

Program Targeting with Machine Learning and Mobile Phone Data: Evidence from an Anti-Poverty Intervention in Afghanistan*

Emily L. Aiken[†] Guadalupe Bedoya[‡] Joshua E. Blumenstock[†]
Aidan Coville[‡]

This version: February 12, 2022

First version: April 10, 2020

Abstract

Can mobile phone data improve program targeting? By combining rich survey data from a “big push” anti-poverty program in Afghanistan with detailed mobile phone logs from program beneficiaries, we study the extent to which machine learning methods can accurately differentiate ultra-poor households eligible for program benefits from ineligible households. We show that machine learning methods leveraging mobile phone data can identify ultra-poor households nearly as accurately as survey-based measures of consumption and wealth; and that combining survey-based measures with mobile phone data produces classifications more accurate than those based on a single data source.

Keywords: Targeting; Machine Learning; Mobile Phone Data; Afghanistan

JEL: I32, I38, O12, O38, C55

*We thank Seungmin Lee, Maria Camila Ayala and Thomas Escande for excellent research assistance. This work was supported by DARPA and NIWC under contract N66001-15-C-4066, the NSF under grant IIS-1942702, and by the World Bank’s Knowledge for Change Program. The U.S. Government is authorized to reproduce and distribute reprints for Governmental purposes not withstanding any copyright notation thereon. The views, opinions, and/or findings expressed are those of the authors and should not be interpreted as representing the official views or policies of the Department of Defense, the U.S. Government, the World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.

[†]School of Information, University of California, Berkeley

[‡]Development Impact Evaluation Department, World Bank

1 Introduction

Each year, hundreds of billions of dollars are spent on targeted social protection programs. The importance of these programs increased dramatically in the past 18 months: In 2020, global extreme poverty increased for the first time in two decades, and most countries expanded their social protection programs, with more than 1.1 billion new recipients receiving government-led social assistance payments (Gentilini et al., 2020).

Determining who should be eligible for program benefits — *targeting* — is a central challenge in the design of these programs (Hanna & Olken, 2018; Lindert et al., 2020). In high-income countries, targeting frequently relies on tax records or other administrative data on income. In low- and middle-income countries (LMICs), where a large fraction of the workforce is informal, programs often require primary data collection. The difficulty and cost of collecting data, and the variable quality of what gets collected, can introduce significant errors in the targeting process (Deaton, 2016; Jerven, 2013; Grosh et al., in press). These issues are exacerbated in fragile and conflict-affected countries, where two thirds of the world’s poor are expected to reside by 2030 (Corral et al., 2020).

This paper evaluates the extent to which non-traditional administrative data, processed with machine learning, can be used for program targeting. Specifically, we match call detail records (CDR) from a large mobile phone operator in Afghanistan to household survey data from the Afghan government’s Targeting the Ultra-Poor (TUP) anti-poverty program. Eligibility for the TUP program was determined through a *hybrid targeting method*, combining a community wealth ranking (CWR) and a short follow-up survey. We then assess the accuracy of three counterfactual targeting approaches at identifying the actual beneficiaries of the TUP program: (i) our *CDR-based method*, which applies machine learning to data from the mobile phone company; (ii) an *asset-based wealth index*, which uses asset ownership to approximate poverty; and (iii) *consumption*, a common benchmark for measuring poverty in LMICs.

Our analysis produces three main results. First, by comparing errors of inclusion and exclusion using the program’s hybrid method as a benchmark, we find that the CDR-based method is nearly as accurate as the asset and consumption-based methods for identifying the phone-owning ultra-poor households. Second, we find that methods combining CDR data with measures of assets and consumption are more accurate than methods using any single data source. Third, we find that when non-phone-owning households are included in the analysis, the CDR-based method remains accurate if non-phone-owning households are classified as ultra-poor; however, targeting performance is quite poor if households without

phones are ineligible for benefits. After presenting these main results, we compile data from several existing targeting programs to give an indication of the substantial reduction in marginal costs associated with CDR-based targeting.

These results connect two distinct strands of prior work. The first is a literature on program targeting, which studies the effectiveness of different mechanisms for identifying program beneficiaries. In LMICs, research has focused on the performance of proxy means tests (PMTs) (Grosh & Baker, 1995; Filmer & Pritchett, 2001; Brown et al., 2018), community-based targeting strategies (CBTs) (Alatas et al., 2012; Fortin et al., 2018), and related approaches (Banerjee et al., 2007; Karlan & Thuysbaert, 2019; Premand & Schnitzer, 2020). A meta-analysis by Coady et al. (2004), which includes 8 PMTs and 14 community-based programs, finds little difference in targeting accuracy between the two methods — but notes that targeting is regressive in a quarter of programs reviewed. In addition to issues with targeting accuracy, the current methods available for poverty targeting in LMICs are time- and resource-intensive, and may be infeasible in fragile or conflict-affected areas or in contexts where social interaction is limited, such as during a pandemic.

The second body of work explores the extent to which non-traditional sources of data, in conjunction with machine learning, might help address data gaps in LMICs (e.g. Blumenstock, 2016; Burke et al., 2021). Much of this work focuses on estimating the geographic distribution of poverty at fine spatial granularity, using data from satellites (Jean et al., 2016; Engstrom et al., 2017), mobile phones (Blumenstock et al., 2015; Hernandez et al., 2017), social media (Fatehkia et al., 2020; Sheehan et al., 2019), or some combination of these data sources (Steele et al., 2017; Pokhriyal & Jacques, 2017; Chi et al., 2020). Most relevant to our current analysis, two prior papers investigate whether the mobile phone use can approximate the wealth of individual mobile subscribers. Blumenstock et al. (2015) show that CDR data are predictive of an individual-level asset-based wealth index among a nationally representative sample of 856 Rwandan mobile phone owners ($r = 0.68$). Blumenstock (2018b) finds similar results with a sample of 1,234 male heads of households in the Kabul and Parwan districts of Afghanistan.

Our paper connects these two literatures by rigorously assessing the extent to which phone-based estimates of poverty can help with program targeting (Blumenstock, 2020). We believe the analysis will be especially relevant to the increasing number of interventions that rely on mobile money to distribute cash payments (Gentilini et al., 2020), and the growing number of contexts where mobile phone data are being made available for humanitarian purposes (Milusheva et al., 2021). For example, in just the past few years, mobile money

was used to make cash transfer payments in countries including Bangladesh (Ali & May, 2021), Ghana (Karlan et al., 2021), Liberia (USAID, 2021), and Malawi (Paul et al., 2021). Mobile phone *data* has been used to guide cash transfers in Colombia (Gentilini et al., 2020), the Democratic Republic of the Congo (Gentilini et al., 2021), Pakistan (Gentilini et al., 2020), and Togo (Aiken et al., 2021).¹

The context of our empirical analysis – identifying ultra-poor households in Afghanistan – is a particularly challenging environment for data collection and program targeting, as 62% of the households classified as not *ultra*-poor still fall below the national poverty line. In such environments, when traditional options for targeting are not feasible, these methods may provide a viable alternative for identifying households with the greatest need. Given the policy relevance of these results, we conclude our analysis by discussing important ethical and logistical considerations that may influence how CDR methods are used to support targeting efforts in practice.

2 Data and Methods

Our main analysis evaluates the extent to which machine learning and mobile phone data can differentiate between ultra-poor and non-ultra-poor households in rural Afghanistan. This section describes the study population, the key datasets, and methods used to perform the evaluation.

2.1 Household Survey Data

Our ground-truth were collected as part of the Targeting the Ultra-Poor (TUP) program implemented by the government of Afghanistan with support from the World Bank. The TUP program was a “big push,” providing multi-faceted benefits to 7,500 ultra-poor households in six provinces of Afghanistan between 2015 and 2018 (Bedoya et al., 2019). Our analysis uses data from the baseline and targeting surveys for an impact evaluation of the TUP program conducted in 80 of the poorest villages of Balkh province between November 2015 and April 2016 (N=2,582)² These data include interviews with nearly all of the 1,173 ultra-poor households in surveyed villages, and a random sample of 1,679 non-ultra-poor

¹The anti-poverty program implemented in Togo and described by Aiken et al. (2021) was based on the methods developed and evaluated in this paper. Due to the time-sensitive nature of the COVID-19 response described in Aiken et al. (2021), the two academic articles are in circulation concurrently.

²Our analysis restricts to 2,814 households for whom consumption and asset data are non-missing.

households.³

Ultra-Poor Designation Eligibility for the TUP program was determined based on based on geographic criteria,⁴ followed by a two-step process including a community wealth ranking (CWR) and a follow-up in-person survey. CWRs were conducted separately in each village, coordinated by a local NGO and village leaders, in collaboration with the government team. CWRs divided households into four categories: well-off (6%), better-off (18%), poor (33%), and extreme-poor (43%). The CWR was followed by an in-person survey to determine whether nominated households met a set of qualifying criteria, coordinated by the NGO and government representatives, and based on a measure of multiple deprivation.

For a household to be designated as *ultra-poor*, and therefore eligible for program benefits, it had to be considered extreme-poor in the CWR, and also meet at least three of six criteria:⁵

1. Financially dependent on women’s domestic work or begging
2. Owns less than 800 square meters of land or living in a cave
3. Primary woman under 50 years old
4. No adult men income earners
5. School-age children working for pay
6. No productive assets

Ultimately, 11% of the households classified as extreme-poor in the community wealth ranking step — 6% of the total population in the study villages — were classified as ultra-poor and eligible for TUP benefits. Of the 2,852 households surveyed for the TUP project, 1,173 (41%) were designated as ultra-poor, and 1,679 (59%) were non-ultra-poor.

Consumption The consumption module of the TUP survey contains information on food consumption for the week prior to the interview and non-food expenditures for the year prior to the interview. These are used to construct monthly *per capita* consumption values, as

³The response rate for ultra-poor households was 96%. Approximately 20 households in each of the study villages were randomly drawn (excluding TUP-eligible households), to provide a representative benchmark for the TUP sample.

⁴The poorest villages were identified by the availability of veterinary services, financial institutions, and social services, and being relatively accessible.

⁵While these were the official criteria used to guide selection, they were not always strictly enforced — see [Bedoya et al. \(2019\)](#).

detailed in [Bedoya et al. \(2019\)](#). Based on these data, we construct as an outcome measure the logarithm of *per capita* monthly consumption, consistent with the approach used by the Afghanistan government to determine the national poverty line.

Asset Index We construct an asset-based wealth index to assess the relative socioeconomic status of surveyed households, calculated as the first principal component of variation in household asset ownership for sixteen items detailed in Table S1. The principle component analysis (PCA) is calculated over the dataset of 2,814 households not missing any asset data, after standardizing each asset variable to zero mean and unit variance. This wealth index explains 25.3% of the variation in asset ownership. Figure S1 shows the distribution of the underlying asset index components and Table S1 shows the direction of the first principal component.

Other Variables The TUP surveys collected several other covariates that we use in subsequent analysis. These include a food security index (composed of variables relating to the skipping and downsizing of meals, separately for adults and children), a financial inclusion index (composed of access to banking and credit, knowledge of banking and credit, and savings), and a psychological well-being index for the primary woman (standardized weighted scores on the Center for Epidemiological Studies Depression scale, the World Values Survey happiness and satisfaction questions, and Cohen’s Stress Scale). The construction of each index is documented in [Bedoya et al. \(2019\)](#). Crucially, the survey also collected data from each household on mobile phone ownership. Nearly all (99%) households with a cell phone provided their phone numbers and consented to the use of their call detail records for this study.

Sample Representativity Portions of our analysis are restricted to the 535 households from the TUP survey with phone numbers that match to our CDR (see Section 2.2). Table 1 compares characteristics of households included and excluded from the 535-household subsample; Figure S2 compares the distributions of these characteristics. There are some systematic differences: the 535-household sample we analyze is richer on average than households surveyed in the TUP study, which is consistent with households in the subsample being required to own at least one phone. For instance, while 88% of non-ultra-poor households in the TUP survey own at least one phone, only 72% of ultra-poor households own at least one phone.

Summary Statistics As shown in Table 1 and Figure S3, the three measures of well-being in our dataset are only weakly correlated with one another: for example, the correlation between the asset index and consumption measure is 0.37 in the full survey and 0.34 in the matched subsample. It is particularly important to note the characteristics of the ultra-poor: while the ultra-poor population makes up 27% of the overall sub-sample, less than half of the ultra-poor fall into the bottom 27% of the sample by wealth index or consumption.

Sample Weights Since the TUP survey oversampled the ultra-poor (by a factor of roughly 12), portions of our analysis use sample weights to adjust for population representativity. When sample weights are applied, it is explicitly noted; if not mentioned, no weights are applied. The sample weights are derived from the population of the village, and the household’s ultra-poor designation.⁶ After sample weights are applied, the ultra-poor make up 5.98% of the overall population, and 4.63% of our matched subsample.

2.2 Mobile Phone Metadata

In a follow-up survey conducted in 2018, we requested informed consent from survey respondents to obtain their mobile phone CDR and match them to the survey data collected through the TUP project. CDR contain detailed information on:

- **Calls:** Phone numbers for the caller and receiver, time and duration of the call, and cell tower through which the call was placed
- **Text messages:** Phone numbers for the caller and recipient, time of the message
- **Recharges:** Time and amount of the recharge

For participants who consented, we match baseline survey data (collected November 2015 - April 2016) to CDR covering that same period, obtained from one of Afghanistan’s main mobile phone operators. For households with multiple phones and a designated household head (N=65), we match to CDR for the phone belonging to the household head. For households where the household head does not have a phone and someone else does (N=17), we match to CDR for one of the households’ phones selected at random. In total, for the 535 households in our sample, 629,543 transactions took place in the months of November 2015 to April 2016, broken down into 310,883 calls, 305,756 text messages, and 12,904 recharges.

⁶A census listing was conducted in each village to facilitate the CWR exercise. In some cases, large villages were split into smaller units and weights are based on the sub-village.

From these CDR, we compute a set of 797 behavioral indicators that capture aggregate aspects of each individual’s mobile phone use (de Montjoye et al., 2016). This set includes indicators relating to an individual’s communications (for example, average call duration and percent initiated conversations), their network of contacts (for example, the entropy of their contacts and the balance of interactions per contact), their spatial patterns based on cell tower locations (for example, the number of unique antennas visited and the radius of gyration), and their recharge patterns (including the average amount recharged and the time between recharges). Each indicator is computed separately for weekdays, weekends, daytime, and nighttime activity. The distributions of a sample of these indicators are shown in Figure S4.

2.3 Machine Learning Predictions

CDR-based Method Extending the approach described in Blumenstock et al. (2015), we test the extent to which ultra-poor status can be accurately predicted from CDR. This analysis uses the 535 TUP households who match to CDR to train a supervised machine learning algorithm to predict ultra-poverty status from the mobile phone features. The intuition — also highlighted in Figure S4 — is that ultra-poor individuals use their phones very differently than non-ultra-poor individuals, and machine learning algorithms can use those differences to predict ultra-poor status.

Our main analysis uses a gradient boosting model, which generally out-performs several other common machine learning algorithms for this task (see Table S3). The feature importances for the trained model are shown in Table S2. Probabilistic predictions are generated via 10-fold cross-validation, with folds stratified to preserve class balance. We tune hyper-parameters using five-fold cross-validation for each fold separately. Additional details on the machine learning methods are provided in Appendix A.

Combined Methods We also evaluate several approaches that use data from multiple sources to predict ultra-poor status. Our main *combined method* trains a logistic regression to classify the ultra-poor and non-ultra-poor households using the predicted ultra-poor probability from the CDR-based method (i.e., the output of the gradient boosting algorithm described above), as well as asset and consumption data collected in the TUP survey. For comparison, we similarly evaluate the performance of methods that combine only two of the available data sources (i.e., assets plus consumption, assets plus CDR, and consumption plus CDR). Predictions for each of the combined methods are pooled over 10-fold cross-validation.

2.4 Targeting Accuracy Evaluation

Evaluation on Matched Subsample Our main analysis focuses on the 535 households for which we observe both CDR and survey data, and evaluates whether machine learning methods leveraging CDR data can accurately identify households designated as ultra-poor by the TUP program (using the two-step hybrid approach described in Section 2.1). We compare the performance of the CDR-based method to the performance of methods based on the wealth index, consumption data, and combinations of these data sources.⁷ Each targeting method is evaluated based on classification accuracy, errors of exclusion (ultra-poor households misclassified as non-ultra-poor) and errors of inclusion (non-ultra-poor households misclassified as ultra-poor). We focus on the ultra-poor designation as the ‘ground truth’ status of the household, against which other methods are evaluated, since it is the most carefully vetted measure of well-being for this population, and the proxy that the government used to target TUP benefits.

To evaluate the performance of the CDR-based and combined methods, we pool out-of-sample predictions across the ten cross-validation folds, so that every household in our dataset is associated with a CDR-based predicted probability of ultra-poor status that is produced out-of-sample. To account for class imbalance, we evaluate model accuracy using a “quota method”, by selecting a cut-off threshold for ultra-poor qualification such that each method identifies the proportion of ultra-poor households in our subsample; this cut-off also balances inclusion and exclusion errors. In our 535-household matched dataset this threshold is 27%; in other samples (see following subsection), the percentage is different. We evaluate each method for precision (positive predictive value) and recall (sensitivity). To capture the trade-off between inclusion and exclusion errors for varying values of this threshold, we also construct receiver operating characteristic (ROC) curves for each method and consider the area under the curve (AUC) as a measure of targeting quality. For each evaluation metric (precision, recall, and AUC), we bootstrap 1,000 samples from the original dataset to calculate the standard deviation of the mean of the accuracy metric. Each bootstrapped sample is of the same size as the original dataset, drawn with replacement.

⁷The CDR-based method uses supervised learning to model the ultra-poverty outcome, whereas the asset- and consumption-based approaches do not. To assess the importance of this difference, we experiment with applying machine learning methods to the asset and consumption data to model the ultra-poverty outcome. In results shown in Table S4, we find that a machine-learned asset predictor provides only marginal improvements on the standard asset-based wealth index and consumption measures.

Accounting for Households Without Phones Our main results assess the performance of different targeting methods on the sample of 535 households for whom we have both survey data and mobile phone data. We also present results that show how performance is affected when the analysis includes TUP households for whom we do not have mobile phone data (typically because they do not have a phone or because they use a different phone network than the one who provided CDR). We provide analysis that targets such households (1) before households with CDR, or (2) after households with CDR (see Section 3.4). These results are evaluated on three different samples:

1. *Matched Sample:* The 535 households for whom could match survey responses to CDR.
2. *Balanced Sample:* This sample includes the 535 matched households as well as the 472 households in the TUP survey who report not owning any phone. It excludes households that own a phone on a different phone network than the one who provided CDR. The motivation for this sample is to provide an indication of targeting performance in a regime in which CDR can be used to target all phone-owning households. In addition to applying sample weights from the survey, households that do not own a phone are downweighted so that the balance of phone owners to non-phone-owners (with sample weights applied) is the same as in the baseline survey as a whole (with sample weights applied, 84% phone owners).
3. *Full Sample:* All 2,814 households in the TUP baseline survey for which asset and consumption data are available, with sample weights applied.

Note that the quota used to evaluate targeting changes for each sample, based on the number of households that are ultra-poor in the sample. For the matched sample, the targeting quota is 27.29%; for the balanced sample and full sample the quotas are 5.47% and 6.02%, respectively.

3 Results

3.1 Performance of Targeting Methods

Our first set of results evaluate the extent to which different targeting methods can correctly identify ultra-poor households. This analysis compares the performance of CDR-based targeting methods to asset-based and consumption-based targeting, using the sample of 535 households for which survey data and CDR data are both available.

An overview of these results is provided in Figure 1. Figure 1a shows the distribution of assets and consumption, as well as the distribution of predicted probabilities of being non-ultra-poor generated by the CDR-based and combined methods, separately for the ultra-poor and non-ultra-poor. The dashed vertical line indicates the threshold at which point 27% of households are classified as ultra-poor; we use this quota because 27% of households in this sample were designed as ultra-poor by TUP. Figure 1b provides confusion matrices that compare the true status (rows) against the classification made by each method (columns). These confusion matrices are also used to calculate the measures of precision and recall reported in Table 2 Panel A.

We find that the CDR-based method (precision and recall of 42%) is close in accuracy to methods relying on assets (precision and recall of 49%) or consumption (precision and recall of 45%). To evaluate the trade-off between inclusion errors and exclusion errors resulting from selecting alternative cut-off thresholds, Figure 1c shows the ROC curve associated with each classification method, and the associated Area Under the Curve (AUC). AUC scores are comparable among methods, with assets (AUC=0.73) slightly superior to consumption (AUC=0.71) and the CDR-based method (AUC=0.68).

3.2 Comparison of Errors Across Methods

To better understand where the targeting methods are making mistakes, Panel A of Table 3 indicates how the ultra-poor misclassified as non-ultra-poor (false negatives) compare to the correctly classified ultra-poor (true positives). Panel B shows how the non-ultra-poor misclassified as ultra-poor (false positives) compare to the correctly classified non-ultra-poor (true negatives).

Across methods, false negatives score higher on food security, financial inclusion, and psychological well-being than true positives – that is, all three targeting methods misclassify ultra-poor households as non-ultra-poor when those ultra-poor households are better-off, according to other observable characteristics not used in the targeting. Likewise, false positives (non-ultra-poor misclassified as ultra-poor) tend to score lower than true negatives across these same measures. The CDR-based method in particular tends to prioritize households that score low on these alternative measures of well-being.

To test for systematic misclassification of certain types of households, Table 4 displays the overlap in errors of exclusion and inclusion between methods. Our results suggest that the three classifiers misidentify the same households at a rate only slightly above random.⁸

⁸The rates of overlap should be interpreted relative to the expected overlap in errors for random classifiers.

3.3 Combining Targeting Methods

Since the different targeting methods identify different populations as ultra-poor, there may be complementarities between asset, consumption, and CDR data. As shown in Panel A of Table 2, we find that a *combined method*, which takes as input the wealth index, total consumption, and the output of the CDR-based method, performs better (AUC = 0.78) than methods using any one data source (AUC = 0.68 - 0.73). As shown in Table S5), the full method also outperforms methods based on any two data sources (AUC = 0.75 - 0.76). The method that combines CDR and asset data (AUC = 0.76); may, however, be more practical than the combined method, since consumption data is difficult to collect for large populations.

3.4 Targeting Households Without Phones

An important limitation of CDR-based targeting is that households without phones do not generate CDR. Here, we show how targeting performance is impacted when households without phones are included in the analysis. This analysis uses two additional samples of TUP households to evaluate targeting performance: (i) the *balanced sample*, which adds all of the 472 households without phones to the sample of 535 for whom we have matched CDR; the balanced sample is intended to illustrate the performance of CDR-based targeting if CDR were available from all operators in Afghanistan — though it relies on the assumption that phone-owners observed on our mobile network are representative of all phone owners in Afghanistan (an assumption that is not fully satisfied, as shown in Table 1); and (ii) the *full sample*, which includes all 2,814 households surveyed in the TUP baseline; this sample includes an additional 1,807 households who report owning a phone, but whose number does not match to any number in the CDR provided to us by the single mobile operator.⁹

Results in Panels B and C of Table 2 show the performance of each targeting approach on the balanced and full sample, respectively. Note that as described in Section 2.4, different targeting quotas are applied for each panel based on the proportion of each sample that is ultra-poor. In the CDR-based and combined approaches, we report performance when the households without CDR are targeted first (i.e. households without CDR are targeted in a

Based on our selection of thresholds such that 27% of the sample is identified as ultra-poor, our three classifiers misidentify 15-27% of the non-ultra-poor and 51-65% of the ultra-poor. If these classifiers were random, we would expect approximately 20% overlap in inclusion errors and 55% overlap in exclusion errors.

⁹These 1,807 households include households that report owning a phone on a different network (this network is estimated to have around 30% market share in Afghanistan), as well as phones on our network that were not active during the six-month period of CDR that we analyze.

random order and then the households predicted to be poorest are targeted until the quota is reached) as well as when households without CDR are targeted last (i.e., after the 535 households with phones are targeted, households without phones are included in a random order until the quota is reached).

Unsurprisingly, these results suggest that CDR-based targeting is not effective when a large portion of the target population does not own a phone (e.g. Panel C of Table 2, where only 16% of the sample has matching CDR). However, when we simulate more realistic levels of phone ownership in Panel B (84% of the households, based on our survey data), CDR-based targeting is once again comparable to asset- or expenditure-based targeting, particularly when households without phones are targeted first (AUC = 0.72, 0.70, 0.68 for assets, consumption, and CDR, respectively). On the other hand, if households without phones are targeted last (for example, if program administrators base targeting wholly on CDR and provide no benefits to any household without a phone), the CDR-based method only improves marginally on random targeting.

3.5 Additional tests and simulations

Our main analysis considers the household head to be the unit of analysis. As described in Section 2.2, this analysis is based on matching survey-based indices to phone data from the household head, which is consistent with the design of the TUP program and the TUP survey sample frame. An alternative approach matches survey data reported by the household head to all phone numbers associated with the household. As shown in Table S6, the predictive accuracy of these models is slightly attenuated relative to the benchmark results (Table S3).

We also explore the extent to which CDR can be used to predict other measures of socioeconomic status. Our main analysis focuses on the household’s ultra-poor designation as the ground truth measure of poverty, since this label was both carefully curated and the actual criterion used to determine TUP eligibility. In Table S7, we report the accuracy with which CDR (obtained from the household head, who is typically male) can predict consumption and asset-based wealth (elicited from the primary woman of each household).¹⁰ In general, these machine learning models trained to directly predict consumption or asset-based wealth do not perform well. This result contrasts with prior work documenting the predictive ability of CDR for measuring asset-based wealth (e.g. [Blumenstock et al., 2015](#)). We suspect a key difference in our setting – aside from the fact that we are matching CDR to socioeconomic

¹⁰Due to the design of the TUP survey, which interviewed women in the household, we cannot avoid this mismatch between the survey respondent and the phone owner.

status at the *household* rather than the *individual* level – is the homogeneity of the beneficiary population: whereas [Blumenstock et al. \(2015\)](#) uses machine learning to predict the wealth of a nationally-representative sample of Rwandan phone owners, our sample consists of 535 individuals from the poorest villages of a single province in Afghanistan, where even the relatively wealthy households are quite poor.

4 Discussion

Our key finding is that, in a sample of 535 phone-owning households in poor villages in Afghanistan, machine learning methods leveraging phone data are nearly as accurate at identifying ultra-poor households as standard asset- and consumption-based methods. Further, we find that methods combining survey data with CDR perform better than methods using a single data source. However, as we demonstrate empirically, low rates of phone ownership — or the inability to access data from all operators — can undermine the value of CDR-based targeting. In our setting, the CDR-based approach still works well if households without phones are targeted before the CDR-based algorithm selects the poorest households with phones. However, this approach may not be appropriate in other contexts where phone ownership is less predictive of wealth, or where potential beneficiaries have the ability to strategically under-report phone ownership ([Björkegren et al., 2020](#)).

As mobile phone penetration rates continue to rise in LMICs ([GSMA, 2020](#)), and as programs increasingly rely on mobile phones and money to distribute benefits (see footnote ??), CDR-based targeting methods will likely play a more prominent role in the set of options considered by policymakers and program administrators — particularly in contexts like Afghanistan, where traditional targeting benchmarks are missing or of low quality. In just the past few years, for instance, data from mobile phone operators was used in the design of social assistance programs in Colombia, the Democratic Republic of the Congo, Pakistan, and Togo (see footnote ??). We conclude by highlighting a few policy important considerations for CDR-based targeting.

Speed and cost An advantage of CDR-based targeting is that it can be used in contexts where face-to-face contact is not feasible, dramatically reducing the time required to implement a targeted program. While it typically takes many months (or years) to implement a proxy-means test (PMT), community-based targeting (CBT), or consumption-based targeting, a CDR-based model can be trained in just a few weeks (see Appendix C). Likewise,

the marginal costs per household screened are substantially lower with CDR-based targeting than with CBT, PMT, or consumption-based targeting. For instance, Table S8 uses cost estimates obtained from the literature (and detailed in Table S9) to estimate targeting costs for the TUP program.¹¹ Whereas the marginal costs of screening an individual with a CBT or PMT are estimated at \$2.20 and \$4.00, respectively, the marginal cost of screening with CDR is negligible (see Appendix C).¹² For the entire TUP program, which screened around 125,721 households in six provinces, CBT and PMT would add an additional estimated \$276,586 and \$502,884, respectively, corresponding to 2.18% and 3.97% of the total program budget.

Data access and privacy Access to phone data is necessary for CDR-based targeting. As we show, targeting performance degrades considerably when CDR are not available for subsets of the population. Encouragingly, the past several years have been characterized by a trend towards public sector access to CDR, particularly in the context of the COVID-19 pandemic, during which mobile network operators shared CDR with governments, researchers, and NGOs for social protection purposes (cf. [Gentilini et al., 2020, 2021](#); [Aiken et al., 2021](#)). CDR have also been shared with the public sector for public health and humanitarian aid applications ([Milusheva et al., 2021](#)). Access issues aside, CDR contain private and sensitive data, including phone numbers and location traces. While much has been written about enabling responsible use of CDR for humanitarian response (e.g. [de Montjoye et al., 2018](#); [Oliver et al., 2020](#)), to date no consistent privacy standards exist. Informed consent can increase participant agency, but also complicates the implementation logistics. Likewise, there may be ways to incorporate differential privacy or other privacy enhancing technologies into a CDR-based targeting system, but such privatization would likely decrease targeting accuracy ([Hu et al., 2015](#)).

Algorithmic transparency and strategic behavior Using CDR to determine program eligibility may introduce incentives for people to manipulate their phone use. This consideration is not unique to CDR, as degrees of manipulation have been documented in social

¹¹In our cost calculations we obtain estimates for a CBT, rather than the hybrid approach used in the TUP program, as there is more information available on CBT-only costs in the literature. However, as the CBT cost can be interpreted as a lower bound for the cost of a hybrid approach, our qualitative results also apply to a hybrid approach.

¹²Marginal costs of CDR-based targeting are negligible because we assume no contact with screened individuals is required. In practice, it may be desirable to solicit informed consent to access CDR. If consent were collected in-person, the marginal costs would approach that of a PMT; if collected over the phone, there would still be significant cost savings, see Appendix Section C).

programs that use proxy means tests and other traditional targeting mechanisms (Camacho & Conover, 2011; Banerjee et al., 2018). Indeed, complex and non-linear machine learning algorithms, like the one presented in this paper, may obfuscate the logic behind targeting decisions, reducing the scope for manipulation. However, society often demands transparency in algorithmic decision-making, as black-box decisions are difficult to audit or hold to account. There is therefore a tension between the goals of increasing transparency and reducing manipulation, though recent advances in machine learning explore mechanisms for pursuing both objectives at once (Björkegren et al., 2020).

Centralized vs. local knowledge CDR-based methods enable a top-down, centralized and standardized approach to program targeting, rather than a bottom-up approach that prioritizes local knowledge that can be elicited, for example, through community wealth rankings. While the empirical results in this paper indicate that the efficiency gains from CDR-based targeting are substantial, it may reinforce existing power structures (Taylor, 2016; Blumenstock, 2018a; Abebe et al., 2021). Efficiency gains should also be considered within the context of evidence suggesting that participating communities may prefer community-based approaches (Alatas et al., 2012), but also may perceive them as less legitimate (Premand & Schnitzer, 2020).

To summarize, our results suggest that there is potential for using CDR-based methods to determine eligibility for economic aid or interventions, substantially reducing program targeting overhead and costs. Our results also indicate that CDR-based methods may complement and enhance existing survey-based methods. We note, however, that the practical and ethical limitations to CDR-based targeting are significant. We emphasize the need to consider these limitations and the constraints of specific local contexts alongside the efficiency gains offered by CDR-based targeting.

References

- Abebe, R., Aruleba, K., Birhane, A., Kingsley, S., Obaido, G., Remy, S. L., & Sadagopan, S. (2021, March). Narratives and Counternarratives on Data Sharing in Africa. In *Proceedings of the 2021 ACM Conference on Fairness, Accountability, and Transparency* (pp. 329–341). New York, NY, USA: Association for Computing Machinery. Retrieved 2021-03-04, from <https://doi.org/10.1145/3442188.3445897> doi: 10.1145/3442188.3445897
- Aiken, E., Bellue, S., Karlan, D., Udry, C. R., & Blumenstock, J. (2021, July). *Machine Learning and Mobile Phone Data Can Improve the Targeting of Humanitarian Assistance* (Working Paper No. 29070). National Bureau of Economic Research. Retrieved from <https://www.nber.org/papers/w29070> doi: 10.3386/w29070
- Alatas, V., Banerjee, A., Hanna, R., Olken, B., & Tobias, J. (2012). Targeting the poor: Evidence from a field experiment in Indonesia. *American Economic Review*, *102*(4), 1206–1240.
- Ali, S., & May, M. (2021). *Bangladesh’s covid-19 response is taking digital finance to new levels*. <https://www.cgap.org/blog/bangladeshs-covid-19-response-taking-digital-finance-new-levels>.
- Banerjee, A., Duflo, E., Chattopadhyay, R., & Shapiro, J. (2007). Targeting efficiency: How well can we identify the poor? *Institute for Financial Management and Research Centre for Micro Finance, Working Paper Series No. 21*.
- Banerjee, A., Hanna, R., Olken, B. A., & Sumarto, S. (2018, December). *The (lack of) Distortionary Effects of Proxy-Means Tests: Results from a Nationwide Experiment in Indonesia* (Working Paper No. 25362). National Bureau of Economic Research. Retrieved from <http://www.nber.org/papers/w25362> doi: 10.3386/w25362
- Bedoya, G., Coville, A., Haushofer, J., Isaqzadeh, M., & Shapiro, J. (2019). No household left behind: Afghanistan targeting the ultra poor impact evaluation. *World Bank Policy Research Working Paper*, *8877*.
- Björkegren, D., Blumenstock, J. E., & Knight, S. (2020). Manipulation-proof machine learning. *arXiv preprint arXiv:2004.03865*.
- Blumenstock, J. (2016). Fighting poverty with data. *Science*, *353*, 753–754.

- Blumenstock, J. (2018a). Don't forget people in the use of big data for development. *Nature*, *561*, 170-172.
- Blumenstock, J. (2018b). Estimating economic characteristics with phone data. *American Economic Review: Papers and Proceedings*, *108*, 72-76.
- Blumenstock, J. (2020, May). Machine learning can help get COVID-19 aid to those who need it most. *Nature*. Retrieved 2020-05-15, from <https://www.nature.com/articles/d41586-020-01393-7> doi: 10.1038/d41586-020-01393-7
- Blumenstock, J., Cadamuro, G., & On, R. (2015). Predicting poverty and wealth from mobile phone data. *Science*, *350*, 1073-1076.
- Brown, C., Ravallion, M., & van de Walle, D. (2018). A poor means test? econometric targeting in Africa. *Journal of Development Economics*, *134*, 109-124.
- Burke, M., Driscoll, A., Lobell, D. B., & Ermon, S. (2021). Using satellite imagery to understand and promote sustainable development. *Science*, *371*(6535).
- Camacho, A., & Conover, E. (2011, May). Manipulation of Social Program Eligibility. *American Economic Journal: Economic Policy*, *3*(2), 41-65. Retrieved 2017-09-25, from <https://www.aeaweb.org/articles?id=10.1257/pol.3.2.41> doi: 10.1257/pol.3.2.41
- Chi, G., Fang, H., Chatterjee, S., & Blumenstock, J. E. (2020). Micro-Estimates of Wealth for the Developing World. *Revise and Resubmit, Nature*.
- Coady, D., Grosh, M., & Hoddinott, J. (2004). Targeting outcomes redux. *The World Bank Research Observer*, *19*(1).
- Corral, P., Irwin, A., Krishnan, N., & Mahler, D. G. (2020). *Fragility and conflict: on the front lines of the fight against poverty*. World Bank Publications.
- Deaton, A. (2016, June). Measuring and understanding behavior, welfare, and poverty. *American Economic Review*, *106*(6), 1221-43. Retrieved from <https://www.aeaweb.org/articles?id=10.1257/aer.106.6.1221> doi: 10.1257/aer.106.6.1221
- de Montjoye, Y., Gambs, S., Blondel, V., Canright, G., De Cordes, N., Deletaille, S., ... others (2018). On the privacy-conscious use of mobile phone data. *Scientific data*, *5*(1), 1-6.

- de Montjoye, Y., Rocher, L., & Pentland, A. (2016). bandicoot: a python toolbox for mobile phone metadata. *Journal of Machine Learning Research*, *17*, 1-5.
- Engstrom, R., Hersh, J. S., & Newhouse, D. L. (2017). Poverty from space: using high-resolution satellite imagery for estimating economic well-being. *World Bank Policy Research Working Paper*(8284).
- Fatehkia, M., Tingzon, I., Orden, A., Sy, S., Sekara, V., Garcia-Herranz, M., & Weber, I. (2020). Mapping socioeconomic indicators using social media advertising data. *EPJ Data Science*, *9*(1), 22.
- Filmer, D., & Pritchett, L. (2001). Wealth effects without expenditure data—or tears: An application to educational enrollments in states of India. *Demography*, *39*, 115-132.
- Fortin, S., Kameli, Y., Kone, K., Belem, B., Sangho, H., & Savy, M. (2018). Targeting vulnerable households in rural mali: Effectiveness of a community-based methodology, with or without addition of a proxy-mean test, 2016. *Revue d'Épidémiologie et de Santé Publique*, *66*, S353. Retrieved from <https://www.sciencedirect.com/science/article/pii/S0398762018310174> (European Congress of Epidemiology “Crises, epidemiological transitions and the role of epidemiologists”) doi: <https://doi.org/10.1016/j.respe.2018.05.317>
- Gentilini, U., Almenfi, M., Orton, I., & Dale, P. (2020, May). Social Protection and Jobs Responses to COVID-19: A Real-Time Review of Country Measures. *World Bank Policy Brief*. Retrieved from <https://openknowledge.worldbank.org/handle/10986/33635>
- Gentilini, U., Khosla, S., & Almenfi, M. (2021). Cash in the city.
- Grosh, M., & Baker, J. L. (1995). Proxy means tests for targeting social programs. *Living standards measurement study working paper*, *118*, 1–49.
- Grosh, M., Leite, P., & Wai-Poi, M. (in press). *A New Look at Old Dilemmas: Revisiting Targeting in Social Assistance*. The World Bank.
- GSMA. (2020). *Mobile economy*. https://www.gsma.com/mobileeconomy/wp-content/uploads/2020/03/GSMA_MobileEconomy2020_Global.pdf.
- Hanna, R., & Olken, B. (2018). Universal basic incomes versus targeted transfers: Anti-poverty programs in developing countries. *Journal of Economic Perspectives*, *32*, 201-226.

- Hernandez, M., Hong, L., Frias-Martinez, V., & Frias-Martinez, E. (2017). Estimating poverty using cell phone data: evidence from Guatemala. , *World Bank Policy Research Working Paper Series No. 7969*.
- Hu, X., Yuan, M., Yao, J., Deng, Y., Chen, L., Yang, Q., ... Zeng, J. (2015). Differential privacy in telco big data platform. *Proceedings of the VLDB Endowment*, 8(12), 1692–1703.
- Jean, N., Burke, M., Xie, M., Davis, W. M., Lobell, D. B., & Ermon, S. (2016, August). Combining satellite imagery and machine learning to predict poverty. *Science*, 353(6301), 790–794. Retrieved 2016-10-07, from <http://science.sciencemag.org/content/353/6301/790> doi: 10.1126/science.aaf7894
- Jerven, P. (2013). *Poor numbers*. Cornell University Press.
- Karlan, D., Lowe, M., Osei, R., Osei-Akoto, I., Roth, B., & Udry, C. (2021). *Cash transfers as covid-19 relief: Evidence from ghana*. <https://www.theigc.org/blog/cash-transfers-as-covid-19-relief-evidence-from-ghana/>.
- Karlan, D., & Thuysbaert, B. (2019). Targeting ultra-poor households in Honduras and Peru. *The World Bank Economic Review*, 33(1), 63-94.
- Lindert, K., Karippacheril, T. G., Caillava, I. R., & Chávez, K. N. (2020). *Sourcebook on the foundations of social protection delivery systems*. World Bank Publications.
- Milusheva, S., Lewin, A., Gomez, T. B., Matekenya, D., & Reid, K. (2021). Challenges and opportunities in accessing mobile phone data for covid-19 response in developing countries. *Data & Policy*, 3.
- Oliver, N., Lepri, B., Sterly, H., Lambiotte, R., Deletaille, S., De Nadai, M., ... others (2020). *Mobile phone data for informing public health actions across the covid-19 pandemic life cycle* (Vol. 6) (No. 23). American Association for the Advancement of Science.
- Paul, B. V., Msowoya, C., Archibald, E., Sichinga, M., Peredo, A. C., & Malik, M. A. A. (2021). *Malawi covid-19 urban cash intervention process evaluation report*. World Bank Publications.
- Pokhriyal, N., & Jacques, D. (2017). Combining disparate data sources for improved poverty prediction and mapping. *Proceedings of the National Academy of Sciences*, 114, E9783–E9792.

- Premand, P., & Schnitzer, P. (2020, 09). Efficiency, Legitimacy, and Impacts of Targeting Methods: Evidence from an Experiment in Niger. *The World Bank Economic Review*. Retrieved from <https://doi.org/10.1093/wber/lhaa019> (lhaa019) doi: 10.1093/wber/lhaa019
- Schnitzer, P., & Stoeffler, Q. (2021). Targeting for social safety nets.
- Sheehan, E., Meng, C., Tan, M., Uz Kent, B., Jean, N., Lobell, D., ... Ermon, S. (2019). Predicting economic development using geolocated wikipedia articles. *Proceedings of the 25th ACM SIGKDD Conference on Knowledge Discovery and Data Mining*.
- Steele, J., Sundsøy, P., Pezzulo, C., Alegana, V., Bird, T., Blumenstock, J., ... Bengtsson, L. (2017). Mapping poverty using mobile phone and satellite data. *Journal of the Royal Society Interface*, 14.
- Taylor, L. (2016). No place to hide? the ethics and analytics of tracking mobility using mobile phone data. *Environment and Planning D: Society and Space*, 34(2), 319–336.
- USAID. (2021). *Usaid's direct cash transfer program helps over 85,000 vulnerable liberians cope with economic fallout from covid-19*. <http://web.archive.org/web/20080207010024/http://www.808multimedia.com/winnt/kernel.htm>.
- World Bank. (2020). *Poverty and Shared Prosperity 2020 : Reversals of Fortune*. The World Bank. Retrieved from <https://openknowledge.worldbank.org/handle/10986/34496> (License: CC BY 3.0 IGO.)

Tables and Figures

Table 1: Summary statistics for different samples of survey respondents

Outcome	(1) Full sample (all observations)	(2) Matched Subsample	(3) Unmatched Owns Phone	(4) Unmatched No Phone
<i>Panel A: Balance of Covariates</i>				
Ultra-Poor	0.42 (0.49)	0.27 (0.45)	0.40 (0.49)	0.66 (0.47)
Asset Index	0.00 (2.01)	1.36 (2.60)	-0.05 (1.76)	-1.35 (0.79)
Log Expenditures	4.43 (0.71)	4.64 (0.70)	4.46 (0.70)	4.12 (0.65)
# Phones	1.35 (1.18)	1.72 (1.33)	1.59 (1.04)	0.00 (0.00)
Food Security Index	0.30 (0.90)	0.35 (0.74)	0.34 (0.93)	0.10 (0.89)
Financial Inclusion Index	0.15 (1.27)	0.34 (1.39)	0.15 (1.32)	-0.05 (0.79)
Psychological Well-being Index	0.35 (1.01)	0.38 (1.00)	0.43 (0.97)	-0.02 (1.07)
CWR Group	0.62 (0.90)	0.89 (1.02)	0.62 (0.88)	0.26 (0.66)
<i>Panel B: Correlations Between Outcomes</i>				
Ultra-Poor \longleftrightarrow Asset Index	-0.32	-0.30	-0.27	-0.14
Ultra-Poor \longleftrightarrow Consumption	-0.39	-0.30	-0.39	-0.26
Asset Index \longleftrightarrow Consumption	0.37	0.34	0.34	0.15
<i>N</i>	2,814	535	1,807	472

Notes: Table reports average characteristics, with standard deviations in parentheses, of TUP survey respondents. Each column represents a different sample of respondents: (1) all respondents in the TUP survey; (2) Just those respondents who own a phone, where the phone number matches to the CDR obtained from the mobile phone operator; (3) Respondents who report owning a phone, but whose phone number does not match to the CDR obtained from the operator; (4) Respondents who report they do not own a phone.

Table 2: Targeting simulation results

Targeting Method	(1) AUC	(2) Accuracy	(3) Precision	(4) Recall
<i>Panel A: Matched Sample (N=535) - for whom we have survey and CDR data</i>				
Random	0.50 (0.028)	0.60 (0.025)	0.27 (0.038)	0.27 (0.038)
Asset Index	0.73 (0.024)	0.72 (0.020)	0.49 (0.041)	0.49 (0.041)
Consumption	0.71 (0.026)	0.69 (0.023)	0.45 (0.038)	0.45 (0.038)
CDR	0.68 (0.027)	0.69 (0.021)	0.42 (0.042)	0.42 (0.042)
Combined	0.78 (0.022)	0.75 (0.020)	0.55 (0.039)	0.55 (0.039)
<i>Panel B: Balanced Sample (N=1,007) - as above, plus households without phones</i>				
Random	0.50 (0.017)	0.90 (0.006)	0.05 (0.010)	0.05 (0.010)
Asset Index	0.72 (0.026)	0.90 (0.006)	0.10 (0.013)	0.10 (0.013)
Consumption	0.70 (0.028)	0.90 (0.006)	0.15 (0.025)	0.15 (0.025)
CDR (Target Phoneless First)	0.68 (0.030)	0.90 (0.006)	0.11 (0.035)	0.11 (0.035)
CDR (Target Phoneless Last)	0.51 (0.028)	0.90 (0.006)	0.12 (0.033)	0.12 (0.033)
Combined (Target Phoneless First)	0.74 (0.026)	0.90 (0.006)	0.11 (0.046)	0.11 (0.046)
Combined (Target Phoneless Last)	0.57 (0.022)	0.90 (0.006)	0.18 (0.007)	0.18 (0.007)
<i>Panel C: Full Sample (N=2,814) - as above, plus households with phones on other networks</i>				
Random	0.50 (0.009)	0.89 (0.005)	0.06 (0.007)	0.06 (0.007)
Asset Index	0.65 (0.017)	0.89 (0.005)	0.07 (0.014)	0.07 (0.014)
Consumption	0.69 (0.015)	0.89 (0.006)	0.08 (0.031)	0.08 (0.031)
CDR (Target Phoneless First)	0.52 (0.008)	0.89 (0.005)	0.06 (0.008)	0.06 (0.008)
CDR (Target Phoneless Last)	0.48 (0.008)	0.89 (0.005)	0.08 (0.010)	0.08 (0.010)
Combined (Target Phoneless First)	0.52 (0.008)	0.89 (0.005)	0.06 (0.008)	0.06 (0.008)
Combined (Target Phoneless Last)	0.49 (0.008)	0.89 (0.005)	0.09 (0.009)	0.09 (0.009)

Notes: Four different measures of performance (columns) reported for different targeting methods (rows), using different samples of survey respondents (panels). Standard deviations, calculated using 1,000 bootstrap samples, in parentheses. Panel A: The 535-household subsample that is matched to CDR. Panel B: The 535-household matched sample, plus the 472 households that do not have a phone; this is meant to approximate targeting performance if CDR from all mobile networks were available. Sample weights are applied as described in Section 2.4. Panel C: All 2,814 observations from the TUP survey, including households matched to CDR, households that own phones not matched to CDR, and households without phones, with sample weights applied. For Panels B and C, we simulate two types of CDR-based targeting: targeting households without phones first and targeting households without phones last.

Table 3: What types of households are misclassified?

<i>Panel A: Ultra-Poor Households (Differences Between True Positives and False Negatives)</i>									
	Asset Index			Consumption			CDR		
	TP	FN	Diff.	TP	FN	Diff.	TP	FN	Diff.
Ultra-Poor	1.00 (0.00)	1.00 (0.00)	0.00 (0.00)	1.00 (0.00)	1.00 (0.00)	0.00 (0.00)	1.00 (0.00)	1.00 (0.00)	0.00 (0.00)
Asset Index	-1.03 (0.49)	1.18 (1.34)	-2.21 (0.17)	-0.34 (1.09)	0.47 (1.69)	-0.81 (0.23)	-0.09 (1.16)	0.25 (1.70)	-0.34 (0.24)
Consumption	4.21 (0.70)	4.40 (0.62)	-0.19 (0.11)	3.78 (0.32)	4.74 (0.56)	-0.96 (0.07)	4.29 (0.60)	4.32 (0.71)	-0.02 (0.11)
# Phones	0.89 (0.68)	1.63 (1.12)	-0.74 (0.15)	1.02 (0.73)	1.48 (1.14)	-0.46 (0.16)	1.18 (0.61)	1.33 (1.21)	-0.16 (0.15)
Food Security Index	-0.59 (1.13)	-0.51 (1.10)	-0.08 (0.18)	-0.83 (1.19)	-0.32 (0.99)	-0.51 (0.18)	-0.51 (1.14)	-0.58 (1.09)	0.07 (0.19)
Financial Inclusion Index	-0.00 (0.79)	0.29 (1.02)	-0.29 (0.15)	0.10 (0.80)	0.19 (1.02)	-0.09 (0.15)	0.16 (0.98)	0.14 (0.88)	0.02 (0.16)
Psychological Well-being Index	-0.35 (0.92)	-0.13 (0.94)	-0.22 (0.15)	-0.37 (0.86)	-0.12 (0.98)	-0.24 (0.15)	-0.31 (0.81)	-0.17 (1.02)	-0.14 (0.15)
CWR Group	0.09 (0.44)	0.01 (0.12)	0.07 (0.05)	0.02 (0.12)	0.08 (0.41)	-0.06 (0.05)	0.06 (0.40)	0.04 (0.24)	0.03 (0.06)

<i>Panel B: Non-Ultra-Poor Households (Differences Between True Negatives and False Positives)</i>									
	Asset Index			Consumption			CDR		
	TN	FP	Diff.	TN	FP	Diff.	TN	FP	Diff.
Ultra-Poor	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Asset Index	2.53 (2.62)	-1.08 (0.50)	3.61 (0.16)	2.06 (2.92)	0.94 (1.75)	1.12 (0.26)	1.94 (2.87)	1.43 (2.27)	0.51 (0.30)
Consumption	4.82 (0.66)	4.57 (0.65)	0.25 (0.08)	4.97 (0.58)	3.98 (0.23)	0.99 (0.04)	4.78 (0.68)	4.74 (0.61)	0.04 (0.08)
# Phones	2.11 (1.43)	0.96 (0.76)	1.15 (0.12)	1.98 (1.49)	1.52 (0.92)	0.46 (0.13)	1.91 (1.44)	1.80 (1.24)	0.11 (0.16)
Food Security Index	0.24 (0.87)	-0.16 (1.03)	0.40 (0.13)	0.24 (0.88)	-0.14 (0.99)	0.37 (0.12)	0.15 (0.91)	0.18 (0.94)	-0.02 (0.12)
Financial Inclusion Index	0.80 (4.92)	-0.01 (0.82)	0.82 (0.29)	0.77 (4.94)	0.18 (1.24)	0.59 (0.31)	0.78 (4.98)	0.17 (1.10)	0.61 (0.31)
Psychological Well-being Index	0.69 (0.97)	0.21 (0.75)	0.47 (0.10)	0.62 (0.98)	0.49 (0.80)	0.13 (0.11)	0.62 (0.95)	0.49 (0.93)	0.13 (0.12)
CWR Group	1.30 (1.00)	0.84 (0.96)	0.46 (0.12)	1.23 (1.03)	1.13 (0.94)	0.10 (0.12)	1.26 (1.01)	1.01 (0.98)	0.25 (0.12)

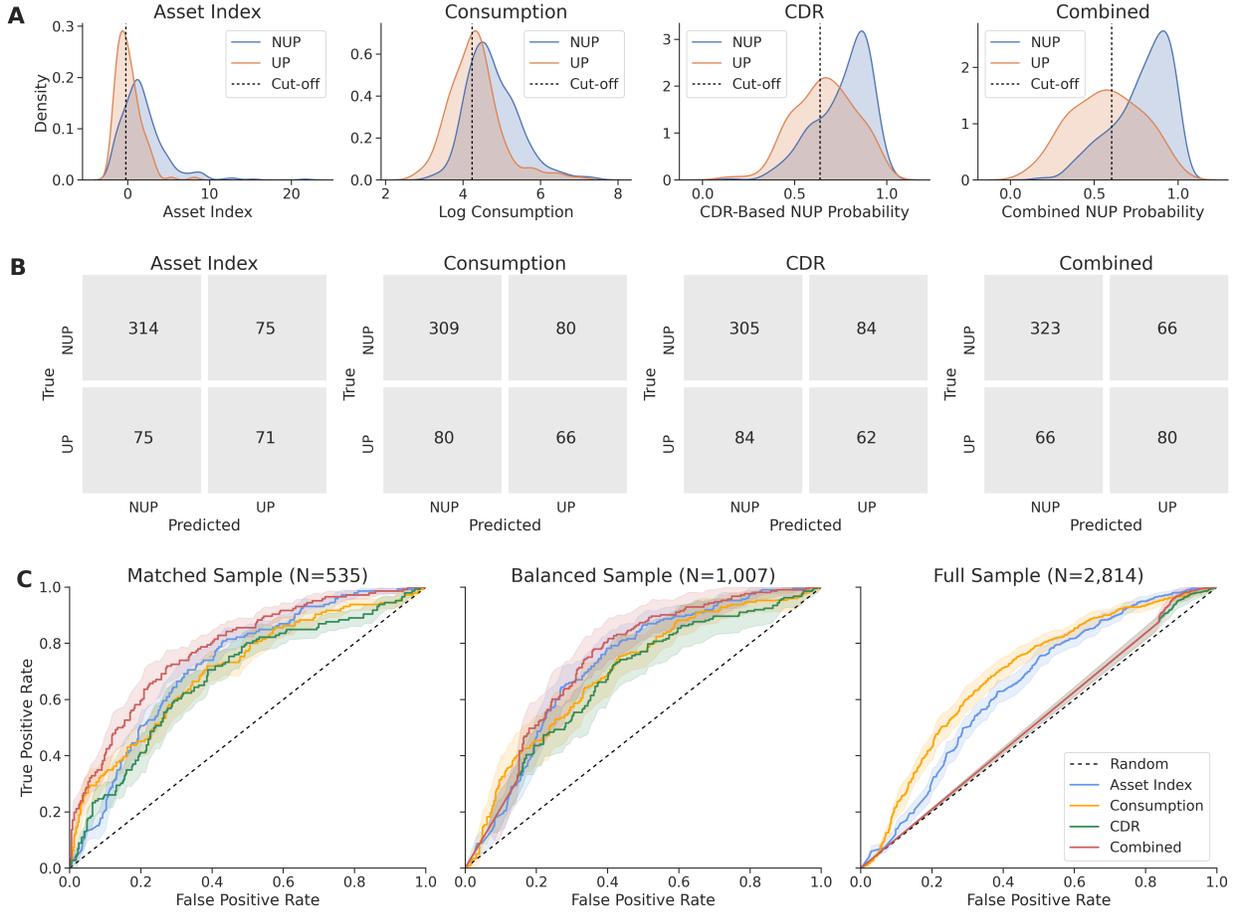
Notes: Table shows the average characteristics (with standard deviations in parentheses) of households that are correctly classified (True Positives [TP] and True Negatives [TN]) and incorrectly classified (False Negatives [FN] and False Positives [FP]), as well as the difference in average characteristics between correctly and incorrectly classified households (Diff.). Panel A: Differences between ultra-poor households correctly classified as such and those misclassified as non-ultra-poor (errors of exclusion). Panel B: Differences between non-ultra-poor households correctly classified as such and those misclassified as ultra-poor (errors of inclusion).

Table 4: Overlap in targeting errors between methods

	Asset Index	Consumption	CDR	Combined
<i>Panel A: Overlap in Errors of Exclusion</i>				
Asset Index	100.00%	65.33%	57.33%	66.67%
Consumption	61.25%	100.00%	56.25%	62.50%
CDR	51.19%	53.57%	100.00%	63.10%
Combined	75.76%	75.76%	80.30%	100.00%
<i>Panel B: Overlap in Errors of Inclusion</i>				
Asset Index	100.00%	26.67%	22.67%	48.00%
Consumption	25.00%	100.00%	16.25%	37.50%
CDR	20.24%	15.48%	100.00%	46.43%
Combined	54.55%	45.45%	59.09%	100.00%

Notes: Table measures the extent to which the targeting errors produced by each pair of targeting methods overlap. Evaluation is performed on the matched sample of 535 TUP respondents. Panel A: Overlap between ultra-poor households that are misclassified as non-ultra-poor (errors of exclusion) for each targeting method. Panel B: Overlap between non-ultra-poor households that are misclassified as ultra-poor (errors of inclusion).

Figure 1: Predicting ultra-poor status from CDR



Notes: Panel A: Comparing the predictive accuracy of assets, consumption, and CDR-based methods for identifying the ultra-poor in our 535-household sample. To adjust for class balance, thresholds for classification (shown in dashed black vertical lines) are selected such that the correct number of households are identified as ultra-poor. Panel B: Confusion matrices showing the targeting accuracy of each method shown in Panel A. Panel C: ROC curves for each of the four targeting methods. In the third subplot, the CDR-based and combined methods target non-phone-owning households first as described in Section 2.4

Online Appendix

A Machine learning methods and hyperparameters

Although our paper is focused on identifying the ultra-poor with CDR, we experiment with predicting four measures of ground-truth welfare with CDR features: ultra-poor status (binary), below the national poverty line (binary), asset index (continuous), and log consumption (continuous). For the binary measures, we experiment with four classification models: logistic regression (unregularized), logistic regression with L1 penalty, a random forest, and a gradient boosting model. For the continuous measures, we experiment with four regression models: linear regression, LASSO regression, a random forest, and a gradient boosting model. The linear models and random forest are implemented in Python’s scikit-learn package. The gradient boosting model is implemented with Microsoft’s LightGBM.

In each case, we produce predictions out-of-sample over 10-fold cross validation. We use nested cross-validation to tune the hyperparameters of each model over 5-fold cross-validation within each of the outer folds to avoid any information leakage between folds. We report both the mean score across the 10 folds as well as the overall score when data from all folds is pooled together. For the linear models and random forest, missing data is mean-imputed and each feature is scaled to zero mean and unit variance before fitting models (these transformations are done separately for each fold, with parameters fitted only on the training data for each fold). For the gradient boosting model missing values are left as-is and features are not scaled. We re-fit the model on the entire data, again tuning hyperparameters over 5-fold cross validation, to report selected hyperparameters and feature importances. We also report the top 5 features for each model, determined by the magnitude of the coefficient for the linear models, and by and by maximum impurity reductions for the tree-based models.

Hyperparameters are selected from the following grids for each model:

Linear/Logistic Regression

- Drop columns with missingness over: {50%, 80%, 100%}
- Drop columns with variance under: {0, 0.01, 0.1}
- Winsorization limit: {0%, 1%, 5%}

LASSO Regression

- Drop columns with missingness over: {50%, 80%, 100%}
- Drop columns with variance under: {0, 0.01, 0.1}
- Winsorization limit: {0%, 1%, 5%}
- L1 penalty: {0.00001, 0.0001, 0.001, 0.01, 0.1, 1, 10, 100}

Random Forest

- Drop columns with missingness over: {50%, 80%, 100%}
- Drop columns with variance under: {0, 0.01, 0.1}
- Winsorization limit: {0%, 1%, 5%}
- Number of Trees: {20, 50, 100}
- Maximum Depth: {1, 2, 4, 6, 8, 10, 12}

Gradient Boosting Model

- Drop columns with missingness over: {50%, 80%, 100%}
- Drop columns with variance under: {0, 0.01, 0.1}
- Winsorization limit: {0%, 1%, 5%}
- Number of Trees: {20, 50, 100}
- Minimum data in leaf: {5, 10}
- Number of leaves: {5, 10, 20}
- Learning rate: {0.05, 0.075}

B Abbreviations in Feature Names

Figure S4 and Tables S7, S2, and S6 use a set of abbreviations in CDR feature names. This appendix lists the relevant abbreviations.

- BOC: Balance of contacts
- CD: Call duration
- IPC: Interactions per contact
- IT: Interevent time
- NOI: Number of interactions
- PPD: Percent pareto durations (percentage of call contacts accounting for 80% of call time)
- PPI: Percent pareto interactions (percentage of contacts accounting for 80% of subscriber’s interactions)
- RD: Response delay
- RR: Response rate
- WD: Weekday
- WE: Weekend

C Cost and Speed Calculations

In the discussion section we provide a cost and speed comparison between targeting methods, as some of the value-add of the phone-based targeting approach relies on how cheap and quick it is compared to asset, consumption- or CBT-based targeting approaches. Administrative data on targeting costs was not collected as part of the TUP program, so we turn to other studies of program targeting to estimate the costs of CBT and asset-based (or PMT) methods. We treat the costs of an asset index-based and PMT approach as equivalent in this section, as they both require comprehensive household surveys.¹³ We identify

¹³In practice, an asset-based approach may be slightly cheaper than a PMT, as it does not require conducting a consumption module for a subset of surveys to train a PMT.

three studies that provide variable targeting costs for PMT and CBT methods: [Alatas et al. \(2012\)](#) provide variable costs for CBT and PMT-based targeting of a single program in Indonesia; [Karlan & Thuysbaert \(2019\)](#) provide variable costs for CBT and PMT-based targeting of an ultra-poor program in Honduras and one in Peru; and [Schnitzer & Stoeffler \(2021\)](#) provide variable costs for three CBT-based programs and four PMT-based programs in seven countries in Sub-Saharan Africa.¹⁴ Table S9 summarizes the cost estimates from each of these papers; we use the median per-household targeting cost for each method in our analysis (\$2.20 per household for CBT and \$4.00 per household for PMT). While using these global estimates to inform our model of targeting costs in Afghanistan is not ideal, since no data on targeting costs from the TUP program or other anti-poverty programs is available for the country, these values are the best available estimate on which to base our cost analysis.

We are unable to find any papers that document the targeting cost associated with consumption-based targeting, as consumption data is rarely used as a real-world targeting strategy. We therefore consider the costs of consumption to be strictly greater than the costs of targeting on a PMT, since consumption modules take longer to collect than PMT data in household surveys. In practice, we expect that the cost of targeting on consumption would be substantially greater than the cost of targeting on a PMT.

For phone-based targeting, we associate no cost with the collection and analysis of phone data. While in some cases phone data may require purchase from the operator, partnerships between mobile network operators and governments for social protection and public health applications have not, to date, involved payment ([Milusheva et al., 2021](#)). The fixed cost of mobile data analysis is non-negligible but its contribution to marginal cost is close to zero as the number of screened households increases. A phone-based targeting method that collects informed consent from program applicants to analyze phone data would have nonzero marginal costs, though the cost of consent would depend on the modality of consent collection. If consent was collected in person, these costs would likely be only slightly lower than those of a PMT, as every household would need to be surveyed in person. If consent was collected over the phone via SMS or voice, these costs would likely be significantly lower.

It is worth noting that our benchmark in this paper is the hybrid model with a CBT plus verification component, but due to limited estimates in the literature we leave this strategy out of our cost analysis. We consider the CBT a lower-bound estimate for the hybrid strategy, and therefore our results would be qualitatively unchanged if the hybrid

¹⁴To our knowledge, no studies incorporate fixed targeting costs, as these are typically indistinguishable from fixed costs of other components of program set-up.

strategy were also considered in cost comparison. [Alatas et al. \(2012\)](#) suggest that there are synergies in targeting approaches so that combining approaches is less costly than the sum of the costs of the two approaches individually, but costs are certainly greater than that of CBT targeting alone.

Our cost analysis finally relies upon administrative data from the TUP program. The TUP program in its entirety served 7,500 households across six provinces of Afghanistan. While there is no data available on the total number of households screened by the TUP program, the portion of the program in Balkh province that was enrolled in the RCT identified 1,235 ultra-poor households out of 20,702 households screened ([Bedoya et al., 2019](#)). Assuming similar eligibility rates across Afghanistan, we estimate that the TUP program as a whole likely screened around 125,721 households. We use this value to estimate total targeting costs for the TUP program under counterfactual targeting approaches. Eligible households received benefits totaling \$1,688, including a productive asset, cash transfers, a health voucher, training, biweekly social worker visits, and veterinarian visit once every two months during the year of intervention. The total benefits dispersed by the program were therefore on the order of 12.7 million (although the total program costs, including overhead, were closer to 15 million ([Bedoya et al., 2019](#))); we use the total value of benefits to compare the costs of program targeting using our set of counterfactual targeting approaches to the direct costs of program benefits in Table S8. We find that targeting costs for a PMT or asset-based approach would represent approximately 3.97% of the total benefits delivered in the program; costs for a CBT approach would represent approximately 2.18% of the total benefits. In comparison, costs for the phone-based approach would be negligible.

When it comes to speed, in-person data collection for an asset-based (or PMT) targeting approach typically takes months or years to prepare and implement ([World Bank, 2020](#)). The CDR-based approach can be rolled out comparatively quickly — but there are still practical hurdles to implementation. First, training data for the CDR-based poverty prediction model must be collected, preferably shortly prior to program roll-out ([Aiken et al., 2021](#)). While in the TUP project training data was collected in-person in a household survey, in other contexts training data collection was expedited via a phone survey ([Blumenstock et al., 2015](#); [Aiken et al., 2021](#)). Even if data is collected over the phone, it will typically take several weeks to design a survey instrument and collect data. Second, the CDR-based method requires data from mobile network operators. Data sharing agreements with mobile network operators take at minimum a few weeks to arrange, and substantially longer in the worst case ([Milusheva et al., 2021](#)). Third, and finally, training a CDR-based poverty prediction model is expensive in

terms of memory, computing power, and human capacity, and will likely take several weeks to implement.

Supplementary Tables and Figures

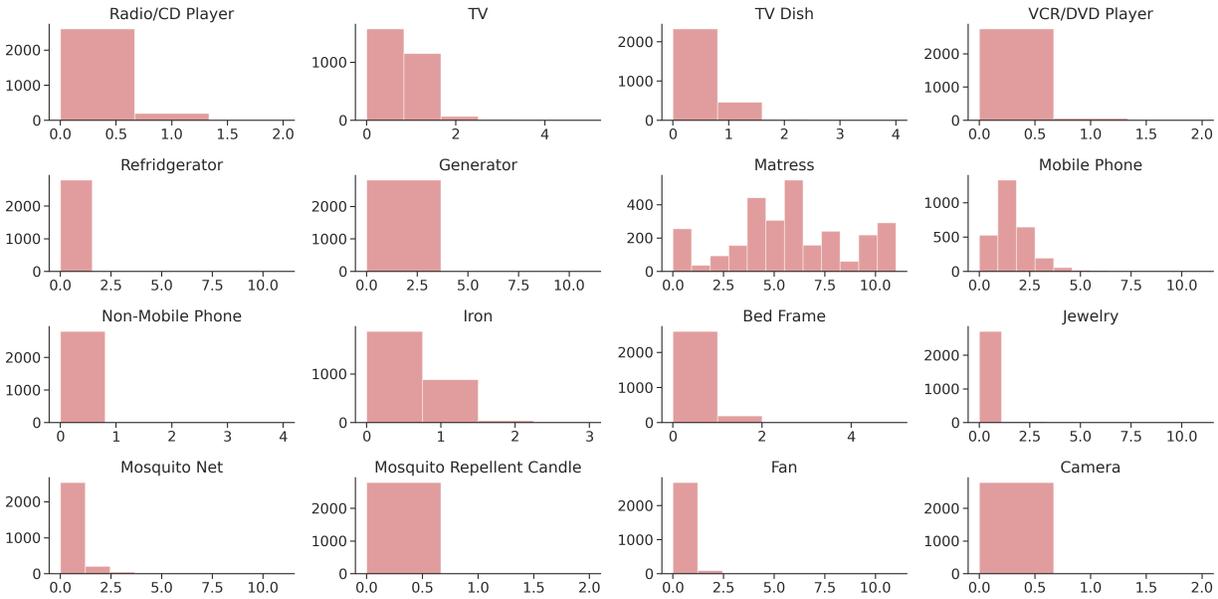


Figure S1: Histograms showing the distribution of each underlying asset used to construct the asset index.

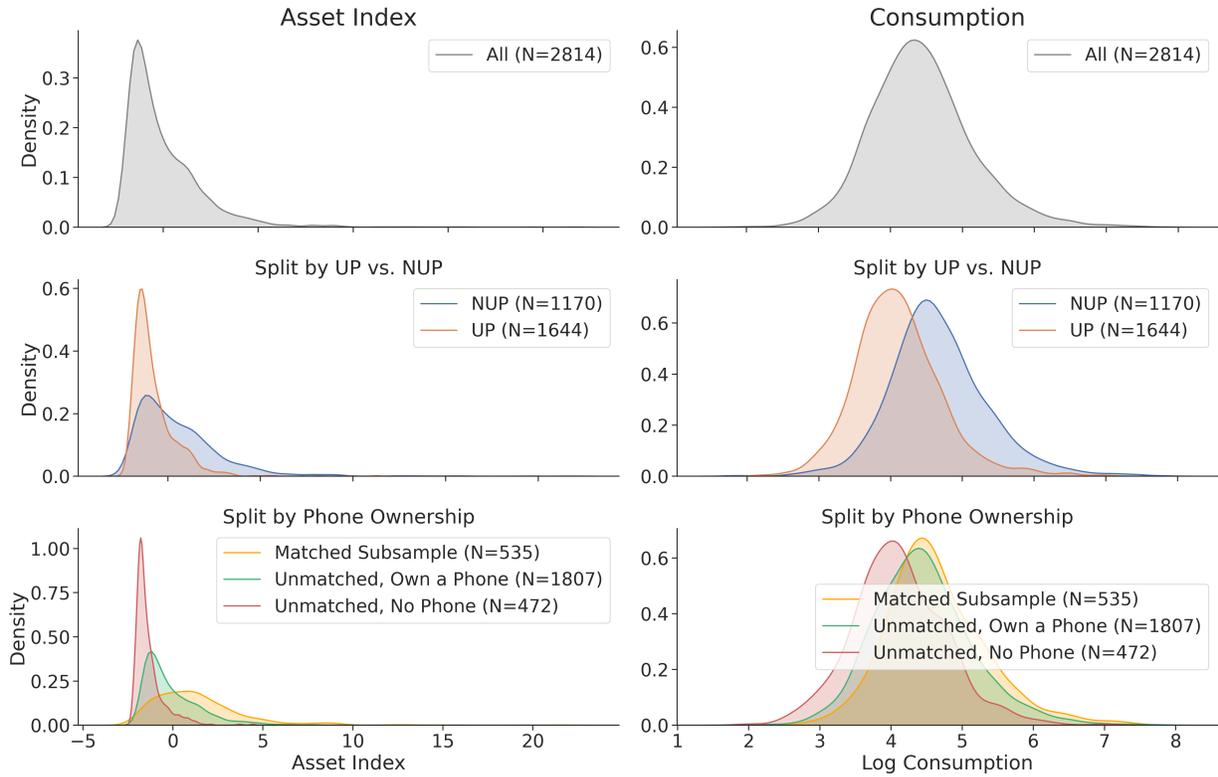


Figure S2: Distributions of asset index and log-transformed consumption, for the entire survey sample, separately for ultra-poor and non-ultra-poor households, and again separately for households in the subsample matched to CDR, households outside of the matched subsample that report owning at least one mobile phone, and households outside of the matched subsample that report not owning a mobile phone.

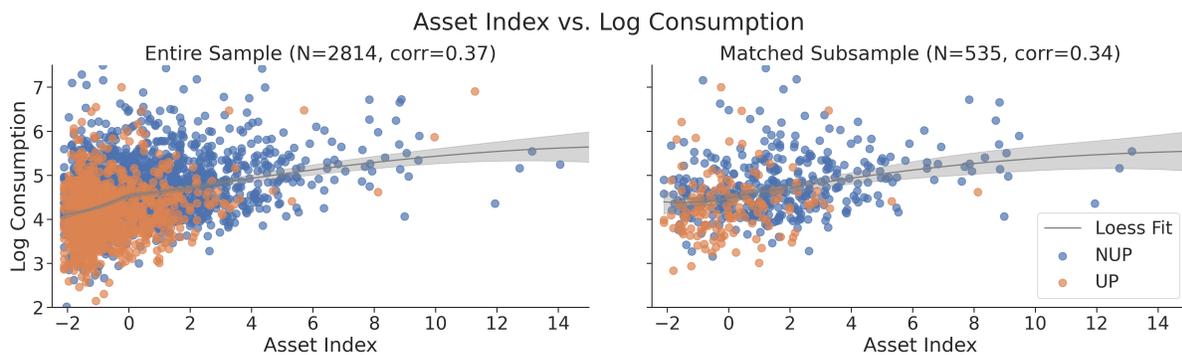


Figure S3: Correlation between asset index and log-transformed consumption, separately for the entire survey sample and the matched subsample. We include the LOESS fit, along with a 95% confidence interval.

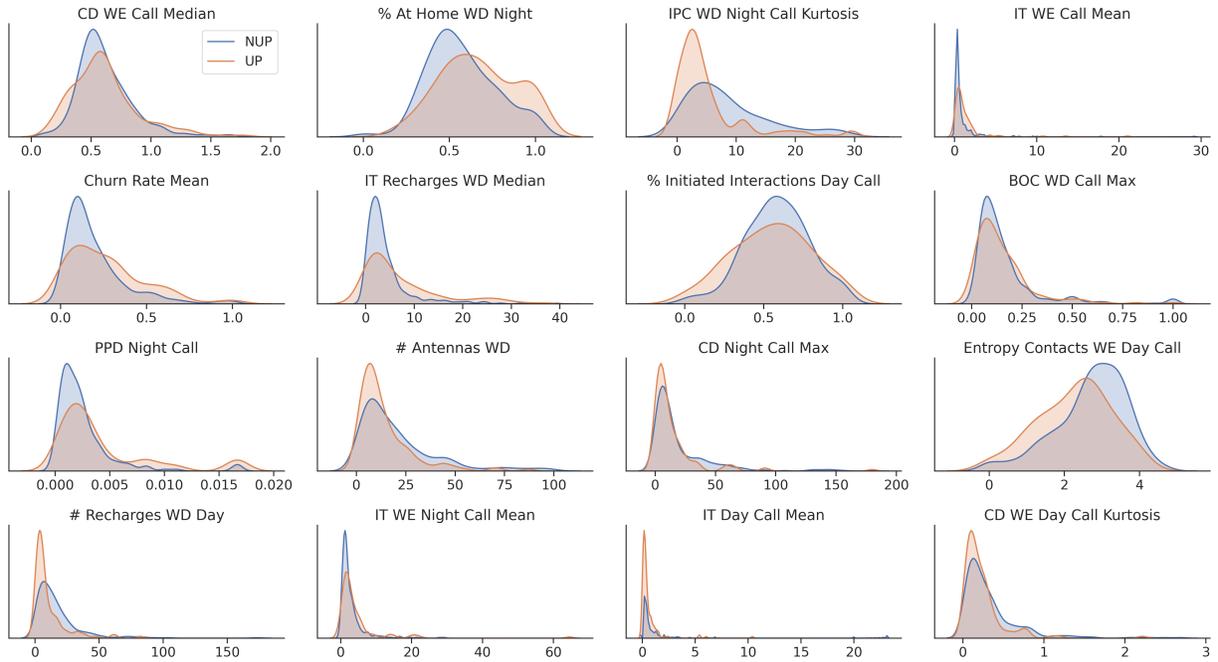


Figure S4: Kernel density estimates for 16 of the most important features for predicting ultra-poor status from CDR, with density estimates shown separately from UP and NUP households. Since many features are near-redundant, rather than showing the raw top 16 features from the table above, we show 16 selected features from the top 50. See Appendix B for abbreviations in feature names.

Table S1: Direction of first principal component of asset ownership

Asset	Magnitude
Radio/CD Player	0.04
TV	0.37
TV Dish	0.29
VCR/DVD Player	0.15
Refridgerator	0.25
Generator	0.11
Matress	0.24
Mobile Phone	0.31
Non-Mobile Phone	0.06
Iron	0.36
Bed Frame	0.29
Jewelry	0.27
Mosquito Net	0.26
Mosquito Repellent Candle	0.08
Fan	0.37
Camera	0.16

Notes: The asset index is calculated over the entire 2,814 household sample, without sample weights. We standardize each of the features to zero mean and unit variance before decomposition. The first principal component accounts for 25.28% of the variation in these standardized features.

Table S2: Feature importances (gradient boosting model)

Feature	Importance	Feature	Importance
CD WE Call Median	8	IT Recharges Night Min	3
% At Home WD Night	7	BOC WD Call Median	3
IPC WD Night Call Kurtosis	7	IT WE Call Min	2
CD Day Call Median	6	BOC WD Night Call Kurtosis	2
IT WE Call Mean	6	IPC Day Call Kurtosis	2
Churn Rate Mean	5	% Nocturnal WD Call	2
IPC Day Call Skew	5	IT WD Day Text Mean	2
IT Recharges WD Median	5	CD Night Call Max	2
% At Home Day	4	IT WE Call Skew	2
% Initiated Interactions Day Call	4	IPC WE Day Call Kurtosis	2
% Initiated Interactions WD Day Call	4	IT WE Text Median	2
BOC WD Call Max	4	% At Home WE Night	2
% Initiated Interactions WD Night Call	3	Entropy Contacts WE Day Call	2
PPD Night Call	3	# Recharges WD Day	2
IT Recharges WD Night Min	3	Entropy Antennas WD	2
IT Recharges Night Median	3	IPC Night Call Skew	2
IPC WD Night Text Mean	3	IT WE Night Call Mean	2
IT Recharges Day Kurtosis	3	# Contacts Day Call	2
IT Night Text Min	3	CD WD Call Max	2
IT WE Day Text Median	3	IT Day Call Mean	2
# Antennas WD	3	IT WD Night Text Min	2
CD WD Night Call Kurtosis	3	Entropy Antennas Day	2
IPC Night Call Kurtosis	3	% Initiated Interactions WE Day Call	2
IPC WE Night Call Kurtosis	3	CD WE Day Call Kurtosis	1
IPC WE Night Call Skew	3	IPC Day Call Std	1

Notes: For our selected machine learning model – the gradient boosting model used to predict ultra-poor status from CDR features – we display feature importances for the top 50 features. Feature importances for the gradient boosting model represent the total number of times the feature is used for a split in the entire ensemble of decision trees. We report feature importances when the model is trained on all 535 observations (rather than over cross validation). See Appendix B for abbreviations in feature names.

Table S3: Details of machine learning models

Model	AUC	Top Five Features
Logistic (No Penalty)	0.53	Reporting # Records, Active Days, Active Days Day, Active Days Night, Active Days WD
Logistic (L1 Penalty)	0.66	Reporting # Records, Active Days, Active Days Day, Active Days Night, Active Days WD
Random Forest	0.68	NOI Out Day Call, NOI Out WD Day Call, Nois Call, Entropy Contacts Night Call, NOI Out WE Call
Gradient Boosting	0.68	CD WE Call Median, % At Home WD Night, IPC WD Night Call Kurtosis, CD Day Call Median, IT WE Call Mean

Notes: Each row indicates performance (AUC) of a different machine learning algorithm, trained to predict ultra-poor status on the sample of 535 matched households. AUC is reported as the mean AUC score over 10-fold cross validation. See Appendix B for details of features.

Table S4: Machine learning an asset index

Model	AUC Score	Top Five Features
Logistic (L1 Penalty)	0.60	TV, TV Dish, Fridge, Mattress, Mobile Phone
Random Forest	0.73	Fridge, Iron, Bedframe, Mattress, TV Dish
Gradient Boosting	0.74	Mattress, Bedframe, Fridge, Mobile Phone, TV Dish

Notes: The asset index benchmark we used is constructed following standard procedures based on principal component analysis (see Table S1). However, it is possible that an alternative asset-based predictor, trained using machine learning to predict ultra-poor status directly from the 16 underlying components, could perform better. We test this hypothesis by adapting our machine learning pipeline for identifying the ultra-poor from CDR to the task of identifying the ultra-poor from asset possession. As with the CDR-based prediction, we evaluate the model over nested cross validation: the model’s predictions are evaluated out-of-sample over 10-fold cross validation, and within each fold hyperparameters are tuned over 5-fold cross validation. We retrain the model on the entire dataset to report hyperparameters and feature importances. Hyperparameters are chosen from the same grid as for the CDR-based models. We display the AUC score and top features for each model.

Table S5: Performance using one, two or three predictor datasets

Data Sources	AUC
Assets	0.73 (0.025)
Consumption	0.71 (0.000)
CDR	0.68 (0.028)
Assets + Consumption	0.76 (0.017)
Assets + CDR	0.76 (0.025)
Consumption + CDR	0.75 (0.016)
Assets + Consumption + CDR	0.78 (0.019)

Notes: AUC scores for targeting methods using a single data source, pair of data sources, and all three data sources together. Standard deviations are calculated from 1,000 bootstrapped samples of the same size as the original sample, drawn with replacement.

Table S6: Matching household to multiple phone numbers

Model	AUC	Top Five Features
Logistic (No Penalty)	0.50	Reporting # Records, Active Days, Active Days Day, Active Days Night, Active Days WD
Logistic (L1 Penalty)	0.65	Reporting # Records, Active Days, Active Days Day, Active Days Night, Active Days WD
Random Forest	0.67	NOI call, NOI Out WE Call, IPC WD Night Call Kurtosis, IPC Night Call Kurtosis, IT Recharges WD Day Min
Gradient Boosting	0.66	Churn Rate Std, CD WE Call Median, IPC WD Night Call Kurtosis, IPC Day Call Skew, % Initiated Interactions Day Call

Notes: In our main analysis, for multi-phone households we use only the phone number belonging to the household head (or to a random household member, where no household head is specified), leaving 535 household-level observations. Here we consider instead using machine learning methods to predict individual-level ultra-poverty, with a dataset of 634 individual phone numbers matched to the ground-truth wealth measures for the associated households. We find that the individual-level models are slightly less accurate than the household-level models presented in the main paper, but we focus on the household-level models in the main paper since the household was the unit of targeting in the TUP program. See Appendix B for abbreviations in feature names.

Table S7: Predicting other measures of poverty from CDR

Model	R^2 or AUC	Top Five Features
<i>Panel A: Predicting below poverty line (binary)</i>		
Logistic (No Penalty)	0.53	Reporting # Records, Active Days, Active Days Day, Active Days Night, Active Days WD
Logistic (L1 Penalty)	0.53	Reporting # Records, Active Days, Active Days Day, Active Days Night, Active Days WD
Random Forest	0.56	NOI Out Night Call, BOC Night Call Kurtosis, CD Day Call Skew, Nois Night Call, IT Night Call Kurtosis
Gradient Boosting	0.55	IT Night Call Kurtosis, IT Text Max, Radius Gyration WE Night, Entropy Antennas, NOI Out WD Call
<i>Panel B: Predicting consumption (continuous)</i>		
Linear Regression	-0.21	% Pareto Recharges WE Night, % Pareto Recharges WE, % Pareto Recharges Night, Entropy Contacts WD Day Text, PPI WE Night Text
LASSO Regression	-0.00	Reporting # Records, PPI Text, PPI Day Text, PPI Night Call, PPI Night Text
Random Forest	-0.02	Churn Rate Mean, IPC WE Night Call Kurtosis, IT Recharges WE Day Skew, IPC WE Night Call Skew, CD WE Call Median
Gradient Boosting	-0.03	CD WD Night Call Skew, IPC WD Day Text Skew, IT WD Night Call Min, IT WD Night Call Max, IT WE Night Call Max
<i>Panel C: Predicting asset index (continuous)</i>		
Linear Regression	-0.06	IPC Text Min, IPC WD Text Min, IPC WD Day Text Min, BOC WD Text Min, % Initiated Conversations WD
LASSO Regression	0.00	Active Days WE Day, Active Days WD, Active Days WE, Active Days, Active Days WD Day
Random Forest	0.00	IT Night Call Skew, IPC Text Min, IT WE Day Call Median, IT WE Call Median, Entropy Contacts WE Night Call
Gradient Boosting	-0.02	IT Text Median, Entropy Antennas WE, Entropy Antennas WD Night, Entropy Contacts WE Night Call, IT Recharges Night Min
<i>Panel D: Predicting CWR group (continuous)</i>		
Linear Regression	0.01	PPI Night Text, IT Recharges Day Skew, IPC WE Call Min, Active Days WE Night, IT Recharges WD Day Skew
LASSO Regression	0.05	PPI Night Text, Active Days WE Day, Active Days WE Night, IT Recharges WD Day Skew, IT Recharges Day Skew
Random Forest	0.04	# Contacts WE Day Call, Entropy Contacts WD Night Call, IPC Night Call Kurtosis, # Contacts WE Call, IT Call Kurtosis
Gradient Boosting	0.03	IT Call Kurtosis, IT Recharges Day Skew, # Contacts WE Day Call, IT Recharges Day Kurtosis, IPC WD Night Call Kurtosis

Notes: Machine learning results for predicting: (A) Below-poverty-line status, using consumption data and based on Afghanistan’s national poverty line; (B) Total consumption (log-scale); (C) Asset index; and (D) Community Wealth Ranking. Performance is evaluated on the sample of 535 matched households. Binary metrics (A) are evaluated using the mean AUC score over 10-fold cross validation; Continuous metrics (B-D) are evaluated using the mean R^2 score over 10-fold cross validation. See Appendix B for details of features.

Table S8: Variable costs of different targeting methods

Targeting Method	Cost per HH screened	Total cost of targeting	Fraction of program costs spent on targeting
CBT	\$2.20	\$276,586	2.18%
PMT	\$4.00	\$502,884	3.97%
Consumption	>\$4.00	>\$502,884	>3.97%
Phone	\$0.00	\$0	0.00%

Notes: Costs for the TUP program, based on costs estimated from the literature. The TUP program screened an estimated 125,721 households; benefits valued at \$1,668 were provided to each of the 7,500 beneficiary households for a total benefits distribution of approximately \$12.7 million. The total value of benefits is used to obtain the targeting costs as a percentage of total program costs. For the Phone option, we assume no contact with beneficiaries is required; if contact were required, for instance to collect informed consent, variable costs would increase accordingly.

Table S9: Costs for CBT and PMT targeting methods obtained from the literature

Source	Location	Cost per household
<i>Panel A: CBT</i>		
Alatas et al. (2012)	Indonesia	\$1.20
Karlan and Thuysbaert (2019)	Honduras	\$1.67
Karlan and Thuysbaert (2019)	Peru	\$1.90
Schnitzer and Stoeffler (2021)	Burkina Faso	\$5.60
Schnitzer and Stoeffler (2021)	Niger	\$5.40
Schnitzer and Stoeffler (2021)	Senegal	\$3.20
Median		\$2.20
<i>Panel B: PMT</i>		
Alatas et al. (2012)	Indonesia	\$2.70
Karlan and Thuysbaert (2019)	Honduras	\$2.62
Karlan and Thuysbaert (2019)	Peru	\$3.05
Schnitzer and Stoeffler (2021)	Burkina Faso	\$5.69
Schnitzer and Stoeffler (2021)	Chad	\$9.50
Schnitzer and Stoeffler (2021)	Mali	\$4.00
Schnitzer and Stoeffler (2021)	Niger	\$6.80
Median		\$4.00

Notes: Costs per household screened for two targeting methods obtained from three papers in the targeting literature. Costs in [Alatas et al. \(2012\)](#) are provided per-village; we use the average of 54 households per village to obtain per-household targeting costs. Cost for the CBT in [Karlan & Thuysbaert \(2019\)](#) is provided as part of the cost for a hybrid CBT and verification approach; although an individual cost for the cBT alone is provided, it is possible this cost excludes some of the mutual costs for the two exercises and is therefore an underestimate of costs of a CBT alone. We use the median of the distribution of targeting costs in our cost analysis.