries for targeting wealth-poor households as a means of reaching children with longer-term nutritional deficiencies. But this is not so for child wasting. In countries with a higher female literacy rate one tends to find a greater concentration of underweight women in poor wealth strata. By contrast, female literacy has little power for predicting whether children's undernutrition is more concentrated among the wealth poor. There is no sign that countries with lower average access to improved water and sanitation tend to have undernourished women and children more concentrated among the wealth poor; while there is little doubt that improved water and sanitation makes for better nourished people, intra-household inequalities appear to be a more plausible explanation for our main findings on the relationship with household wealth than these aspects of the health environment. In all cases, the size of the wealth effect—how much undernutrition falls as the wealth index rises—is a significant predictor of how effectively one can expect to identify nutritionally-disadvantaged individuals by targeting poor households. However, as we have also emphasized, it is better to focus directly on the relevant conditional probabilities for this purpose, rather than the wealth effect.

Chapter 3: A Poor Means Test? Econometric Targeting in Africa[63]

3.1 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.[64] 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

[63] This chapter is coauthored with Martin Raallion and Dominique van de Walle. Martin Ravallion is with the Department of Economics at Georgetown University and Dominique van de Walle is with the Development Research Group at the World Bank. For their comments the authors thank Arthur Alik-Lagrange, Kathleen Beegle, Mary Ann Bronson, Raphael Calel, Phillippe Leite, Essama Nssah, Mead Over, Mark Schreiner, Don Sillers, Adam Wagstaff and seminar participants at Georgetown University and the World Bank. The authors are grateful to the World Bank's Strategic Research Program for funding assistance for this research. These are the views of the authors, and need not reflect those of their employers, including the World Bank or its member countries.

[64] This idea appears to have emerged in social policy making in Chile in the 1980s (Grosh, 1994, Ch.5). Grosh et al. (2008) provides a useful overview of PMT and other targeting methods found in practice in developing countries, with details on many examples.

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.[65] Practitioners have turned to more sophisticated statistical methods in an effort to further improve targeting accuracy. This has come to be known as a *proxy means test* (PMT).[66]

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.[67] To distinguish it from other methods of means testing, we will use the term "econometric targeting" to refer to any PMT based on a regression model. An influential early contribution by Grosh (1994) compared numerous social programs in Latin America and concluded that this class of methods produced the best targeting outcomes, measured in terms of reducing inclusion errors, whereby a nonpoor person is counted as poor. Various versions of econometric targeting have since been proposed, and the method has been widely implemented in developing countries.[68]

[65] 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.
[66] 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."
[67] For a critical review of the methods used see Ravallion (2016, Part 2).
[68] Useful overviews can be found in Mills et al. (2015) and USAID's website on Poverty Assessment Tools. Note that USAID does not endorse these tools for targeting purposes.

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

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

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

Another critique relates to the goals of social protection policies, which can be thought of as involving both protection from uninsured risks as well as promotion from poverty over the longer-term.[69] 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

[69] On this distinction and the implications for assessing social protection policies see Ravallion et al. (1995).

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, standard regression-based calibration of the PMT score will tend to work less well toward the extremes of the distribution of household consumption. By its design, a standard regression line passes through the means of the data. The residuals will be positively correlated with the dependent variable (more so the higher the variance of the residuals given exogenous regressors).[70] One can expect the method to have a tendency to overestimate living standards for the poorest and underestimate them for the richest, though the degree to which this is problematic for targeting accuracy is unclear. Indeed, it is theoretically possible that the PMT method predicts that nobody lives below a poverty line for which even a sizable share of the population is deemed to be poor based on observed consumptions. Another possibility is that the variables used are not sufficiently good proxies for household consumption. In other words, that there is an information problem.

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

[70] 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).

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.[71] 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).[72] For a subset of these countries we also have panel data.

In advocating and assessing PMT, social policy making in developing countries has often emphasized the need to avoid the "leakage" of benefits to the non-poor, and to assure broad coverage of the poor. Following the literature, one can term failures with regard to these two aspects of targeting as the aforementioned "errors of inclusion" (i.e., counting someone as poor who is not) and "errors of exclusion" (i.e., counting someone as non-poor who is in fact poor).[73] The difference is important when deciding how much to spend on a program. Inclusion errors are generally costly to the public budget while exclusion errors save public money. Governments and international financial institutions concerned about the fiscal cost of social policies have thus put greater emphasis on avoiding inclusion errors as a means of cutting the cost to the government without hurting poor people.[74] Some observers have questioned this prioritization, arguing that exclusion errors should get higher weight when the

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

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

[73] 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).

[74] 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).

policy objective is to minimize poverty.[75] 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.[76] The methods appear to vary considerably across the studies to date, such as in how many variables are used in the PMT, how targeting performance is assessed, and what poverty cutoff point is used. However, documentation is rarely ideal, often leaving the reader to guess what has been done. This makes it difficult to compare results. We provide similar tests on a consistent basis.

We go further than past work in a number of other respects. We consider alternative econometric methods for calibrating the PMT scores. These include methods that recognize explicitly that the goal of PMT is poverty reduction rather than obtaining unbiased estimates of conditional means. We also simulate stylized policies to see how well econometric targeting works. Here we consider simpler alternatives to PMT that have a long history, going back to the state-contingent transfers that were introduced under England's Poor Laws, and the various proposals that have been made over the last 200 years for a "basic-income scheme."[77] Additionally, we compare PMT to optimally differentiated transfers based on the same information set. In considering these options, we focus directly on the impacts on poverty rather than looking solely at measures of targeting performance. Here we take the view that "better targeting" should not be seen as an end in itself but rather as a possible means of assuring a greater impact on poverty.

The panel data that are available for a subset of countries help us address concerns about measurement errors in consumption. To some degree, what are called "targeting errors" are likely to be

[75] See Cornia and Stewart (1995), Smolensky et al. (1995) and Ravallion (2009).
[76] See Grosh and Baker (1995) (Jamaica, Bolivia, Peru), Ahmed and Bouis (2002) (Arab Republic of Egypt), Narayan and Yoshida (2005) (Sri Lanka), Sharif (2009) (Bangladesh), Stoeffler et al. (2015) (Cameroon), Pop (2015) (Ghana) and Cnobloch et al. (2015) (Malawi).
[77] On the history of these policy options see Ravallion (2016, Part 1).

measurement errors (Ravallion 2008). By using the panel data to calculate time-mean consumption we can at least partly reduce the effect of measurement error, as a robustness test of our main findings.

Another departure from past work is that we allow for likely lags in implementation; past assessments have ignored the fact that PMT invariably entails such lags, given that the score must be set in advance of implementation. There are lags between the survey year and the release of the PMT formula, and further lags to implementation.[78] We can expect a degree of churning, with households moving in and out of poverty.[79] So implementation lags are likely to constrain the performance of econometric targeting in identifying the currently poor. We exploit the panel nature of our data for a subset of countries to explicitly introduce lags.

There are a number of issues that we do not take up. One of these is whether household consumption obtained from a survey is an adequate welfare indicator. The methods of econometric targeting studied here make that assumption, and we accept it for the purpose of evaluating the performance of these methods. Another issue not taken up here is how well a low level of household consumption identifies deprived individuals; Brown et al. (2016) take up this issue in the context of attempts to reach undernourished women and children. 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

[78] 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.

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

requirements ("workfare") or community-based targeting in which local communities are engaged directly in deciding who is poor and who is not.[80] Nor do we consider the (economic, social and political) costs of targeting, which have received some attention in the literature.[81] For example, we do not discuss behavioral responses, social stigmas, or implications for social cohesion and political support for poverty programs.[82]

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 nonpoor comes with seemingly weak coverage of poor people—a high rate of exclusion errors. In other words, the method helps exclude the poor as well as the non-poor. The paper finds that econometric targeting typically provides at most modest gains in the poverty impacts over other policy-relevant alternatives. Indeed, in a number of cases and depending on the country and the nature of its poverty profile, simpler state-contingent targeting methods or even a "basic-income scheme" (in which everyone is covered) dominate in certain policy-relevant cases, such as when one allows for plausible lags in PMT implementation. However, none of these methods can be considered to perform especially well. Prevailing methods do not reliably reach the poorest. The costs of each method in practice may then be decisive in the choice.

The following section describes the PMT method that we assess, while Section 3 describes the measures we use in assessing econometric targeting. Section 4 studies the basic version of PMT, while

[80] 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.
[81] See the discussions in van de Walle (1998), Gelbach and Pritchett (2000) and Ravallion (2016, Ch. 10).
[82] 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 5 turns to various extensions and revisions to that version. For stylized transfer programs, Section 6 compares the poverty impacts of econometric targeting to those of less methodologically sophisticated methods, including un-targeted (universal) transfers and simple demographic "scorecard" methods. Section 7 presents our results for (informationally-feasible) differentiated transfers, including optimal transfer schemes for poverty reduction with a given budget but limited information. Section 8 uses the panel surveys to introduce lags in implementation. Section 9 offers some cross-country comparisons of the performance of econometric targeting; here we ask how much the impacts of PMT on poverty for a given budget are explicable in terms of the alternative targeting measures and the predictive ability of the PMT regressions. Section 10 concludes.

3.2 Econometric targeting

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

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

The PMT score is then based on:

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

The most common method in practice for estimating α_{jt} and β_{jt} in (1) 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 $\hat{\alpha}_{jt}$ and $\hat{\beta}_{jt}$ to minimize $\sum \hat{\varepsilon}^2_{ijt}$ for each *j, t*). We also considered the option of using a binary indicator for whether a household's actual consumption falls below the poverty line as the dependent variable (equal to one if a household is poor, and zero otherwise). We tried this for both OLS (giving a linear probability model) and a Probit. However, we found that targeting errors were substantially higher with a binary dependent variable in all cases. So we confine attention to the continuous dependent variable in the rest of this paper.

Another option to OLS in estimating equation (1) is to try to better tailor the estimator to the specific policy problem, in this case poverty reduction. Two ways of doing this can be suggested. The first is the quantile regression method of Koenker and Bassett (1978). This is more robust to outliers than OLS, and (importantly) the method can be tailored to the problem at hand in that the quantile can be set at the overall poverty rate.[83] In other words, we calibrate the PMT score to how that specific quantile in the distribution of log consumption, given the covariates, changes with those covariates. The second method entails placing higher weight on the squared errors of poorer people, giving "poverty-weighted least-squares" (PLS). Among the various weighting schemes that might be used, we choose the method proposed by Mapa and Albis (2013), which weights equally all observations below the poverty line but gives zero weight to those above the line. In other words, we run the regression on poor households only. We extend this method by including households somewhat above the line. Once we have the PLS parameter estimates we calculate the revised PMT scores using the actual values of x_{ijt} .

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.

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

There are feasibility constraints associated with the number and nature of the variables used in practice; administrative costs almost certainly rise with the number of variables. There are also incentive constraints, stemming from the scope for manipulation by local agents when there are many variables in the PMT (Niehaus et al. 2013).

The variables used in practice typically cover readily observed living conditions of the household, such as basic consumer durables or assets, demographic variables (size and composition) and attributes of the head.[84] Two important exclusions are notable. First, prices are rarely used and assets are identified in broad categories; clearly, two households can each own a "fridge" but in one case it is 30 years old and works poorly while in the other case it is a fancy new model. Second, an important exclusion is that one cannot use fine geographic effects, such as at the level of the village, since one is constrained to estimating on a sample survey that will typically only cover a sample of villages (typically determined by the first stage of a two-stage sampling design). One does not know the geographic effect for the population, as required for implementing the PMT.[85] However, in one version we include community-level variables that go some way toward addressing this concern.

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

[84] See, for example, the various studies in the compilation by Del Ninno and Mills (2015).
[85] The same limitation is shared by small-area estimation methods ("poverty mapping") as in Elbers et al. (2003).

3.3 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.[86] 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.

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

We focus on three main measures of targeting performance. The first is the Inclusion Error Rate (*IER*), defined by the proportion of those identified as poor who are not. This can be written as:[87]

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

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

Inclusion errors have received much attention in efforts to reduce the budgetary cost of social policies aiming to use transfer payments (in cash or kind) to reduce poverty. Inclusion errors imply a fiscal cost without any direct impact on poverty. For a uniform transfer paid to all those who are deemed

[86] 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.
[87] The indicator function 1(.) takes the value unity when the condition in parentheses is true and zero otherwise.

to be poor, the *IER* gives the share of the transfers going to the non-poor.[88] If everyone is deemed "poor," so the transfer payment is universal, then *IER* is simply one minus the poverty rate.

The *IER* is often normalized by the poverty rate when the latter varies, which we will also do in some cases. The resulting measure has been used extensively—clearly more than any other targeting measure—in comparing the targeting performance of social programs across developing countries.[89] 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.[90]

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.[91] 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.[92] The Exclusion Error Rate can be written as:

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

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

[88] This is what Weisbrod (1970) dubbed "vertical efficiency."
[89] This normalized share of transfers going to the poor was used by Coady et al. (2004a, b) to compare 85 programs across many countries.
[90] 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).
[91] See Ravallion (2009) who finds supportive evidence using data for a large cash transfer program in China.
[92] For example, for the headcount index of poverty one focuses on whether there is exclusion at the poverty line.

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

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

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

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

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

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

The third measure is the Normalized Targeting Differential (*NTD*). In the context of a transfer program, the (ordinary) Targeting Differential (*TD*) is defined as the mean transfer made to the poor less that made to the non-poor.[93] For a uniform transfer paid to all those who are deemed eligible, the *TD* becomes the difference between the proportion of the poor who are predicted to be poor and the proportion of the non-poor who are predicted to be poor. (In the case of a specific antipoverty program it is the difference between the program's coverage rate for the poor and that for the non-poor.) The *NTD* divides this measure by the mean transfer receipt, to make the resulting measure more comparable across countries and programs. For a basic income guarantee, *NTD*=0. When only the poor get help from the program and all of them are covered, the *NTD* reaches its upper bound of 1; when only the non-poor get the program and all of them do, the *NTD* is at its lower bound of -1. For a uniform transfer to all recipients in the amount τ_{jt} we have:

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

where H_{jt} is the 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 headcount index obtained based on predicted consumptions.

Another concept of targeting errors occasionally found in the literature makes the distinction between "Type 1" (*T*1) and "Type 2" (*T*2) errors of targeting (borrowing the terms from statistics).[94] 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:[95]

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93 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).

94 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.

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

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

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

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

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

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

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

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

While *H* is (by far) the most popular measure in practice, its limitations are widely appreciated, notably that the measure does not reflect changes in living standards below the poverty line. We also consider two "higher-order" measures. The first is the poverty gap index, as given by the mean distance below the poverty line as a proportion of the line where the mean is taken over the whole population, counting those above the line as having zero gap.[96] The poverty gap index can be written as:

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

We also make use of a distribution-sensitive measure, namely the Watts index proposed by Watts (1968) given by the mean proportionate poverty gap (counting the non-poor as having zero gap). This measure penalizes inequality among the poor, by putting higher weight on poorer people.[97] The Watts index can be written as:

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

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. Quite generally one can think of the informationally-feasible transfers as a function of *m* observed *x*'s. The policy maker only observes the covariates *x* for each person; it is not known who is poor and who is not. However, the policy maker has a survey with

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

much more information available for a sample. The problem is to choose the parameters of a score for assigning the real-world transfers based on the x's, as given by:

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

(Here $x_{ijt}^{0} = 1$ so that γ_{jt}^{0} is the intercept—the transfer received by someone with $x_{ijt}^{k} = 0$ for k=1,…,m.) In one version transfers are linear in the x's, i.e., $\vartheta = 1$. We call this the linear optimization. We also estimate a nonlinear version with $\vartheta = 2$, which introduces squared terms and interaction effects among the x's.[98] The choice of the score parameters γ_{jt}^{k} is made to minimize the Watts index in the sample survey data:

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

The choice is constrained by the budget:

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

We solve this problem numerically.[99] One start value we use for the optimization is the uniform case obtained by setting $\gamma_{jt}^{0} = B_{jt}$ and $\gamma_{ijt}^{k} = 0$ for k=1,…,m. Other start values are tested. When there are multiple local optima the solution for the parameters γ_{jt}^{k} that gives the lowest value of the poverty measure is chosen.

The data for implementing these measures come from the World Bank's well-known LSMS.[100] Table 2 lists the countries, years of survey, and numbers of households surveyed. In keeping with the

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[98] Glewwe (1992) recommended this in his formulation of the optimal targeting problem.
[99] We use the "fmincon" program in Matlab.
[100] 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.

bulk of the literature, our dependent variable is log total consumption per capita.[101] Consumption is measured in local currency units. Spatially deflated consumption values are available for all countries except Burkina Faso.[102] 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.[103] The 20% rate allows us to focus on how well the method does at identifying those who can be considered extremely poor. When comparing the actual values and the PMT scores one can chose to either fix the poverty rate (at 0.2 or 0.4) or fix the poverty line in the consumption space (i.e., fixing $z_{jt} \equiv F_{jt}^{-1}(0.2)$ or $F_{jt}^{-1}(0.4)$). This choice makes a difference and practice varies so we present results for both options.

3.4 Results for Basic Proxy Means Test (PMT)

The "Basic PMT" closely follows our reading of 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. The Statistical Addendum gives the OLS regression results for the Basic PMT. The simple average R^2 is 0.53, with a range from 0.32 (for Ethiopia) to 0.64 (Burkina Faso); Table 3 provides summary statistics for the Basic PMT (as well as for the extended version discussed below). This explanatory power is typical of past

[101] 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.
[102] We use nominal consumption for Burkina Faso.
[103] 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).

studies.[104] We did not try to 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.

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

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

There is considerable variation across countries, with *IER* ranging from 33% to 100%, and *EER* from 55% to 100%. In the country with the lowest coverage rate of the poor implied by PMT, Mali, all

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

poor families are incorrectly identified as non-poor. Unsurprisingly, we also find a tendency for the PMT to do better at correctly identifying poor households when the R^2 in the PMT regression is higher (comparing Tables 5 and 3). However, the proportion of households correctly included is less than half of those who are poor under a poverty line corresponding to *H*=0.2.

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

The finding that the errors tend to be higher using the lower poverty line again suggests that econometric targeting may have difficulty in identifying those who are very poor. A further insight on this is found in Figure 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. In one case, Mali, there are no data points in the bottom left quadrant; only one household is (incorrectly) predicted to be poor (thus giving the result that *IER*=*EER*=1.0 from Table 5). While Mali is exceptional in this respect, the point remains that PMT is missing many of the poorest households in all countries. Figure 2 gives the implied residuals. As expected, these tend to be lower (more negative) for poor people, but it is notable just how much the PMT regression is over-estimating the living standards of the poorest. For the poorest 20% in terms of actual consumption, the mean residual ranges from -0.73 to -0.37, implying that the PMT regressions yield predicted consumptions for the poor between 50% and 100% above their actual consumption.[105] (The fact that consumptions of the poor are overestimated by the PMT regressions at the poverty line, as is evident in Figure 2, echoes our finding above that Basic PMT underestimates the poverty rate.)

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

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

As noted in the introduction, "targeting errors" may reflect to some extent time-varying measurement errors in the cross-sectional data. For those countries with panel data we can address this problem by assessing targeting performance using the time-mean consumption instead of current consumption. This will reduce, though probably not eliminate, any bias due to time-varying measurement errors. The lower panel of Table 5 gives the results. In the majority of cases, the measures

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

of targeting performance improve, although this is less evident for exclusion errors than inclusion errors when using a fixed line. (Ethiopia accounts for about half of the exceptions.) Overall, the results are broadly consistent with the view that measurement errors are playing some role, but the panel data do not overturn our main conclusions about PMT. (Section 8 returns to the panel data, as a means of allowing for implementation lags in PMT.)

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

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

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

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

3.5 Extended PMT

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

As expected, the Extended PMT does better than Basic PMT with respect to targeting errors (Table 9). However, the improvement would have to be judged as modest (comparing Tables 9 and 5).

For example, many more than half of the poorest 20% are still misidentified as non-poor. Table 4 also gives the average proportion of households that are predicted as poor using the poverty line methodology under the extended PMT method. At H=0.2, we see a slight improvement over the Basic PMT, with 11% of the sample predicted to be poor using the poverty line corresponding to H=0.2 (as compared to 8% using Basic PMT). The results are more similar between Basic PMT and Extended PMT for a line corresponding to H=0.4.

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

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

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

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

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

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

3.6 Poverty impacts of stylized transfer schemes using various targeting methods

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

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

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

We also consider counterfactual policies that use categorical targeting rather than PMT. These policies make uniform transfers within a specified category of people, as defined by a "poverty scorecard." Here we consider an especially simple form of demographic scorecard.[106] The first category is the set of persons 65 years or older. The second is any person who is a (female) widow, disabled (where disabled is defined as an illness or condition that significantly impairs a person over the age of 14 and their ability to work or study), or orphaned (defined as any child 14 or younger whose parents have both died or whose whereabouts are unknown). The third is a combination of the first two: a transfer to the elderly, widowed, disabled or orphaned. Note that if a person fits two categories, the score and (hence) transfer is doubled. The fourth transfer is a payment to households with children – whereby up to three children are each allotted a transfer. Finally, the last scheme combines all previous schemes, where children, the elderly, widowed, disabled or orphaned are eligible. (Recall that all stylized schemes considered have the same aggregate budget.)

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

[106] 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.

the greatest impact on poverty. While categorical targeting does not have quite as much impact on poverty as the Basic PMT, it clearly comes close and is simpler and more transparent.

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

3.7 Allowing differentiated transfers

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

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

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

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

3.8 Allowing for lags in PMT implementation

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

We have panel data for a subset of our study countries, namely Ethiopia, Malawi, Nigeria, Tanzania and Uganda. By exploiting the panel data, we can introduce a 1 to 2 year lag in the implementation of PMT. The precise lags are one year for Uganda, and two years for the other countries.[107] In other words, we develop the PMT on the Round 1 survey data and then apply it in Round 2. If anything, our lags appear to be less than found in practice.[108]

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

The targeting errors obtained using Methods 1 and 2 are found in Tables 15 and 16, respectively. Comparing the results in Table 15 (top panel) with Table 5 we see that allowing for lags increases the targeting errors on average.[109] For the lower line, we now find that, on average, about half of those predicted to be poor are not in fact poor based on the survey data (a mean IER of 0.553, as compared to 0.481 from Table 5). Exclusion errors are also affected, though these errors rose less markedly. Using the Extended PMT we also find a substantial increase in the targeting errors, especially for inclusion, when we allow for lags using Method 1. A similar pattern is found for Method 2.

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

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

[108] 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).

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

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

3.9 Predictors of poverty impacts

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

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

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

3.10 Conclusions

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

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

The method we find to generally perform best from the point of view of reducing exclusion errors is a "poverty-quantile regression." This method generates more inclusion errors than prevailing PMT methods, though still less than un-targeted transfers.

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

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

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

Figures and Tables

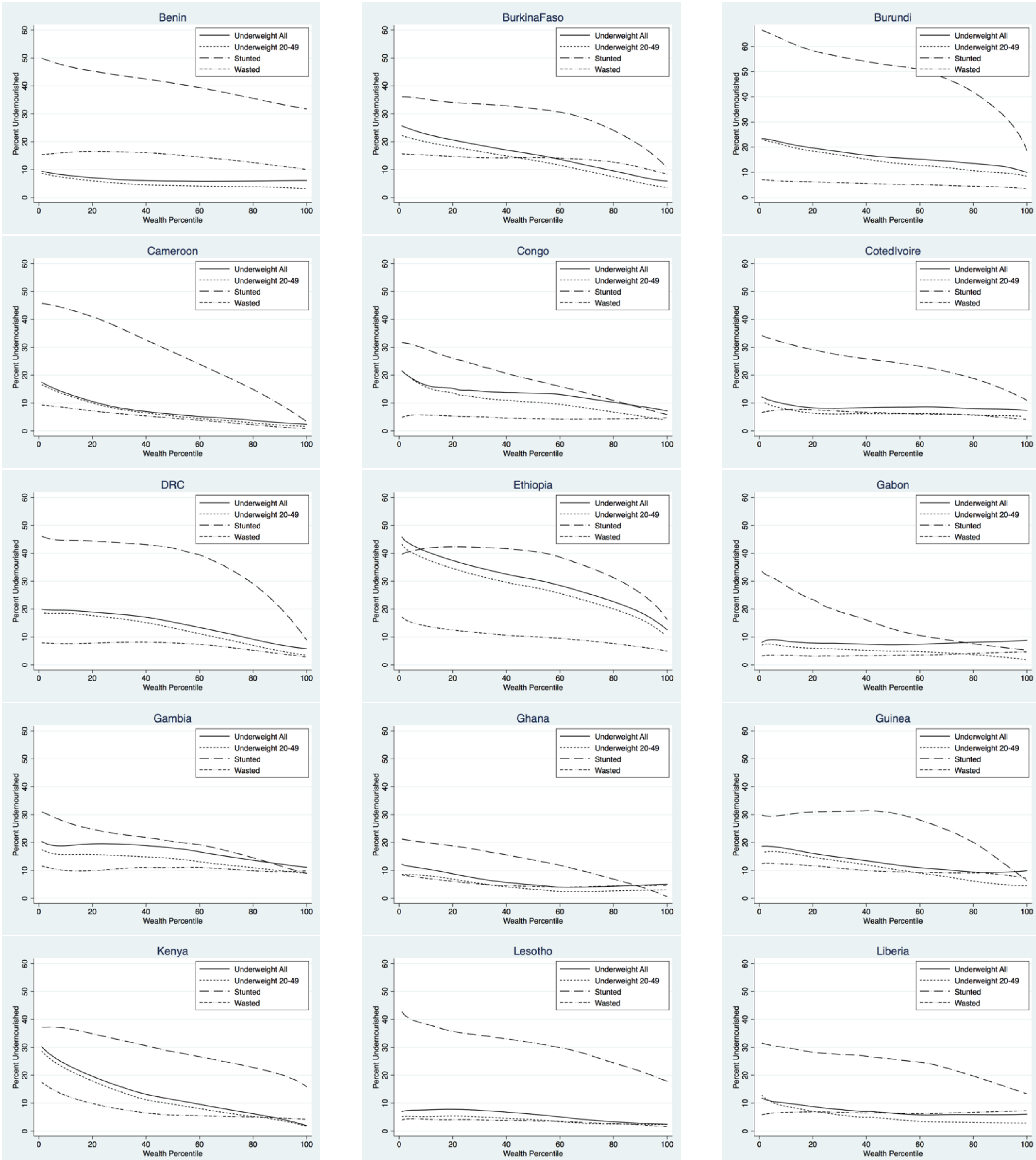

Figure 2.1: Nutritional outcomes and household wealth. The graphs show the proportion of women who are underweight and the proportion of children who are stunted and wasted at each wealth percentile. Data are drawn from DHS. Observations with missing values and pregnant women have been dropped. Women between 15 and 49 years of age are included in the construction of the solid line. Woman between 20 and 49 years of age are included in the construction of the dashed line. Children aged between 0 and 5 are included in the stunted and wasted lines. The household wealth index is used to construct the wealth percentiles. Wealth percentiles are constructed separately for women and children. A lowess regression is used to fit the lines.

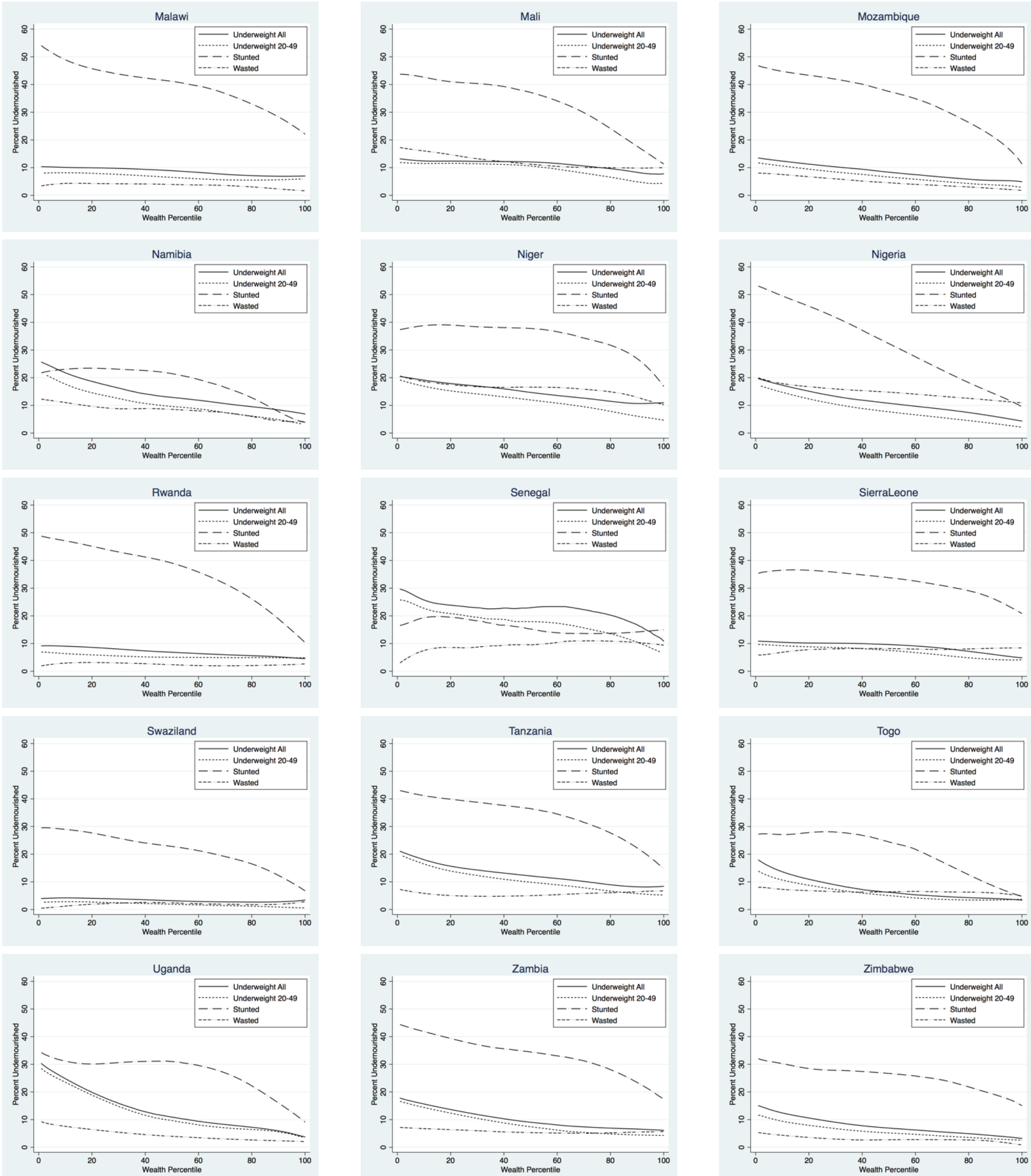

Figure 2.1: (cont.)

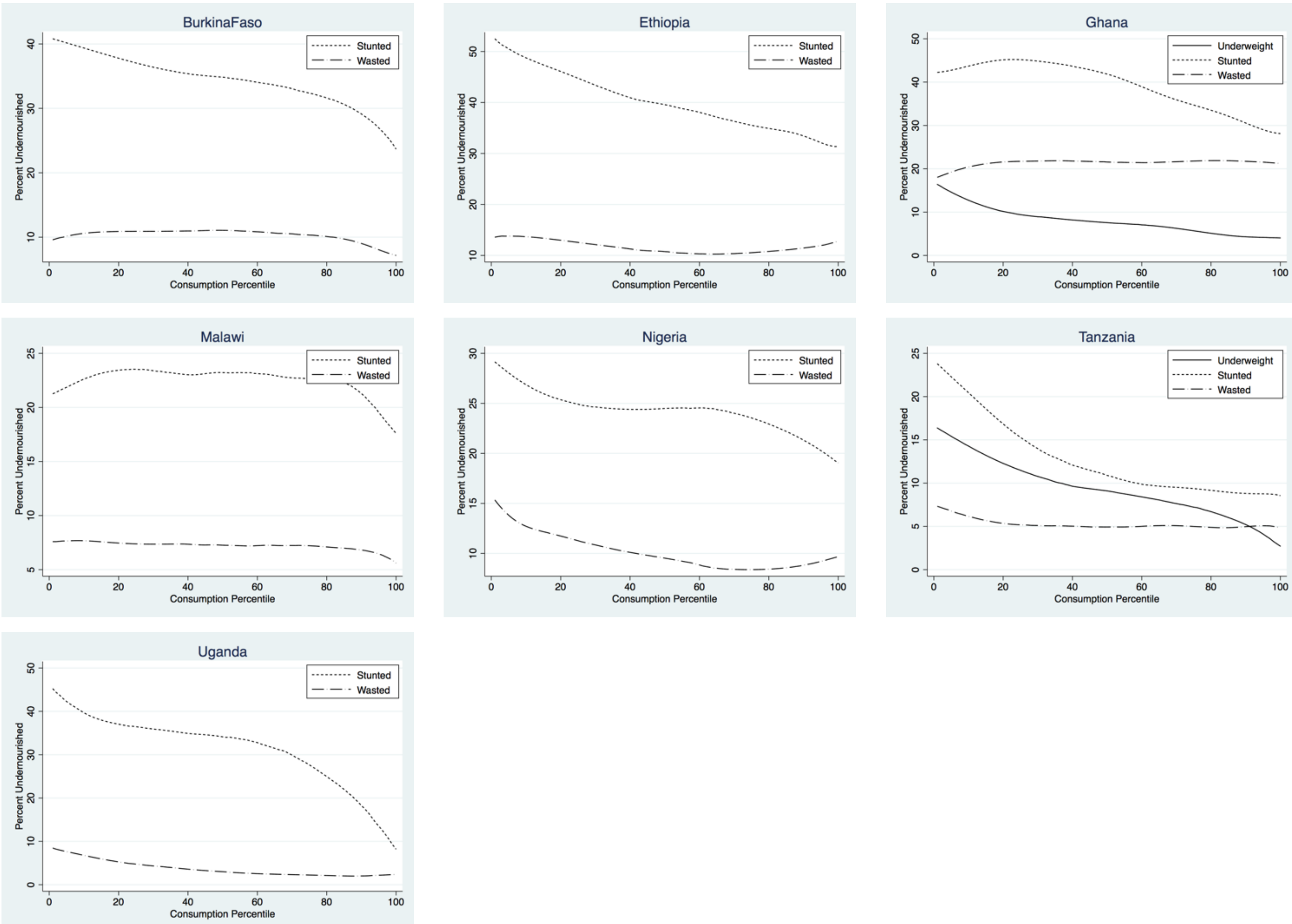

Figure 2.2: Nutritional outcomes and household consumption. The graphs show the proportion of women who are underweight, and children who are stunted and wasted at each wealth percentile. Data are drawn from LSMS surveys. Observations with missing values and pregnant women in Ghana have been dropped. Women between 15 and 49 years of age and children between 0 and 5 years of age are included in the sample. Household consumption, which is spatially deflated and in per capita terms, is used to construct the consumption percentiles. Consumption percentiles are constructed separately for women and children. A lowess regression is used to fit the lines.

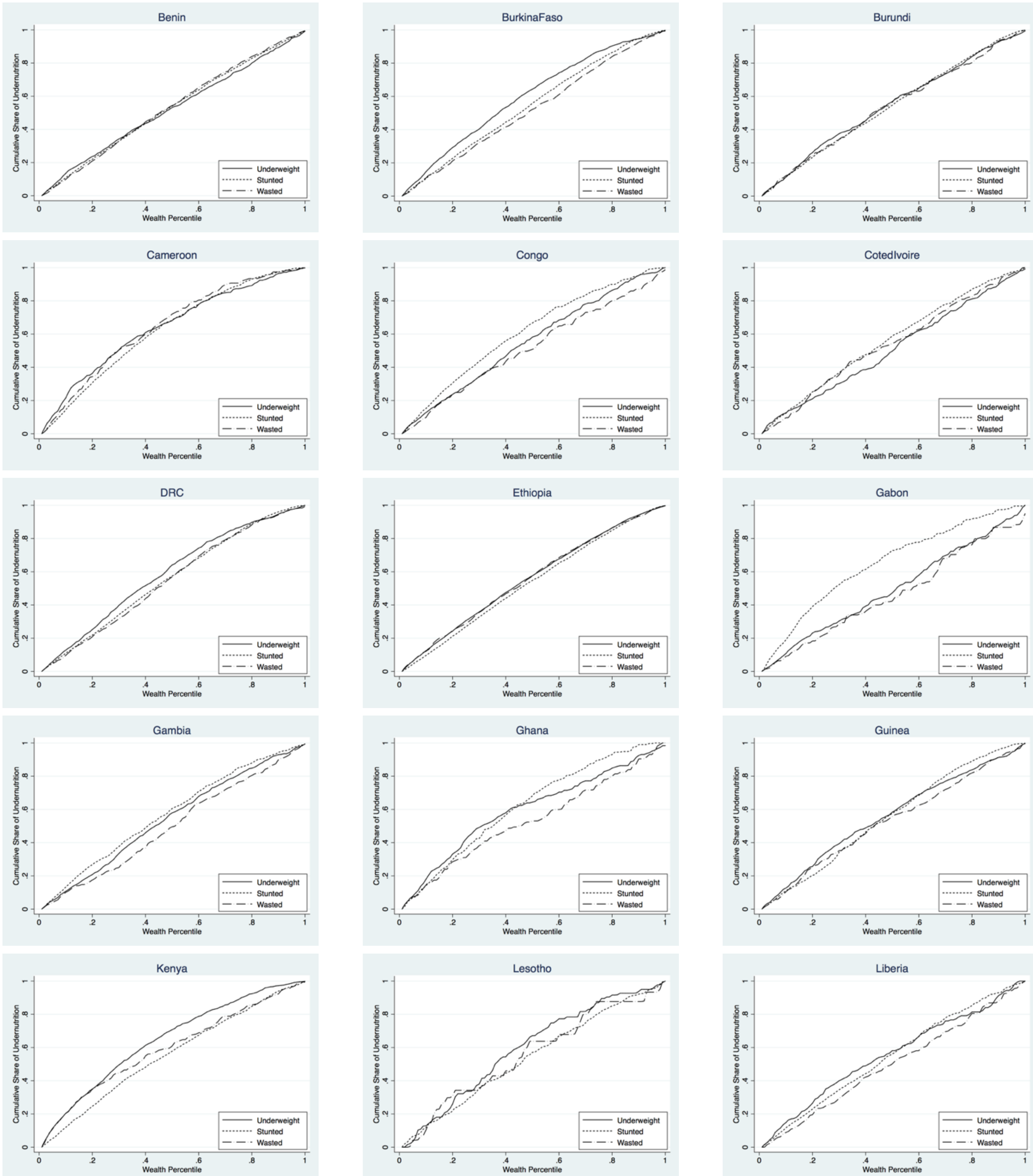

Figure 2.3: Concentration curves for undernutrition and household wealth. The graphs show the concentration curves for cumulative proportion of women who are underweight, and children who are stunted and wasted at each wealth percentile. Data is drawn from the DHS. Observations with missing values and pregnant women have been dropped. Women between 15 and 49 years of age and children between 0 and 5 years of age are included in the sample. The household wealth index is used to construct the wealth percentiles. Wealth percentiles are constructed separately for women and children. The Stata command glcurve is used to construct the lines.

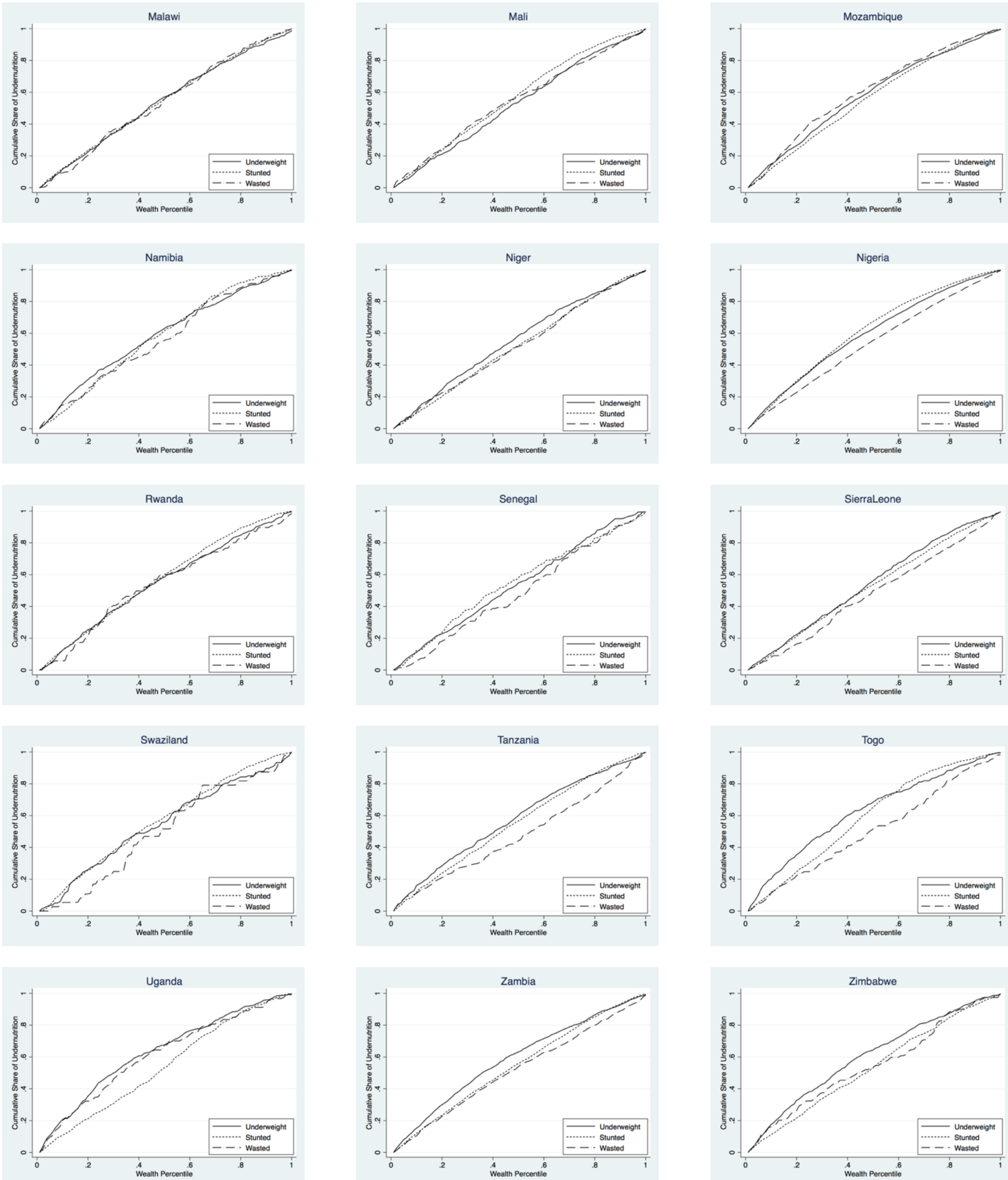

Figure 2.3: (cont.)

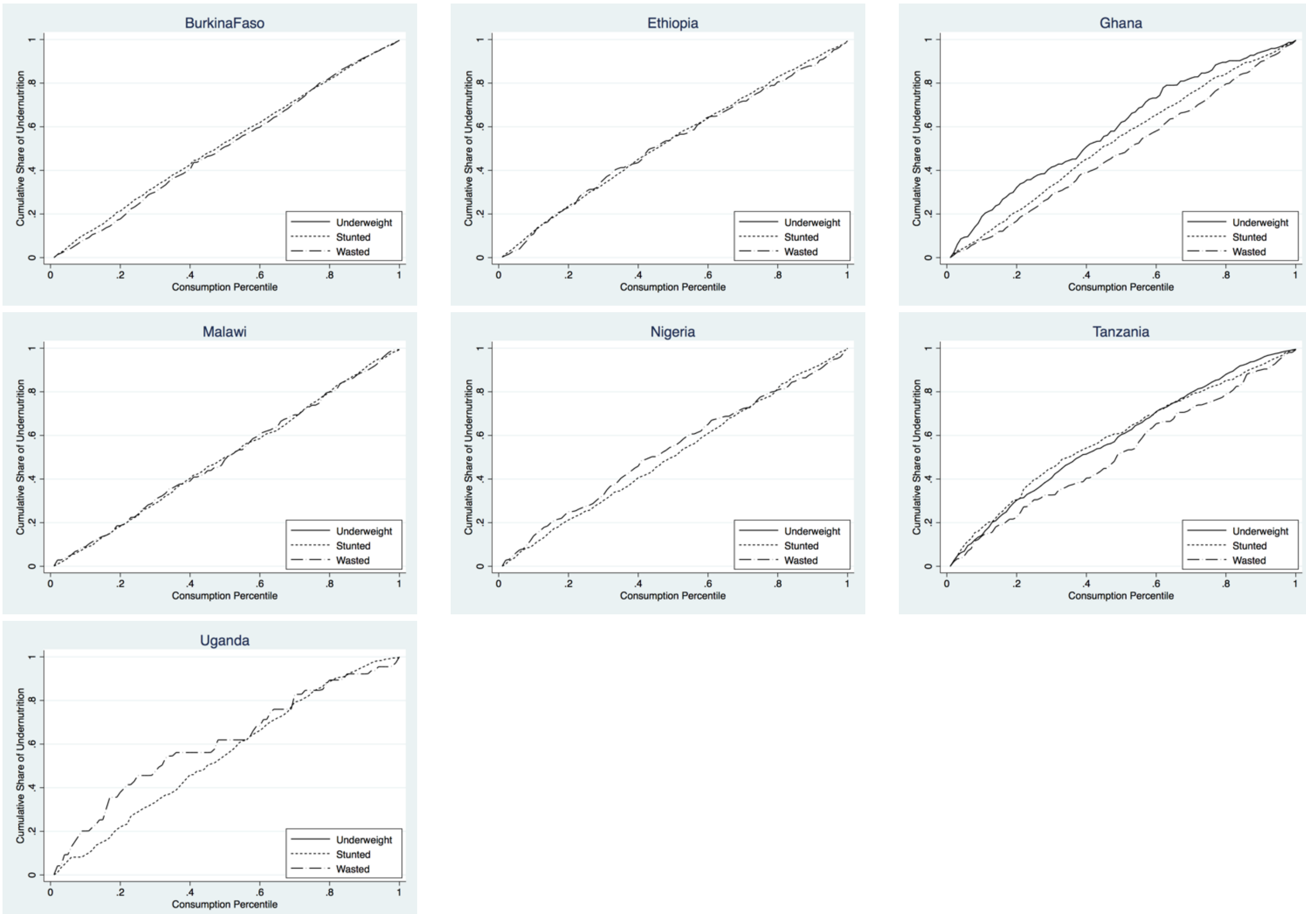

Figure 2.4: Concentration curves for undernutrition and household consumption. The graphs show the concentration curves for cumulative proportion of women who are underweight, and children who are stunted and wasted at each consumption percentile. Data is drawn from the LSMS surveys. Observations with missing values have been dropped. Women between 15 and 49 years of age and children between 0 and 5 years of age are included in the sample. Household consumption is used to construct the consumption percentiles. Consumption percentiles are constructed separately for women and children. The Stata command glcurve is used to construct the lines.

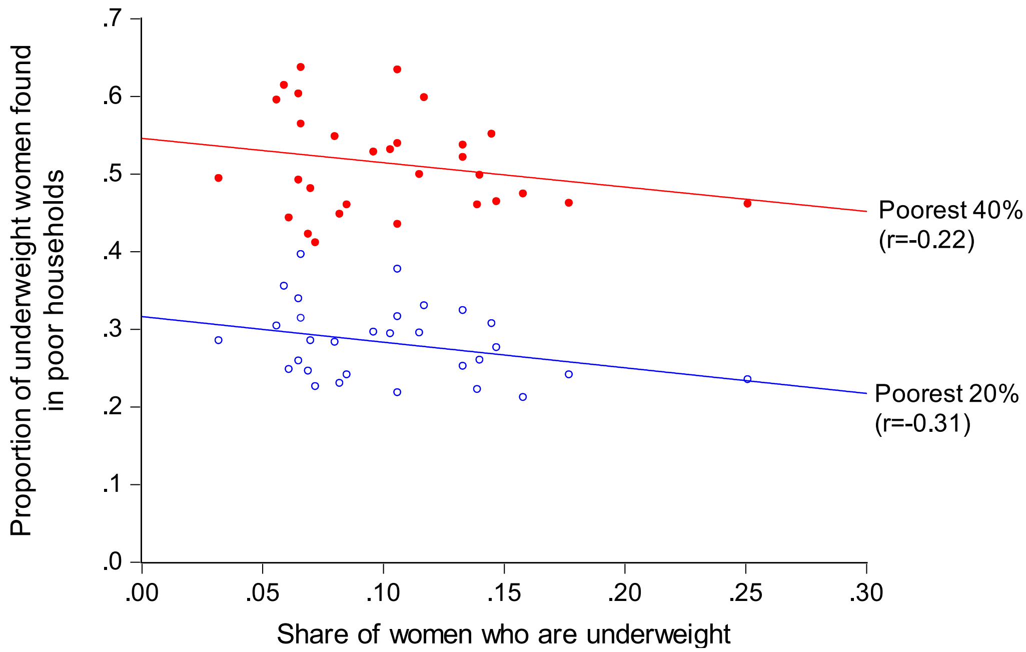

Figure 2.5: Countries with fewer underweight women tend to have a higher proportion of those women in wealth-poor households. The graph plots the joint probability of a woman being both underweight and in a poor household against the share of women who are underweight for each country.

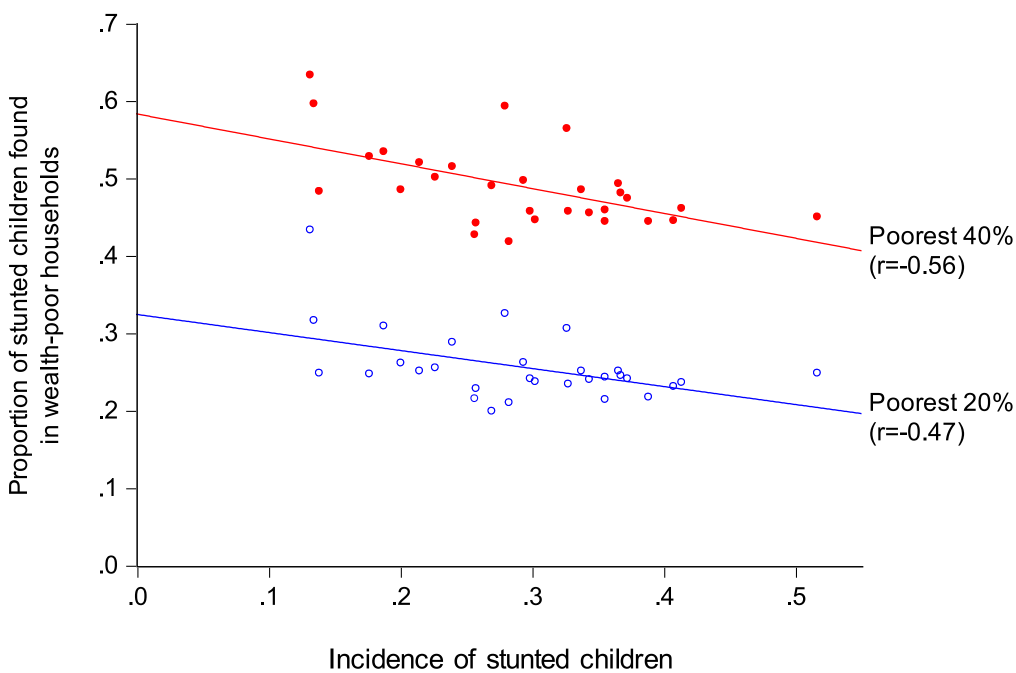

Figure 2.6: Countries with fewer stunted children tend to have a higher proportion of these children in wealth-poor households. The graph plots the joint probability of a child being both stunted and in a poor household against the share of children who are stunted for each country.

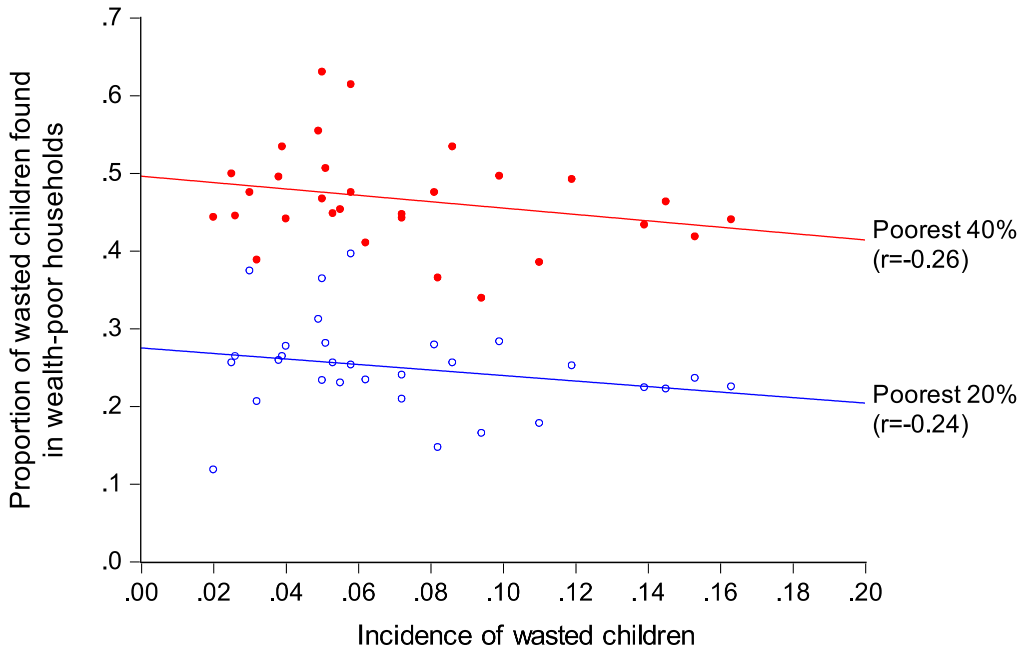

Figure 2.7: Countries with fewer wasted children tend to have a higher proportion of those children in wealth-poor households. The graph plots the joint probability of a child being both wasted and in a poor household against the share of children who are wasted for each country.

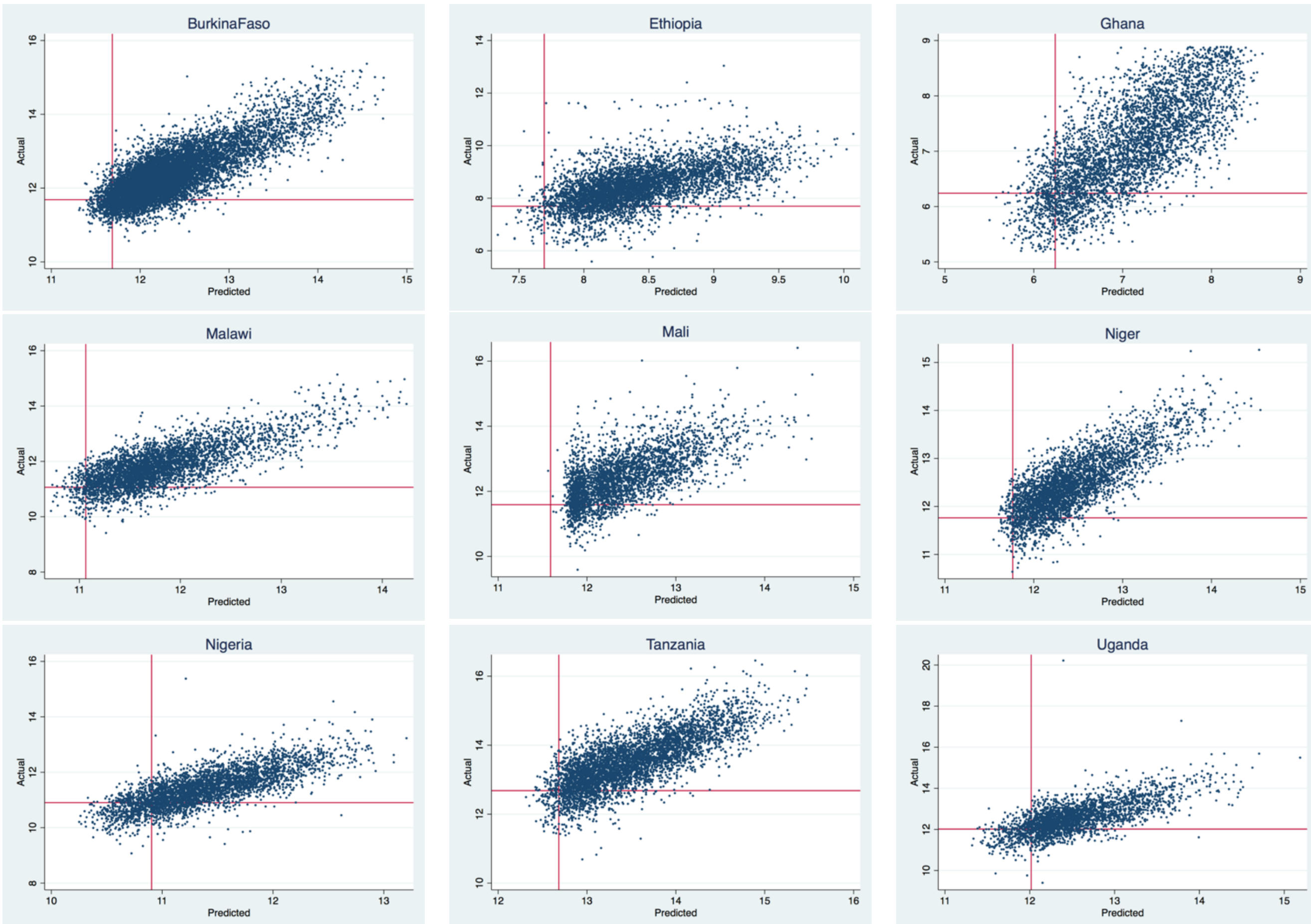

Figure 3.1: Actual and predicted consumption for the Basic Proxy Means Test. The figure shows actual and predicted consumption in logged values using Basic PMT. The red lines represent the poverty line at the 20th percentile in logged values. Points in the top left corner are incorrectly predicted as poor (inclusion errors). Points in the bottom right corner are incorrectly predicted as non-poor (exclusion errors). Points in the bottom left and top right corners are correctly predicted as poor and non-poor respectively.

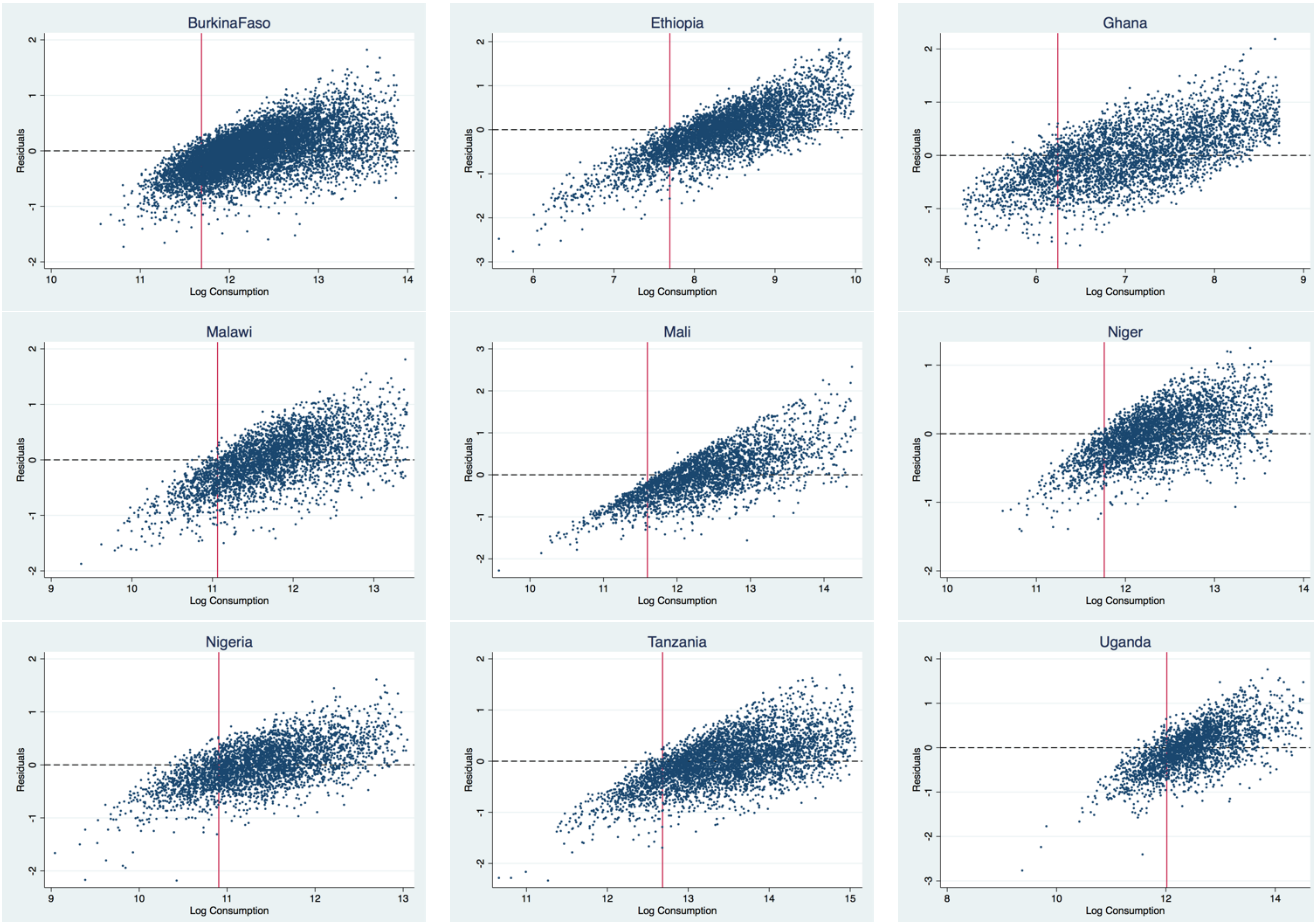

Figure 3.2: Residuals for the Basic Proxy Means Test plotted against log real consumption per capita. The figure shows log real household consumption per capita and the residuals for the predicted consumption values. Basic PMT is used to predict consumption. The red lines represent the poverty line at the 20th percentile.

Table 1.1: Child enrollment rates by state. All children between 5 and 16 years of age are included. Assam was not surveyed in 2006 due to conflict. Chhattisgarh and Jharkhand were formed in 2001.

	1999			2006		
	N	Mean	Std. Dev.	N	Mean	Std. Dev.
Andhra Pradesh	515	0.73	0.44	614	0.88	0.32
Assam	439	0.64	0.48			
Bihar	748	0.53	0.5	376	0.61	0.49
Chhattisgarh				519	0.76	0.43
Gujarat	962	0.69	0.46	778	0.78	0.41
Haryana	750	0.76	0.43	749	0.76	0.43
Himachal Pradesh	163	0.9	0.31	128	0.88	0.32
Jharkhand				305	0.64	0.48
Karnataka	1053	0.63	0.48	980	0.76	0.43
Kerala	349	0.93	0.26	324	0.91	0.28
Madhya Pradesh	1452	0.65	0.48	1078	0.72	0.45
Maharashtra	473	0.79	0.41	577	0.77	0.42
Orissa	648	0.79	0.41	526	0.79	0.41
Punjab	417	0.84	0.36	429	0.81	0.39
Rajasthan	1489	0.66	0.47	1285	0.78	0.42
Tamil Nadu	494	0.86	0.35	504	0.96	0.19
Uttar Pradesh	1981	0.57	0.5	2168	0.78	0.41
West Bengal	371	0.66	0.48	440	0.76	0.43
Mean	12304	0.68	0.47	11780	0.78	0.41

Table 1.2: Average enrollment by state and caste group. All children between 5 and 16 years of age are included. Assam was not surveyed in 2006 due to conflict. Chhattisgarh and Jharkhand were formed in 2001. SC/ST refers to Scheduled Caste/Scheduled Tribe. OBC refers to Other Backwards Caste. General caste includes all other caste groups excluding Muslims, specifically Brahmin, Other Upper, and all Non-Class children excluding Non-Class Muslim.

	1999				2006			
	SC/ST	OBC	General	Muslim	SC/ST	OBC	General	Muslim
Andhra Pradesh	0.68	0.67	0.80	0.50	0.91	0.86	0.89	0.93
Assam	0.56	0.66	0.69	0.62				
Bihar	0.56	0.57	0.60	0.42	0.64	0.64	0.74	0.52
Chhattisgarh					0.71	0.78	0.60	
Gujarat	0.56	0.75	0.73	0.67	0.73	0.79	0.82	0.65
Haryana	0.80	0.84	0.87	0.48	0.67	0.73	0.89	0.62
Himachal Pradesh	0.76	0.50	0.96		0.80	0.79	0.93	0.76
Jharkhand					0.44		0.76	
Karnataka	0.54	0.64	0.73	0.50	0.71	0.79	0.79	0.95
Kerala	0.88	0.95	0.94	0.89	0.89	0.91	0.93	0.89
Madhya Pradesh	0.60	0.65	0.71	0.57	0.71	0.70	0.84	0.68
Maharashtra	0.80	0.75	0.82	0.50	0.76	0.77	0.78	0.50
Orissa	0.77	0.81	0.81	0.48	0.69	0.81	0.90	0.50
Punjab	0.74	1.00	0.86	1.00	0.73	0.81	0.93	
Rajasthan	0.55	0.70	0.67	0.76	0.74	0.78	0.83	0.76
Tamil Nadu	0.87	0.86	0.89		0.97	0.96	1.00	1.00
Uttar Pradesh	0.51	0.57	0.65	0.43	0.81	0.77	0.85	0.61
West Bengal	0.49	0.67	0.76	0.67	0.73	0.77	0.86	0.66
Mean	0.61	0.69	0.75	0.53	0.74	0.79	0.85	0.66

Table 1.3: Descriptive statistics for key variables by caste. The table reports the average value across children between 5 and 16 years of age by year and by caste group. Education refers to years of schooling completed. Distance is measured in kilometers. Consumption is per capita and in 1999 prices.

	1999					2006				
	SC/ST	OBC	General	Muslim	Mean	SC/ST	OBC	General	Muslim	Mean
Individual										
Female	0.48	0.47	0.47	0.43	0.47	0.47	0.46	0.45	0.48	0.46
Age	10.0	10.3	10.3	10.2	10.2	10.9	10.7	10.8	10.5	10.8
Mother education	1.50	3.03	3.80	2.14	2.95	1.85	3.00	4.86	1.96	3.05
Grandmother education	1.42	2.94	3.68	1.95	2.84	2.17	3.15	5.07	2.21	3.36
Father education	3.81	5.68	6.96	4.82	5.73	4.38	6.14	7.89	4.79	5.97
Father is head	0.64	0.66	0.60	0.70	0.64	0.73	0.65	0.58	0.73	0.66
Hindu	0.96	0.99	0.93	0.00	0.88	0.94	0.98	0.93	0.00	0.88
Muslim	0.00	0.00	0.00	1.00	0.09	0.00	0.00	0.00	1.00	0.08
Caste: SC/ST				0.00	0.20				0.00	0.26
Caste: OBC				0.09	0.32				0.70	0.48
Caste: General				0.91	0.48				0.30	0.26
Household										
Household size	7.90	7.80	8.53	9.22	8.23	6.52	6.89	6.88	7.66	6.86
Head age	47.3	47.5	49.5	48.0	48.3	45.6	48.1	50.5	47.5	48.0
Head is married	0.90	0.92	0.90	0.94	0.91	0.91	0.89	0.88	0.89	0.89
Head education	3.18	4.87	5.65	4.52	4.81	3.56	4.90	6.28	4.18	4.81
Share 0 to 18 years male	0.28	0.28	0.27	0.33	0.28	0.29	0.28	0.26	0.31	0.28
Share 0 to 18 years female	0.26	0.25	0.24	0.25	0.25	0.25	0.24	0.23	0.29	0.24
Share 19 to 55 years male	0.03	0.03	0.03	0.03	0.03	0.03	0.04	0.05	0.03	0.04
Share 19 to 55 years female	0.03	0.03	0.03	0.03	0.03	0.04	0.04	0.05	0.03	0.04
Oldest child is female	0.46	0.48	0.49	0.37	0.47	0.49	0.48	0.51	0.45	0.49
Own land	0.65	0.80	0.83	0.74	0.78	0.55	0.71	0.79	0.65	0.68
Total consumption	4947	5940	7078	4931	6103	7641	10184	11150	7646	9539
Village										
Population	7928	8799	9073	5865	8474	2542	3452	4419	5803	3635
Distance to nearest block headquarters	12.42	11.61	13.33	9.29	12.25	14.47	12.65	13.51	7.47	12.89
Distance to nearest paved road	3.39	1.50	3.22	0.66	2.49	1.43	0.13	2.77	0.05	1.07
Distance to nearest town	13.27	12.48	14.21	12.46	13.32	14.03	12.72	12.65	13.67	13.12
Percent Hindu	84.59	90.19	84.60	45.51	82.90	89.62	92.09	88.08	48.81	86.92
Percent Muslim	10.78	6.58	8.27	53.84	12.26	4.49	4.81	5.77	50.91	8.79
Number of schools	1.54	1.51	1.83	1.83	1.67	2.34	2.54	2.59	3.14	2.55
Distance to nearest secondary school	5.20	4.59	4.70	4.86	4.78	4.22	3.13	3.20	3.42	3.45
Share of government schools	1.20	1.35	1.11	0.89	1.18	1.90	1.53	1.43	1.42	1.59
Share of schools with midday meal	0.44	0.56	0.40	0.27	0.45	1.42	1.15	1.04	1.02	1.18

Table 1.4: Maternal grandmother education by state. Education refers to years of schooling. All maternal grandmothers that can be linked to children between 5 and 16 years of age are included. Assam was not surveyed in 2006 due to conflict. Chhattisgarh and Jharkhand were formed in 2001. The smaller sample size in 2006 reflects the fact that many maternal grandmothers could not be matched with their grandchildren.

	1999			2006		
	N	Mean	Std. Dev.	N	Mean	Std. Dev.
Andhra Pradesh	477	2.74	3.65	313	3.04	3.83
Assam	430	3.40	3.74			
Bihar	710	1.85	2.81	235	2.23	2.79
Chhattisgarh				177	4.00	3.88
Gujarat	844	2.79	3.49	484	3.34	3.46
Haryana	685	1.78	3.43	355	3.27	4.46
Himachal Pradesh	160	5.66	4.84	62	5.29	4.79
Jharkhand				162	1.69	2.94
Karnataka	993	2.97	3.87	517	4.06	4.09
Kerala	333	8.76	3.88	218	7.30	3.82
Madhya Pradesh	1260	1.89	3.43	460	3.10	3.58
Maharashtra	414	3.98	3.89	288	4.33	3.97
Orissa	596	3.82	4.25	306	3.68	3.22
Punjab	403	5.02	3.98	212	5.33	3.71
Rajasthan	1411	1.25	2.80	686	1.80	3.22
Tamil Nadu	472	4.34	4.06	341	4.07	3.36
Uttar Pradesh	1844	2.39	3.99	1220	2.81	4.14
West Bengal	350	3.88	4.31	244	3.45	3.94
Mean	11382	2.84	3.94	6280	3.36	3.92

Table 1.5: Regression results for social effects. Peer groups are defined within village and by caste group and survey year. The four caste groups used are SC/ST, OBC, General, and Muslim. Peer enrollment is the leave-out mean enrollment rate for each child's caste group within the village and survey year. Peer mother education is the leave-out mean years of schooling for the mothers within the child's caste group. Other women education is the average years of schooling for women within the caste group who do not have school-aged children. Peer grandmother education is the leave-out mean years of schooling for the maternal grandmothers within the child's caste group. The list of individual, household, and village control variables can be found in Table 1.3. Clustered standard errors are in parentheses. Average peer maternal grandmother education is used as the IV. ***: significant at 1% level; **: 5%; *: 10%.

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Peer Enrollment	0.572***	0.766***	0.443***	0.738***	0.343***	0.583**
	(0.029)	(0.194)	(0.038)	(0.246)	(0.044)	(0.277)
Peer Mother Education	-0.000	-0.004	0.001	-0.004	0.005**	-0.000
	(0.002)	(0.005)	(0.002)	(0.005)	(0.002)	(0.006)
Other Women Education	0.001	-0.001	0.003**	-0.000	0.002	-0.000
	(0.001)	(0.002)	(0.001)	(0.003)	(0.002)	(0.003)
Individual and Household controls	Yes	Yes	Yes	Yes	Yes	Yes
Village controls	Yes	Yes	Yes	Yes		
Tehsil fixed effects			Yes	Yes		
Village fixed effects					Yes	Yes
R^2	0.164	0.160	0.173	0.166	0.180	0.176
N	15734	15734	15424	15424	15831	15831
First Stage						
Peer Grandmother Education		0.016***		0.015***		0.013***
		(0.004)		(0.004)		(0.003)
R^2		0.445		0.592		0.647

Table 1.6: Regression results for social effects by lower and general caste. Peer groups are defined within village and by caste group and survey year. See the notes in Table 1.5 for further details.

	Lower Caste				General Caste			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Peer Enrollment	0.357***	0.909**	0.199***	0.850	-0.167	0.527	-0.665***	0.724
	(0.064)	(0.407)	(0.074)	(0.520)	(0.125)	(0.642)	(0.206)	(0.484)
Peer Mother Education	0.003	-0.009	0.008*	-0.009	0.014***	0.004	0.017**	0.004
	(0.004)	(0.010)	(0.005)	(0.014)	(0.005)	(0.010)	(0.007)	(0.007)
Other Women Education	0.003	-0.001	0.005*	-0.000	0.005	-0.002	0.006	-0.007
	(0.002)	(0.004)	(0.003)	(0.005)	(0.004)	(0.007)	(0.006)	(0.006)
Individual and household controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village controls	Yes	Yes			Yes	Yes		
Tehsil fixed effects	Yes	Yes			Yes	Yes		
Village fixed effects			Yes	Yes			Yes	Yes
R^2	0.171	0.150	0.184	0.160	0.183	0.168	0.206	0.162
N	8729	8729	8914	8914	5329	5329	5548	5548
First Stage								
Peer Grandmother Education		0.013***		0.011**		0.010		0.016**
		(0.005)		(0.005)		(0.006)		(0.006)
R^2		0.633		0.709		0.792		0.854

Table 1.7: Regression results for social effects by gender and age group. Peer groups are defined within village and by caste group, survey year, and further by gender and age group. See the notes in Table 1.5 for further details.

	Boys		Girls		Age 5 to 10		Age 11 to 16	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Peer Enrollment	0.135***	0.458	0.251***	0.530**	0.225***	0.975**	0.228***	-0.163
	(0.050)	(0.521)	(0.051)	(0.269)	(0.043)	(0.435)	(0.047)	(0.467)
Peer Mother Education	0.009***	0.002	0.004	-0.001	0.006**	-0.007	0.005	0.013
	(0.003)	(0.012)	(0.003)	(0.005)	(0.003)	(0.008)	(0.003)	(0.010)
Other Women Education	0.001	-0.000	0.006**	0.002	0.006***	0.001	0.003	0.007
	(0.002)	(0.003)	(0.002)	(0.005)	(0.002)	(0.004)	(0.002)	(0.006)
Individual and household controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.186	0.176	0.208	0.199	0.317	0.268	0.247	0.230
N	8349	8349	7186	7186	8024	8024	7485	7485
First Stage								
Peer Grandmother Education		0.009**		0.019***		0.010***		0.010**
		(0.004)		(0.004)		(0.004)		(0.004)
R^2		0.586		0.611		0.609		0.571

Table 1.8: Time allocation by caste group and survey year. Average hours spent on each activity per day is shown above. Farm employment includes casual labor, crop production, and livestock production. Non-farm employment includes salary work, self-employment (non-farm), and wage labor (non-farm). Housework includes casual household work and fuel collection. Leisure in the 2006 survey includes time spent sleeping.

	1999				2006			
	SC/ST	OBC	General	Muslim	SC/ST	OBC	General	Muslim
Study	2.19	2.57	2.72	1.90	5.45	5.93	6.57	4.60
Farm & Non-farm employment	0.29	0.23	0.25	0.33	0.44	0.32	0.23	0.44
Housework	0.86	0.70	0.74	0.77	0.69	0.65	0.64	0.71
Leisure	5.64	5.43	5.21	5.77	15.96	16.07	15.59	17.66

Table 1.9: Regression results for time allocation. Peer groups are defined within village and by caste group and survey year. The four caste groups used are SC/ST, OBC, General, and Muslim. Outcome variables are measured in hours per day. Peer enrollment, peer mother education, and other women education are defined in Table 1.5.

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Study						
Peer Enrollment	2.567***	2.839**	2.142***	5.791***	1.982***	2.397
	(0.236)	(1.196)	(0.259)	(1.502)	(0.263)	(1.666)
Peer Mother Education	0.029	0.023	0.031	-0.037	0.049**	0.040
	(0.019)	(0.028)	(0.019)	(0.031)	(0.019)	(0.037)
Other Women Education	0.011	0.009	0.012	-0.028	0.018	0.015
	(0.013)	(0.013)	(0.014)	(0.019)	(0.016)	(0.018)
Farm & Non-farm employment						
Peer Enrollment	-0.298***	-0.151	-0.269***	0.233	-0.272***	0.687
	(0.081)	(0.437)	(0.082)	(0.551)	(0.083)	(0.620)
Peer Mother Education	-0.015**	-0.018*	-0.014**	-0.023**	-0.015**	-0.035**
	(0.006)	(0.010)	(0.006)	(0.011)	(0.006)	(0.014)
Other Women Education	-0.001	-0.003	-0.004	-0.009	-0.005	-0.014**
	(0.005)	(0.005)	(0.004)	(0.007)	(0.004)	(0.007)
Housework						
Peer Enrollment	-0.382***	-0.335	-0.348***	-0.490	-0.185	0.336
	(0.113)	(0.516)	(0.108)	(0.634)	(0.113)	(0.717)
Peer Mother Education	-0.017**	-0.018	-0.023***	-0.020	-0.023***	-0.034**
	(0.008)	(0.012)	(0.009)	(0.013)	(0.008)	(0.016)
Other Women Education	-0.009	-0.009	0.003	0.004	0.004	-0.001
	(0.006)	(0.006)	(0.006)	(0.008)	(0.005)	(0.008)
Leisure						
Peer Enrollment	-2.206***	-0.669	-2.140***	-0.156	-1.981***	2.065
	(0.264)	(1.401)	(0.303)	(1.730)	(0.286)	(1.963)
Peer Mother Education	0.016	-0.017	0.045**	0.008	0.035*	-0.050
	(0.019)	(0.033)	(0.021)	(0.035)	(0.020)	(0.044)
Other Women Education	0.001	-0.013	-0.012	-0.033	0.005	-0.032
	(0.017)	(0.016)	(0.017)	(0.022)	(0.020)	(0.021)
Individual and Household controls	Yes	Yes	Yes	Yes	Yes	Yes
Village controls	Yes	Yes	Yes	Yes		
Tehsil fixed effects			Yes	Yes		
Village fixed effects					Yes	Yes
N	15734	15734	15424	15424	15831	15831

Table 2.1: List of countries and survey years. Observations with missing values and for pregnant women have been dropped. Women between 15 and 49 years of age and children between 0 and 5 years of age are included. The LSMS surveys used are Burkina Faso's 2014 Multisector Survey; the 2013-14 Ethiopian Rural Socioeconomic Survey; Ghana's 2009 Socioeconomic Panel Survey; Malawi's 2013-14 Third Integrated Household Survey; Nigeria's 2012-13 General Household Survey; Tanzania's 2012-13 National Panel Survey and Uganda's 2011-12 National Panel Survey. It is not possible to determine whether a woman is pregnant at the time of measurement in the Tanzania survey.

	Demographic and Health Surveys			Consumption surveys with anthropometric data		
		Observations in DHS			Observations in the survey	
Country	Year	Women	Children	Year	Women	Children
Benin	2011	13,626	7,193			
Burkina Faso	2010	7,218	6,223	2014	n.a.	9,134
Burundi	2010	3,751	3,190			
Cameroon	2011	6,431	4,585			
Congo	2011	4,543	4,127			
Cote D'Ivoire	2011	3,950	2,967			
DRC	2013	7,872	7,791			
Ethiopia	2011	13,830	9,144	2013/14	n.a.	2,731
Gabon	2012	4,195	3,043			
Gambia	2013	3,843	2,828			
Ghana	2014	4,153	2,589	2009	2,165	1,968
Guinea	2012	3,996	2,969			
Kenya	2008	7,286	4,852			
Lesotho	2009	1,895	731			
Liberia	2013	4,015	3,075			
Malawi	2010	6,409	4,283	2013/14	n.a.	2,400
Mali	2012	4,402	4,134			
Mozambique	2011	11,186	8,622			
Namibia	2013	3,393	1,649			
Niger	2012	3,896	4,285			
Nigeria	2013	30,900	22,499	2012/13	n.a.	2,742
Rwanda	2010	5,491	3,507			
Senegal	2010	2,188	1,139			
Sierra Leone	2013	7,023	3,938			
Swaziland	2006	4,190	1,883			
Tanzania	2010	8,528	6,402	2012/13	6,170	3,633
Togo	2013	4,153	3,023			
Uganda	2011	2,297	1,987	2011/12	n.a.	1,494
Zambia	2013	13,872	10,769			
Zimbabwe	2010	7,382	4,071			
Total	2011	205,914	147,498		8,335	24,102

Table 2.2: Summary statistics for nutritional indicators using the Demographic and Health Surveys (DHS). Observations with missing values and pregnant women are dropped. Means are population weighted. Women between 15 and 49 years of age and children between 0 and 5 years of age are included. A woman is underweight if she has a BMI less than or equal to 18.5. A child is stunted if she is two standard deviations below median height-for-age and wasted if she is two standard deviations below median weight-for-height.

	Underweight women	Stunted children	Wasted children
Benin	0.064	0.407	0.144
Burkina Faso	0.154	0.298	0.139
Burundi	0.160	0.516	0.051
Cameroon	0.068	0.279	0.050
Congo	0.144	0.187	0.051
Cote D'Ivoire	0.078	0.238	0.071
DRC	0.144	0.366	0.072
Ethiopia	0.266	0.388	0.086
Gabon	0.074	0.131	0.032
Gambia	0.167	0.200	0.110
Ghana	0.061	0.134	0.053
Guinea	0.122	0.269	0.099
Kenya	0.122	0.293	0.058
Lesotho	0.058	0.302	0.030
Liberia	0.073	0.257	0.058
Malawi	0.087	0.413	0.038
Mali	0.114	0.337	0.119
Mozambique	0.086	0.371	0.049
Namibia	0.140	0.176	0.081
Niger	0.154	0.355	0.153
Nigeria	0.111	0.326	0.163
Rwanda	0.070	0.365	0.025
Senegal	0.222	0.159	0.090
Sierra Leone	0.090	0.327	0.082
Swaziland	0.033	0.226	0.020
Tanzania	0.113	0.355	0.040
Togo	0.070	0.214	0.062
Uganda	0.117	0.281	0.039
Zambia	0.102	0.343	0.055
Zimbabwe	0.070	0.256	0.028
Mean	0.114	0.321	0.086

Table 2.3: Summary statistics for nutritional indicators using the Living Standards and Measurement Surveys (LSMS). Data are drawn from LSMS surveys. Observations with missing values have been dropped. Means are population weighted. Women between 15 and 49 years of age and children between 0 and 5 years of age are included in the sample. A woman is underweight if she has a BMI less than or equal to 18.5. A child is stunted if she is two standard deviations below median height-for-age and wasted if she is two standard deviations below median weight-for-height.

	Underweight women	Stunted children	Wasted children
Burkina Faso	n.a.	0.342	0.110
Ethiopia	n.a.	0.406	0.121
Ghana	0.081	0.385	0.202
Malawi	n.a.	0.260	0.079
Nigeria	n.a.	0.234	0.106
Tanzania	0.095	0.120	0.048
Uganda	n.a.	0.280	0.036
Mean	n.a.	0.288	0.093

Table 2.4: Summary statistics on selected other indicators. Poverty rates are for $1.90 per person per day at 2011 PPP; estimates from PovcalNet, accessed 8/18/2016. Mean poverty rate is for Sub-Saharan Africa as a whole. GDP, literacy, access to improved water and sanitation are all taken from the World Bank's World Development Indicators. Literacy rate for 2011 or closest available year to 2011 in 2007-15; more recent year for ties. Water and sanitation for 2011.

	GDP per capita, 2011, $PPP/year	Poverty		Female literacy rate	Access to improved water (%)	Access to improved sanitation (%)
		Poverty rate (%)	Year for poverty rate			
Benin	1762	53.1	2011	0.184	75.3	17.8
Burkina Faso	1470	55.3	2009	0.216	80.0	18.0
Burundi	713	77.7	2006	0.846	75.0	47.2
Cameroon	2614	29.3	2007	0.648	73.1	44.9
Congo	5632	28.7	2011	0.729	51.1	27.2
Cote D'Ivoire	2547	29.0	2008	0.305	74.8	14.4
DRC	617	773	2012	0.629	80.9	21.3
Ethiopia	1165	33.5	2010	0.289	49.7	23.0
Gabon	17101	8.0	2005	0.799	91.6	41.2
Gambia	1532	45.3	2003	0.446	89.5	58.8
Ghana	3431	25.3	2005	0.653	84.3	14.0
Guinea	1184	35.3	2012	0.122	73.8	18.4
Kenya	2623	33.6	2005	0.669	60.8	29.4
Lesotho	2297	59.7	2010	0.850	81.0	28.9
Liberia	733	68.6	2007	0.270	72.0	15.8
Malawi	1079	70.9	2010	0.513	82.9	39.2
Mali	1863	49.3	2009	0.246	68.9	22.9
Mozambique	952	68.7	2008	0.365	49.3	19.3
Namibia	8626	22.6	2009	0.784	88.0	32.7
Niger	807	50.3	2011	0.089	55.1	9.8
Nigeria	5231	53.5	2009	0.414	64.5	30.2
Rwanda	1397	60.3	2010	0.647	73.7	58.2
Senegal	2159	38.0	2011	0.404	75.6	45.6
Sierra Leone	1415	52.3	2011	0.350	58.5	12.7
Swaziland	7620	42.0	2009	0.824	72.3	57.0
Tanzania	2207	46.6	2011	0.744	55.3	13.6
Togo	1255	54.3	2011	0.480	60.5	11.5
Uganda	1649	33.2	2012	0.620	74.2	18.3
Zambia	3343	64.4	2010	0.518	62.2	43.0
Zimbabwe	1524	n.a.	n.a.	0.801	77.7	37.5
Mean	2806	42.7	2012	0.472	71.1	29.1

Table 2.5: Correlation matrix for nutritional and other indicators. The critical value for prob.=0.05 is r=0.306. GDP and literacy are taken from the World Bank's World Development Indicators. Literacy rate for 2011 or closest available year to 2011 in 2007-15; more recent year for ties.

	Under-weight women	Stunted children	Wasted children	GDP per capita	Poverty rate	Female literacy rate	Access to water	Access to sanitation
Underweight women	1.000	0.126	0.384	-0.208	-0.066	-0.232	-0.214	0.000
Stunted children	0.126	1.000	0.114	-0.540	0.712	-0.167	-0.331	-0.060
Wasted children	0.384	0.114	1.000	-0.215	0.047	-0.729	-0.071	-0.307
GDP per capita	-0.208	-0.540	-0.215	1.000	-0.613	0.462	0.326	0.291
Poverty rate	-0.066	0.712	0.047	-0.613	1.000	-0.147	-0.144	0.007
Female literacy rate	-0.232	-0.167	-0.729	0.462	-0.147	1.000	0.260	0.475
Access to water	-0.214	-0.331	-0.071	0.326	-0.144	0.260	1.000	0.377
Access to sanitation	0.000	-0.060	-0.307	0.291	0.007	0.475	0.377	1.000

Table 2.6: Incidence of undernutrition for countries with data on male body mass index (BMI). The table shows the proportion of undernourished women and children in male headed households separated by the nutritional status of the household head. The figures in parentheses are the shares of those women or children who are undernourished found in each of the two groups of households according to whether the male head is underweight. Men and women are between 15 and 49 years of age. Male heads of household are also restricted to 15 and 49 years of age. Children are between 0 and 5 years of age.

	Underweight		Male head is underweight			Male head is not underweight		
	Men	Women	Underweight women	Stunted children	Wasted children	Underweight women	Stunted children	Wasted children
Ethiopia	0.371	0.266	0.301	0.392	0.122	0.249	0.355	0.066
			(0.300)	(0.291)	(0.408)	(0.700)	(0.709)	(0.592)
Ghana	0.104	0.061	0.171	0.183	0.083	0.050	0.126	0.056
			(0.146)	(0.069)	(0.070)	(0.854)	(0.931)	(0.930)
Lesotho	0.188	0.062	0.084	0.406	0.041	0.043	0.240	0.029
			(0.154)	(0.185)	(0.156)	(0.846)	(0.815)	(0.844)
Namibia	0.232	0.137	0.270	0.183	0.070	0.097	0.169	0.067
			(0.289)	(0.142)	(0.137)	(0.711)	(0.858)	(0.863)
Rwanda	0.158	0.073	0.116	0.375	0.053	0.057	0.360	0.016
			(0.166)	(0.092)	(0.246)	(0.834)	(0.908)	(0.754)
Senegal	0.275	0.216	0.278	0.184	0.088	0.219	0.181	0.062
			(0.184)	(0.145)	(0.190)	(0.816)	(0.855)	(0.810)
Sierra Leone	0.155	0.091	0.138	0.247	0.102	0.083	0.279	0.062
			(0.118)	(0.060)	(0.106)	(0.882)	(0.940)	(0.894)
Mean	0.240	0.159	0.259	0.358	0.108	0.141	0.296	0.053
			(0.256)	(0.196)	(0.291)	(0.744)	(0.804)	(0.709)

Table 2.7: Regression coefficients of individual nutritional outcomes on the Demographic and Health Surveys (DHS) household wealth index and household consumption per person. The table gives coefficients from a regression of standardized nutritional outcomes on the wealth index or standardized consumption per capita. Robust standard errors are used; * prob.<.10 ** prob.<.05 *** prob.<.01.

	DHS			LSMS		
	BMI	Height-for-age	Weight-for-height	BMI	Height-for-age	Weight-for-height
Benin	0.194***	0.189***	0.080***			
Burkina Faso	0.279***	0.281***	0.097***	n/a	0.377***	-0.016
Burundi	0.242***	0.505***	0.115***			
Cameroon	0.285***	0.451***	0.257***			
Congo	0.265***	0.292***	0.051**			
Cote D'Ivoire	0.203***	0.279***	0.043			
DRC	0.276***	0.378***	0.073***			
Ethiopia	0.352***	0.374***	0.229***	n/a	0.037	0.025
Gabon	0.182***	0.397***	0.056**			
Gambia	0.208***	0.297***	0.061**			
Ghana	0.385***	0.299***	0.070***	0.120***	0.178**	0.172
Guinea	0.284***	0.285***	0.023			
Kenya	0.331***	0.257***	0.210***			
Lesotho	0.263***	0.182***	0.091*			
Liberia	0.182***	0.183***	-0.024			
Malawi	0.197***	0.224***	0.072***	n/a	0.103**	0.004
Mali	0.025***	0.039***	0.002	n/a	0.044	0.006
Mozambique	0.344***	0.372***	0.156***			
Namibia	0.289***	0.323***	0.217***			
Niger	0.268***	0.254***	0.119***			
Nigeria	0.291***	0.566***	0.042***	n/a	0.450***	0.200***
Rwanda	0.211***	0.395***	0.026			
Senegal	0.157***	0.110***	-0.025			
Sierra Leone	0.189***	0.253***	0.027			
Swaziland	0.174***	0.305***	0.115***			
Tanzania	0.295***	0.303***	-0.022	0.213***	0.111**	0.036
Togo	0.323***	0.319***	0.073**			
Uganda	0.364***	0.278***	0.175***			
Zambia	0.284***	0.255***	0.070***			
Zimbabwe	0.311***	0.147***	0.151***			

Table 2.8: Proportion of undernourished individuals who fall into the poorest 20% and 40% of the household wealth distribution. Data are drawn from DHSs. Means are population weighted. The table lists the proportion of underweight women, stunted children and wasted children who fall below the bottom 20th and 40th percentiles of the wealth index distribution. For example, 24.8 percent of underweight women fall below the bottom 20th percentile of wealth in Benin.

	Poorest 20% of households			Poorest 40% of households		
	Underweight women	Stunted children	Wasted children	Underweight women	Stunted children	Wasted children
Benin	0.248	0.233	0.223	0.444	0.446	0.464
Burkina Faso	0.307	0.242	0.224	0.551	0.458	0.433
Burundi	0.276	0.249	0.281	0.464	0.451	0.506
Cameroon	0.396	0.326	0.364	0.637	0.594	0.630
Congo	0.221	0.310	0.232	0.460	0.534	0.465
Cote D'Ivoire	0.226	0.289	0.240	0.414	0.516	0.447
DRC	0.252	0.247	0.209	0.521	0.482	0.442
Ethiopia	0.235	0.218	0.259	0.461	0.445	0.534
Gabon	0.246	0.434	0.206	0.422	0.634	0.388
Gambia	0.212	0.262	0.178	0.474	0.486	0.385
Ghana	0.355	0.317	0.256	0.614	0.597	0.448
Guinea	0.295	0.200	0.283	0.499	0.491	0.496
Kenya	0.329	0.262	0.396	0.599	0.497	0.614
Lesotho	0.304	0.238	0.374	0.595	0.447	0.475
Liberia	0.285	0.229	0.253	0.481	0.443	0.475
Malawi	0.230	0.237	0.259	0.448	0.462	0.495
Mali	0.218	0.252	0.252	0.434	0.486	0.492
Mozambique	0.283	0.242	0.312	0.548	0.476	0.554
Namibia	0.324	0.248	0.279	0.537	0.529	0.475
Niger	0.260	0.215	0.236	0.498	0.445	0.418
Nigeria	0.294	0.307	0.225	0.531	0.565	0.440
Rwanda	0.259	0.252	0.256	0.492	0.494	0.499
Senegal	0.241	0.249	0.165	0.462	0.484	0.339
Sierra Leone	0.241	0.235	0.147	0.460	0.458	0.365
Swaziland	0.285	0.256	0.118	0.494	0.502	0.443
Tanzania	0.316	0.243	0.277	0.539	0.459	0.441
Togo	0.339	0.252	0.234	0.607	0.521	0.410
Uganda	0.377	0.211	0.264	0.634	0.419	0.534
Zambia	0.296	0.241	0.230	0.528	0.456	0.454
Zimbabwe	0.315	0.219	0.260	0.564	0.430	0.433
Mean	0.275	0.255	0.240	0.508	0.487	0.461

Table 2.9: Proportion of undernourished individuals who fall into the poorest 20% and 40% of the household consumption per capita distribution. Data are drawn from LSMS surveys. Means are population weighted. The table lists the proportion of underweight women, stunted children and wasted children who fall in the bottom 20th and 40th percentiles of the consumption per capita distribution.

	Poorest 20% of households			Poorest 40% of households		
	Underweight women	Stunted children	Wasted children	Underweight women	Stunted children	Wasted children
Burkina Faso		0.222	0.184		0.449	0.420
Ethiopia		0.250	0.230		0.463	0.465
Ghana	0.297	0.217	0.184	0.467	0.448	0.378
Malawi		0.184	0.182		0.414	0.419
Nigeria		0.222	0.275		0.424	0.526
Tanzania	0.322	0.319	0.284	0.529	0.565	0.442
Uganda		0.214	0.265		0.466	0.496
Mean	0.318	0.241	0.228	0.519	0.465	0.448

Table 2.10: Joint probabilities of being undernourished and wealth poor. Data are drawn from the DHS. Means are population weighted. The correlation coefficient is that between the joint probability and the relevant undernutrition rate from Table 7. Elasticities estimated by double-log regression. Robust standard errors in parentheses.

	Poorest 20% of Households			Poorest 40% of Households		
	Underweight women	Stunted children	Wasted children	Underweight women	Stunted children	Wasted children
Benin	0.016	0.095	0.032	0.028	0.181	0.067
Burkina Faso	0.047	0.072	0.031	0.085	0.136	0.060
Burundi	0.044	0.129	0.014	0.074	0.233	0.026
Cameroon	0.027	0.091	0.018	0.043	0.166	0.031
Congo	0.032	0.058	0.012	0.066	0.100	0.023
Cote D'Ivoire	0.018	0.069	0.017	0.032	0.123	0.032
DRC	0.036	0.090	0.015	0.075	0.177	0.032
Ethiopia	0.063	0.085	0.022	0.123	0.173	0.046
Gabon	0.018	0.057	0.007	0.031	0.083	0.012
Gambia	0.035	0.052	0.020	0.079	0.097	0.042
Ghana	0.022	0.042	0.014	0.038	0.080	0.024
Guinea	0.036	0.054	0.028	0.061	0.132	0.049
Kenya	0.040	0.077	0.023	0.073	0.146	0.035
Lesotho	0.018	0.072	0.011	0.034	0.135	0.014
Liberia	0.021	0.059	0.015	0.035	0.114	0.027
Malawi	0.020	0.098	0.010	0.039	0.191	0.019
Mali	0.025	0.085	0.030	0.049	0.164	0.058
Mozambique	0.024	0.090	0.015	0.047	0.177	0.027
Namibia	0.045	0.044	0.022	0.075	0.093	0.038
Niger	0.040	0.076	0.036	0.077	0.158	0.064
Nigeria	0.033	0.100	0.037	0.059	0.184	0.072
Rwanda	0.018	0.092	0.006	0.034	0.181	0.013
Senegal	0.053	0.040	0.015	0.102	0.077	0.031
Sierra Leone	0.022	0.077	0.012	0.041	0.150	0.030
Swaziland	0.009	0.058	0.002	0.016	0.114	0.009
Tanzania	0.036	0.086	0.011	0.061	0.163	0.018
Togo	0.024	0.054	0.014	0.042	0.111	0.025
Uganda	0.044	0.059	0.010	0.074	0.118	0.021
Zambia	0.030	0.083	0.013	0.054	0.157	0.025
Zimbabwe	0.022	0.056	0.007	0.040	0.110	0.012
Mean	0.031	0.082	0.021	0.058	0.156	0.040
Corre. coeff.	0.914	0.912	0.928	0.965	0.961	0.969
Elasticity of joint to marginal	0.888	0.765	0.953	0.950	0.824	0.947
	(0.057)	(0.096)	(0.109)	(0.045)	(0.045)	(0.033)

Table 2.11: Correlation coefficients for conditional probabilities. The critical value for prob.=0.05 is r=0.306. The wealth-index effect is for BMI in the case of underweight women, while it is height-for-age and weight-for-height in the case of the conditional probabilities for stunting and wasting.

	Poorest 20% of Households			Poorest 40% of Households		
	Underweight women	Stunted children	Wasted children	Underweight women	Stunted children	Wasted children
Poorest 20%						
Underweight women	1.000	0.013	0.503	0.911	0.191	0.450
Stunted children	0.013	1.000	-0.039	-0.072	0.884	-0.103
Wasted children	0.503	-0.039	1.000	0.531	0.016	0.790
Poorest 40%						
Underweight women	0.911	-0.072	0.531	1.000	0.102	0.422
Stunted children	0.191	0.884	0.016	0.102	1.000	-0.025
Wasted children	0.450	-0.103	0.790	0.422	-0.025	1.000
Marginal probabilities						
Underweight women	-0.312	-0.236	-0.073	-0.222	-0.241	-0.036
Stunted children	-0.150	-0.467	0.226	-0.133	-0.561	0.367
Wasted children	-0.266	-0.160	-0.236	-0.237	-0.069	-0.255
Other indicators						
Wealth-index effect	0.640	0.390	0.469	0.713	0.439	0.640
GDP per capita	-0.013	0.766	-0.195	-0.165	0.674	-0.213
Poverty rate	-0.173	-0.525	-0.011	-0.049	-0.598	-0.001
Female literacy rate	0.311	0.417	0.199	0.302	0.335	0.145
Access to water	0.029	0.293	-0.106	-0.046	0.282	-0.214
Access to sanitation	-0.149	0.257	-0.143	-0.155	0.200	0.031

Table 2.12: Proportion of underweight women who fall into the bottom 20 and 40 percent of predicted values for all women. Data are drawn from DHSs. Means are population weighted. The table lists the proportion of underweight women who fall into the bottom 20th and 40th percentiles of predicted values from the regressions with log BMI as the dependent variable. For example, 27.1 percent of underweight women in Benin have predicted BMI values that fall into the bottom 20 percent of all predicted values for women.

	Model 2		Model 3	
	Bottom 20%	Bottom 40%	Bottom 20%	Bottom 40%
Benin	0.271	0.482	0.369	0.620
Burkina Faso	0.351	0.597	0.368	0.610
Burundi	0.287	0.562	0.318	0.579
Cameroon	0.494	0.746	0.481	0.749
Congo	0.287	0.537	0.358	0.631
Cote D'Ivoire	0.254	0.459	0.346	0.569
DRC	0.389	0.654	0.416	0.669
Ethiopia	0.283	0.515	0.302	0.511
Gabon	0.228	0.513	0.433	0.755
Gambia	0.293	0.528	0.389	0.618
Ghana	0.374	0.624	0.447	0.673
Guinea	0.314	0.553	0.326	0.575
Kenya	0.363	0.629	0.389	0.654
Lesotho	0.408	0.601	0.484	0.639
Liberia	0.301	0.530	0.350	0.612
Malawi	0.302	0.514	0.378	0.572
Mali	0.274	0.490	0.314	0.541
Mozambique	0.303	0.565	0.365	0.591
Namibia	0.350	0.589	0.397	0.667
Niger	0.320	0.582	0.379	0.630
Nigeria	0.335	0.577	0.418	0.682
Rwanda	0.327	0.584	0.402	0.608
Senegal	0.307	0.537	0.376	0.629
Sierra Leone	0.280	0.510	0.333	0.577
Swaziland	0.354	0.588	0.451	0.759
Tanzania	0.346	0.587	0.360	0.601
Togo	0.399	0.652	0.388	0.645
Uganda	0.406	0.636	0.380	0.629
Zambia	0.325	0.549	0.331	0.573
Zimbabwe	0.343	0.585	0.432	0.635
Mean	0.322	0.562	0.369	0.611

Table 2.13: Proportion of undernourished children who fall into the bottom 20 and 40 percent of predicted values for all children. Data are drawn from DHSs. Means are population weighted. The table lists the proportion of stunted and wasted children who fall into the bottom 20th and 40th percentiles of predicted values from the regressions with height-for-age and weight-for-height respectively as the dependent variable.

	Stunting				Wasting			
	Model 2		Model 3		Model 2		Model 3	
	Bottom 20%	Bottom 40%	Bottom 20%	Bottom 40%	Bottom 20%	Bottom 40%	Bottom 20%	Bottom 40%
Benin	0.267	0.496	0.288	0.515	0.278	0.486	0.280	0.512
Burkina Faso	0.268	0.504	0.310	0.583	0.331	0.564	0.402	0.661
Burundi	0.252	0.483	0.289	0.522	0.298	0.497	0.434	0.596
Cameroon	0.352	0.605	0.407	0.663	0.527	0.768	0.561	0.802
Congo	0.355	0.571	0.419	0.633	0.335	0.536	0.352	0.616
Cote D'Ivoire	0.340	0.608	0.401	0.639	0.239	0.460	0.342	0.549
DRC	0.303	0.543	0.346	0.603	0.320	0.563	0.314	0.566
Ethiopia	0.262	0.481	0.283	0.554	0.297	0.547	0.421	0.630
Gabon	0.416	0.713	0.431	0.635	0.294	0.575	0.303	0.504
Gambia	0.311	0.553	0.378	0.598	0.306	0.543	0.354	0.592
Ghana	0.374	0.646	0.453	0.706	0.203	0.484	0.443	0.679
Guinea	0.297	0.544	0.351	0.620	0.352	0.599	0.435	0.638
Kenya	0.274	0.520	0.324	0.588	0.448	0.641	0.419	0.628
Lesotho	0.355	0.586	0.379	0.668	0.645	0.812	0.634	0.655
Liberia	0.287	0.514	0.319	0.581	0.269	0.521	0.441	0.679
Malawi	0.253	0.478	0.283	0.526	0.291	0.541	0.284	0.526
Mali	0.283	0.514	0.313	0.588	0.289	0.495	0.351	0.574
Mozambique	0.286	0.498	0.301	0.537	0.352	0.558	0.442	0.626
Namibia	0.341	0.601	0.388	0.621	0.361	0.656	0.382	0.602
Niger	0.289	0.531	0.328	0.579	0.271	0.469	0.359	0.568
Nigeria	0.346	0.624	0.375	0.649	0.325	0.552	0.370	0.595
Rwanda	0.299	0.529	0.328	0.599	0.353	0.567	0.445	0.589
Senegal	0.392	0.593	0.474	0.689	0.401	0.637	0.273	0.599
Sierra Leone	0.250	0.471	0.264	0.511	0.218	0.492	0.278	0.545
Swaziland	0.337	0.556	0.387	0.646	0.370	0.594	0.362	0.636
Tanzania	0.274	0.517	0.327	0.581	0.383	0.645	0.415	0.566
Togo	0.361	0.607	0.390	0.650	0.338	0.575	0.445	0.641
Uganda	0.325	0.586	0.400	0.625	0.473	0.681	0.455	0.668
Zambia	0.254	0.477	0.291	0.536	0.337	0.555	0.342	0.537
Zimbabwe	0.276	0.486	0.321	0.579	0.355	0.525	0.389	0.546
Mean	0.294	0.533	0.329	0.584	0.320	0.548	0.372	0.595

Table 3.1: Counts of inclusion and exclusion. Notation explained in the main text.

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

Table 3.2: Countries and survey rounds. All surveys except for Ghana are LSMS-ISA surveys.

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

Table 3.3: Summary table for the regressions. Values are taken from the regression tables presented in the Addendum.

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

Table 3.3: (cont.)

	Burkina Faso	Ethiopia	Ghana	Malawi	Mali	Niger	Nigeria	Tanzania	Uganda
Weighted Bottom 60									
R^2	0.267	0.192	0.358	0.256	0.235	0.225	0.435	0.274	0.357
N	4895	2307	2325	1929	1599	1629	2427	2293	1595
Stepwise (p=0.01)									
R^2	0.687	0.344	0.579	0.676	0.534	0.701	0.606	0.632	0.553
N	10265	5017	4224	3931	3212	3833	3720	4753	2650
Stepwise (p=0.05)									
R^2	0.688	0.349	0.582	0.679	0.539	0.703	0.607	0.634	0.559
N	10265	5017	4224	3931	3212	3833	3720	4753	2650
Household Shocks and Food Security									
R^2	0.726	0.395		0.690		0.722	0.670	0.654	0.604
N	10265	5017		3931		3833	3720	4753	2650
Shocks, Food Security and Community Variables									
R^2		0.400		0.698		0.725	0.672	0.655	0.607
N		5017		3931		3833	3720	4753	2650

Table 3.4: Proportion of sample predicted to be poor using the Proxy Means Test regressions. Predicted values are calculated from the Basic PMT and Extended PMT regressions (Addendum). Statistics are population weighted.

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

Table 3.5: Targeting errors using the Basic Proxy Means Test. Errors are calculated using the predicted values from the regression given in the Addendum. Statistics are population weighted.

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

Table 3.6: Targeting errors for the Basic Proxy Means Test using quantile regression centered at the poverty line. Errors are calculated using the predicted values from the regressions given in the Addendum. A quantile regression centered at the poverty line at the 20th and 40th percentile is used to estimate the model. Statistics are population weighted.

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

Table 3.7: Targeting errors for the Basic Proxy Means Test using a poverty-weighted regression. Errors are calculated using the predicted values from the regression given in the Addendum with full weight on the bottom 20 and 40 percentiles respectively. Statistics are population weighted.

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

Table 3.8: Targeting errors using a Basic Proxy Means Test weighted regression with "poor plus 20 percent." Errors are calculated using the predicted values from the regression in the Addendum with full weight on the bottom 40 and 60 percentiles respectively. Statistics are population weighted.

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

Table 3.9: Targeting errors using the Extended Proxy Means Test. Errors are calculated using the predicted values from the extended PMT regressions shown in the Addendum. Statistics are population weighted.

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

Table 3.10: Targeting differential for the various Proxy Means Test specifications. The targeting differential is computed using the poverty line at the 20th percentile. Statistics are population weighted.

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

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

Table 3.11: Headcount index of poverty post transfer. Eligible households receive uniform per capita transfers. The total transfer amount for each country is equal to the country's aggregate poverty gap. In the top two panels uniform transfers are based on their predicted consumption under the various PMT models. The poverty line is used to determine whether a household is eligible. The statistics in the table give the change in the country's headcount index following the transfer. The starting value of the headcount index is 0.2. Statistics are population weighted. Categorical targeting gives transfers to each household member who meets the specified category. If a member meets the category twice he receives two transfers (e.g. elderly and disabled). The number of children who can receive transfers under the Children category is capped at 3. If a household satisfies either of the Shock categories, every household member receives a transfer.

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

Table 3.11: (cont.)

	Niger	Nigeria	Tanzania	Uganda	Mean
Universal (basic income)	0.177	0.169	0.183	0.168	0.171
Basic PMT covariates					
Basic PMT	0.175	0.149	0.165	0.153	0.163
Using means from panel data	n.a.	0.147	0.153	0.150	0.159
Poverty quantile regression	0.166	0.150	0.162	0.154	0.155
Poverty weighted: Poor only	0.177	0.166	0.180	0.166	0.170
Poverty weighted: Poor + 20	0.164	0.156	0.165	0.157	0.162
PMT with Urban/Rural	0.173	0.145	0.161	0.147	0.159
Extended PMT covariates					
Extended PMT	0.159	0.144	0.150	0.149	0.154
Using means from panel data	n.a.	0.151	0.137	0.128	0.155
Poverty quantile regression	0.168	0.151	0.159	0.146	0.154
Poverty weighted: Poor only	0.175	0.163	0.181	0.162	0.168
Poverty weighted: Poor + 20	0.168	0.151	0.164	0.151	0.157
Stepwise (p=0.01)	0.161	0.153	0.154	0.149	0.160
HH Shocks + Food Security	0.155	0.140	0.148	0.149	0.155
Shocks, Food Security + Community Variables	0.156	0.142	0.147	0.146	0.157
Categorical Targeting					
Elderly 65+	0.185	0.182	0.185	0.171	0.181
Widowed or disabled	0.192	0.181	0.187	0.174	0.182
Elderly, widows & disabled	0.182	0.180	0.188	0.169	0.180
Children under 14 (max 3)	0.179	0.169	0.178	0.165	0.170
Elderly, widows, disabled & children	0.179	0.170	0.183	0.163	0.171
Female heads with children	0.191	0.190	0.179	0.166	0.185
Shock: drought, flood or livestock death	0.192	0.196	0.196	0.197	0.196
Shock: drought, flood, livestock death, job loss	0.192	0.196	0.195	0.198	0.197

Table 3.12: Poverty gap index post transfer. The initial value of the poverty gap index is shown in the first row. See notes to Table 3.11 for other details.

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

Table 3.12: (cont.)

	Niger	Nigeria	Tanzania	Uganda	Mean
Pre-transfer poverty gap	0.039	0.050	0.053	0.059	0.051
Universal (basic income)	0.034	0.039	0.044	0.045	0.041
Basic PMT covariates					
Basic PMT	0.034	0.029	0.041	0.038	0.037
Using means from panel data	n.a.	0.026	0.031	0.033	0.031
Poverty quantile regression	0.026	0.030	0.036	0.038	0.033
Poverty weighted: Poor only	0.034	0.038	0.044	0.044	0.040
Poverty weighted: Poor + 20	0.029	0.031	0.037	0.040	0.035
PMT with Urban/Rural	0.033	0.028	0.040	0.036	0.036
Extended PMT covariates					
Extended PMT	0.028	0.027	0.033	0.033	0.033
Using means from panel data	n.a.	0.025	0.022	0.026	0.029
Poverty quantile regression	0.026	0.030	0.036	0.038	0.033
Poverty weighted: Poor only	0.033	0.036	0.044	0.043	0.039
Poverty weighted: Poor + 20	0.029	0.029	0.035	0.038	0.033
Stepwise (p=0.01)	0.031	0.032	0.034	0.032	0.035
HH Shocks + Food Security	0.026	0.026	0.031	0.032	0.033
Shocks, Food Security + Community Variables	0.026	0.027	0.031	0.032	0.034
Categorical targeting					
Elderly 65+	0.036	0.044	0.048	0.044	0.045
Widowed or disabled	0.036	0.044	0.047	0.048	0.045
Elderly, widows & disabled	0.036	0.043	0.047	0.044	0.044
Children under 14 (max 3)	0.034	0.039	0.043	0.045	0.040
Elderly, widows, disabled & children	0.034	0.040	0.043	0.044	0.041
Female heads with children	0.038	0.047	0.045	0.046	0.047
Shock: drought, flood or livestock death	0.038	0.049	0.051	0.057	0.050
Shock: drought, flood, livestock death, job loss	0.038	0.049	0.051	0.057	0.050

Table 3.13: Watts index post transfer. The initial value of the Watts index is shown in the first row. See notes to Table 3.11 for other details.

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

Table 3.13: (cont.)

	Niger	Nigeria	Tanzania	Uganda	Mean
Actual	0.048	0.064	0.069	0.085	0.067
Universal (basic income)	0.040	0.049	0.057	0.062	0.052
Basic PMT covariates					
Basic PMT	0.041	0.036	0.053	0.049	0.048
Using means from panel data	n.a.	0.031	0.037	0.040	0.038
Poverty quantile regression	0.030	0.037	0.046	0.050	0.041
Poverty weighted: Poor only	0.040	0.047	0.056	0.061	0.051
Poverty weighted: Poor + 20	0.034	0.038	0.047	0.054	0.044
PMT with Urban/Rural	0.040	0.035	0.051	0.047	0.046
Extended PMT covariates					
Extended PMT	0.033	0.032	0.042	0.040	0.041
Using means from panel data	n.a.	0.030	0.026	0.032	0.035
Poverty quantile regression	0.030	0.036	0.043	0.047	0.040
Poverty weighted: Poor only	0.039	0.045	0.056	0.059	0.050
Poverty weighted: Poor + 20	0.035	0.035	0.044	0.050	0.041
Stepwise (p=0.01)	0.038	0.039	0.043	0.042	0.045
HH Shocks + Food Security	0.031	0.032	0.040	0.040	0.041
Shocks, Food Security + Community Variables	0.031	0.032	0.039	0.039	0.042
Categorical targeting					
Elderly 65+	0.043	0.055	0.063	0.058	0.058
Widowed or disabled	0.043	0.056	0.062	0.066	0.058
Elderly, widows & disabled	0.043	0.054	0.062	0.058	0.056
Children under 14 (max 3)	0.041	0.049	0.055	0.064	0.052
Elderly, widows, disabled & children	0.041	0.050	0.056	0.061	0.052
Female heads with children	0.045	0.061	0.059	0.067	0.061
Shock: drought, flood or livestock death	0.046	0.062	0.067	0.083	0.065
Shock: drought, flood, livestock death, job loss	0.046	0.062	0.067	0.083	0.065

Table 3.14: Watts index post transfer using differentiated transfers. This table shows the Watts index for each country following the transfers made using the Basic PMT and the differentiated transfers implied by the optimization procedure based on both linear and quadratic transfers as a function of the same variables used in the PMT with weights chosen to minimize the Watts index (see text).

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

Table 3.15: Targeting errors with lags using Method 1. The parameters of the PMT score are estimated using Round 1 data, then predicted values are generated using Round 2 covariate values. Underlying regressions are found in the Addendum. Statistics are population weighted.

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

Table 3.16: Targeting errors with lags using Method 2. The PMT is calibrated using Round 1 panel data, then predicted values are compared to actual consumption values in Round 2 panel data. Regressions are found in the Addendum. Statistics are population weighted.

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

Table 3.17: Targeting differentials for panel Proxy Means Tests. The targeting differential is computed using the poverty line at the 20th percentile. Method 1 uses Round 1 PMT calibration and Round 2 data to generate predicted values. Method 2 uses Round 1 predicted values and Round 2 actual consumption. "No lags" refers to results when the PMT is calibrated and compared to Round 2 data (i.e. the panel structure is not used but only panel households are included). Statistics are population weighted.

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

Table 3.18: Headcount index post transfer, round 2. Method 1 uses Round 1 PMT calibration and Round 2 data to generate predicted values. Method 2 uses Round 1 predicted values and compares to Round 2 actual consumption. The Basic and Extended PMT methods (rows 1 and 2) are using Round 2 data only (i.e. no lags). Only panel households are included. Statistics are population weighted.

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

Table 3.19: Targeting errors as predictors of the post-transfer poverty measures obtained by the Proxy Means Test. Standard errors in parentheses.

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

Table 3.20: R-squared in the Proxy Means Test regression as a predictor of the post-transfer poverty measures. Standard errors in parentheses.

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

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