Poverty measurement and the targeting of social protection programs

Onur Altındağ, Zeynep Balcıoğlu, Stephen D. O'Connell, Aytuğ Şaşmaz

This Version: July 10, 2022

Abstract

In targeted antipoverty programs, the definition of deprivation determines who is in need, who receives program benefits, and, through them, program effects. Deprivation can be experienced in a wide range of dimensions, however, leading to tradeoffs in the determination of who is poor. We pre-specify the analysis of a nationwide field experiment in which we estimate the effects of targeting unconditional cash transfers to alternative measures of deprivation. To do this, we partnered with leading humanitarian agencies in Lebanon to experimentally vary the poverty measure used to target an at-scale year-long structured cash assistance program for refugees. Targeting among similar populations with the same amount of resources, we will estimate the program effect resulting from each of the alternative poverty targets across a range of economic well-being indicators. We further pre-specify the protocol for qualitative data collection and quantitative data analysis aimed at understanding the mechanisms that lead to differences in targeting effects.

Keywords: poverty targeting, poverty measurement, social protection, antipoverty programs, unconditional cash transfers, refugees, forced displacement, Lebanon.

JEL Classification: I38, I32, O12, D74

Contact Information. Altındağ: Bentley University Department of Economics, IZA Institute of Labor Economics, and Economic Research Forum, oaltindag@bentley.edu. Balcıoğlu: Humboldt University of Berlin and Leiden University, zeynep.balcioglu.tasma@hu-berlin.de. O'Connell: Emory University Department of Economics and IZA Institute of Labor Economics, soconnell@emory.edu. Şaşmaz: Bryn Mawr College, asasmaz@brynmawr.edu.

1 Introduction

The targeting methodology of social assistance programs is a defining component of their structure and operation. A central tension in program design is in the choice of who is poor because it determines beneficiaries, and through them, program effects. This choice is particularly salient for at-scale programs serving heterogeneous populations that experience different price levels, earning capacities, consumption preferences, social support networks, public goods, and access to other markets (Deaton, 2006). A growing literature quantifies the implications of targeting approaches on antipoverty program effectiveness (Ravallion, 2009; Alatas *et al.*, 2012, 2016; Brown *et al.*, 2018; Banerjee *et al.*, 2019; Basurto *et al.*, 2020; Premand and Schnitzer, 2020; Haushofer *et al.*, 2022).

The definition and measurement of poverty is a long-debated topic, which has resulted in numerous ways to characterize deprivation, including monetary poverty, food insecurity, anthropometrics, capabilities, or coping strategies, among others (Sen, 1981, 1999; Young, 2012; Meyer and Sullivan, 2012). Despite the varied expressions of deprivation, most anti-poverty programs default to targeting income or consumption either directly or via proxy measures. In this study, we investigate whether the type of economic vulnerability that an at-scale unconditional cash assistance program targets generates different levels of economic benefit among the targeted population.

The program we study considers approximately 1.5 million Syrian refugees in Lebanon as potentially eligible for unconditional cash transfers. Targeting transfers to the poorest of this population is challenging due to the nature of poverty and vulnerability varying substantially throughout Lebanon. For example, urban poverty is typically characterized by high prices for basic needs, high expenditure levels, and low food security. In contrast, rural households exhibit low expenditure levels but relatively higher food security, largely due to agricultural work and more extensive and interdependent communities and networks.

In Lebanon, the UN and its partners distribute more than $300MM annually in a year-long structured transfer program to around one million people. Starting in 2021, we partnered with UN agencies to experimentally vary the targeting of their program to alternative measures of deprivation. The poverty measures that form the basis for the targeting in our four treatment arms are: (i) monetary poverty, as measured by per capita expenditure, (ii) severe or moderate food insecurity, as measured by the reduced Coping Strategies Index (rCSI), (iii) having poor or borderline food consumption, as measured by the Food Consumption Score (FCS), and (iv) being moderately or severely deprived in a Multidimensional Deprivation Index (MDI). International organizations, governments, and humanitarian agencies frequently use these measures to assess vulnerability and structure social assistance programs.

For the 2021-22 assistance cycle, the full population of Syrian refugee households were assigned to one of the targeting treatment arms by lottery. Each treatment arm received 25 percent of total program resources to distribute to a fixed number of households deemed eligible in that arm. There is substantial variation in assistance amount and eligibility depending on the poverty measure by which a household is targeted: more than one third of households would be allocated a different assistance amount under at least one of the other three treatment arms to which they were not assigned.

From June to July 2022 – approximately eight months into the yearlong assistance cycle – the implementing agencies conduct their annual nationally representative survey. We expect to receive these data by August 2022. Our analysis will then recover two related quantities: (i) an aggregate program design effect, which compares households across treatment arms that receive the same aggregate amount of resources but are targeted to different measures of deprivation, and (ii) the program effects for the households whose assistance package would change had they been targeted according to a different poverty measure. The first quantity will indicate whether the poverty measure used to target a scaled social assistance program has a meaningful effect on average household well-being. The second quantity will allow us to directly estimate the effect of an additional dollar of transfer in each targeting arm on a large set of outcomes for these marginal households.

Our work builds on empirical studies that quantify well-being differences across alternative targeting methods for antipoverty programs (Ravallion, 2009; Alatas *et al.*, 2012, 2016; Karlan and Thuysbaert, 2019; Basurto *et al.*, 2020; Premand and Schnitzer, 2020; Haushofer *et al.*, 2022). The focus of this literature has been understanding how alternative methods to target the poor affect targeting efficiency and program effectiveness. Independent of the methodology, however, all anti-poverty programs make the implicit or explicit choice of who to serve by defining who the poor are. In our study, we estimate the effects of targeting alternative types of deprivation holding other aspects of the targeting method constant. Our primary contribution is therefore in quantifying tradeoffs in this first-order design choice.

Our study also addresses a natural limitation of the existing literature. Differences in program effects that arise from allocating the same resources to different populations can only be explained by the complier population outcomes – those whose support would change had they been under a different targeting regime. Our research design allows us to observe the counterfactual allocations across all treatment arms. Thefore, we can observe compliers directly and estimate average treatment effects for each treatment arm. To further explain findings, we complement existing household surveys with additional quantitative and qualitative data collection directly from complier households to understand mechanisms by which program effects (may) differ by poverty target.

We organize the rest of our pre-analysis plan as follows. Section 2 provides a summary of the country context and the program details. We describe the various data sources used in the analysis and their linkage in Section 3. Section 4 outlines the hypotheses that we aim to test in the study and corrections for multiple hypothesis testing. In Section 5 we present the empirical framework, including pre-intervention balance tests and power calculations. Section 6 discusses our qualitative data collection, and Section 7 discusses other concerns and considerations.

2 Country setting and program design

As of 2022, more than 1.5 million forcibly displaced Syrians reside in Lebanon. They live in non-camp settings and are spread throughout the country, with no statutory restrictions on mobility. The United Nations World Food Programme (WFP) and the United Nations High Commissioner for Refugees (UNHCR) support the refugee population in Lebanon through education, protection, shelter, public health, and medical insurance programs, among others. In collaboration with international and local NGOs, the UN agencies' primary form of assistance is through unconditional cash-based transfers. These programs annually disburse over $300 million USD, reaching between 40 and 70 percent of the refugee population in recent years.

The assistance cycle operates on an annual basis and beneficiary assignment uses proxy-means test (PMT) targeting of household expenditure per capita. Since 2016, the PMT has been based on an econometric model that uses survey and administrative data held by the UNHCR.[1] The program structure has three tiers. The poorest eligible households – approximately 35 percent of the population – receive 800,000 Lebanese Pounds (LBP) plus 300,000 LBP per family member (up to six) per month. Depending on a set of programmatic background factors, the middle tier reaches approximately 45 percent of households and provides either 800,000 LBP cash, or 300,000 LBP per person (up to six) in food voucher credit, per month. Those in the least poor quintile receive no assistance. These transfer values are substantial. Using an exchange rate of 15,000 LBP per dollar from mid-2021, a household of five eligible for the highest transfer value would receive approximately 153 USD per month. According to our survey data, the median expenditure

[1] Since the 2018-19 program cycle, the econometric targeting model was based on a cross-validated shrinkage estimator, detailed in Altındağ *et al.* (2021).

for a refugee household of five in June 2021 was 90 USD.

For the 2021-22 assistance cycle, we collaborated with the UN agencies to test the targeting of this unconditional cash transfer program to alternative measures of poverty. At the outset of the assistance cycle, all refugee households were randomly assigned to one of four arms. The first arm targets monetary poverty, as measured by expenditure per capita and a poverty threshold of the survival minimum expenditure basket (SMEB), the official expenditure-based monetary poverty line determined by the the World Food Programme in Lebanon.[2] The second arm uses the reduced Coping Strategies Index (rCSI), which measures the degree of food insecurity of a household via eight food coping strategies that the household engaged in during the week before the interview, where the poverty threshold is a score of 18 or greater (out of 56) indicating high food insecurity. The third arm is based on the food consumption score (FCS), which is a proxy measure of a household's caloric intake based on the frequency of consumption across eight differentially weighted food categories over the previous week, where a score of 42 or lower (out of 112) indicates inadequate food consumption. The last arm is based on a multidimensional deprivation index (MDI) that aims to reflect deprivation in various essential needs experienced by the household in food, health, education, shelter, water supply, sanitation, hygiene (WASH), and safety. Binary deprivation indicators are aggregated across subcategories resulting in an index that ranges from zero (not deprived) to one (deprived in all dimensions); a household with a score of .33 or greater being considered significantly deprived.[3]

Because our study design was embedded within an ongoing at-scale program, the implementing agencies kept intact the general structure of their targeting approach. This involved a mix of proxy-means testing, location-specific caseload quotas, and other logistical constraints – all of which were orthogonal to the household-level randomization into treatment arms described above.[4] Further details of implementation are available in Appendix A. In addition to baseline balance, we establish two features of this approach that are essential for the research design. First, we show that each targeting arm is able to allocate more assistance to households who are poor according to the type of poverty targeted in

[2]This poverty line reflects the consumption level required for a family of two adults and three children, one aged over five and other two aged under five, to satisfy the basic needs such as food, shelter, heating, water, and clothing.

[3]See https://documents.wfp.org/stellent/groups/public/documents/manual_guide_proced/wfp211058.pdf, https://documents.wfp.org/stellent/groups/public/documents/manual_guide_proced/wfp197216.pdf, and https://docs.wfp.org/api/documents/WFP-0000074197/download/ for official definitions and guidance on the construction of the rCSI, FCS, MDI, respectively.

[4]The inputs to eligibility determination are only available to a few UNHCR and WFP staff in Beirut headquarters and, as described and empirically validated in Altındağ and O'Connell (2022), are not manipulable by field staff or households.

that arm. Second, we can replicate the assistance allocation mechanism used by the implementing agencies to recover counterfactual allocations had a household been randomized to any other treatment arm. This latter feature allows us to directly observe complier populations and estimate the complier average treatment effect for each arm, which we describe in further detail in Section 5.

2.1 Timeline

The coauthors of this analysis plan have been involved in the targeting of cash transfer programs in Lebanon since 2018; the specific study undertaken in the 2021-22 cycle was the result of field work taken over several years, including consultations with implementing agencies, discussions with NGO and donor partners, and feedback from refugee focus group discussions. In early 2021, we suggested the current study, and subsequently worked with implementing agencies to determine logistical needs and constraints to jointly undertake the research while not affecting their ability to administer their programs. Our study thus used existing program administration processes and data collection and was incorporated into existing institutional structure and operations.

The program and study timeline of the 2021-22 program cycle was as follows:

- May - June 2021: Collection of household sample survey (VASyR 2021, $>$ 5,000 households, used for PMT modeling and baseline tests).
- August 2021: Estimation of PMT, randomization of households to treatment arms.
- October 2021: Selection of beneficiaries per arm following targeting scores, and notification of beneficiaries.
- November 2021: First disbursement under new (study) assistance cycle.
- December 2021: pre-PAP focus group discussions held.
- June - July 2022: Collection of household survey (VASyR 2022; 5,000 households clustered random sampling + 2,000 additional households targeted for complier sampling).
- July 2022: PAP filed
- August 2022 (expected): Qualitative focus group discussions with complier households.
- August 2022 (expected): Receipt of data and empirical analysis

3 Data

The analysis is based on administrative and survey data held or collected by UNHCR, WFP, and their partners.[5] A unique masked household identifier is provided with each data source in order to link records at the household level. For the majority of data sources, we will have two rounds (or contemporaneous snapshots) from mid-2021 and mid-2022 for use in baseline and post-treatment analyses, respectively. The data sources are listed and described below.

1. **UNHCR administrative data** includes demographics, targeting scores, and assistance records.

    - Sample: entire population of registered/enrolled refugee cases in Lebanon.
    - Years/periods available: May 2021; June 2022.
    - Used in our analysis for: targeting model (as proxy measures for PMT), baseline tests of balance for full population, parametric control for targeting score in empirical specification.

    Description: The UNHCR keeps a record of all Syrian refugees in Lebanon who have registered or enrolled with UN agencies. These data include every individual's arrival date, their home region in Syria, a self-reported education level, age (in years), relationship to the household head, gender, age, targeting scores, assistance history, and a series of other indicators reflecting specific vulnerabilities or protection concerns. These data are updated on a regular basis through mobile phone and in-person communication with refugee families.

2. **Survey data: the Vulnerability Assessment of Syrian Refugees in Lebanon (VASyR)** provides the poverty measures used to target assistance across treatment arms, and the outcome measures used to assess program impacts.

    - Sample: clustered random sample of approximately 5,000 households annually, drawn from the administrative database.
    - Years/periods available: May-June 2021; June-July 2022.
    - Used in our analysis for: targeting model (as poverty outcomes to target), baseline tests of balance across additional indicators, outcomes for endline analysis.

[5] The UNHCR is the primary owner and custodian of these datasets; a Memorandum of Understanding (dated 19 December 2019, Amended 21 December 2021) between UNHCR, Emory University, and Bentley University specifies the terms of our access to these data, and is available upon request.

Description: The majority of our baseline and endline analysis will be based on rounds of the VASyR survey, which provides measures of the outcomes of interest. Since 2016, the VASyR survey has collected detailed information on refugee families' well-being and expenditures, similar to household-level labor force and/or living standards surveys administered in various developing country settings, and with additional modules specific to the forced displacement context.[6] The sample size varies annually, but typically collects data from 4,000 to 5,000 households across Lebanon. The targeting PMT and our baseline balance tests use the 2021 round of the VASyR, which surveyed over 5,000 households.

Our planned endline analysis will use the 2022 VASyR survey. This survey round is being collection in June and July 2022. We have coordinated with partner agencies to add an additional sample of 2,000 complier households due to power needs (see section 5.3). We also coordinated to add new questions related to our primary and mechanism outcomes for collection in this round (see Table 1). Because the randomization to treatment arms was performed across cases for the entire population, we are able to rely on this cross-sectional representative sample to conduct the analysis.

4 Hypotheses and Outcomes

We have two hypotheses, which we state in terms of the null. The first is that there are no endline differences in measures of well-being across treatment arms. The second is that complier average treatment effects are zero across treatment arms. We test these hypotheses across a range of outcomes – including those used to target the arms themselves, as well as others, detailed in Table 1. Primary outcome domains include the targeted poverty measures, aspects of children's well-being, and living conditions and livelihoods. Mechanisms relate to fundamental features of the household or environment that may alter beneficiaries ability or way to use additional income, and include measures of property rights over transfers, social support networks, asset ownership, and financial access. To account for multiple hypothesis testing, inference for the hypotheses will be based on Anderson (2008) sharpened q-values adjusted within domain.

[6] See https://www.dropbox.com/s/t8w4169z0m27kop/VASyR%202020%20%28Eng%20-%20Print%20version%29.pdf?dl=0 and https://www.unicef.org/lebanon/media/7841/file/VASyR%202021%20Report%20EN.pdf for VASyR 2021 survey instruments and report, respectively.

Table 1: Outcomes measures corresponding to specified hypotheses, by domain

Domain	**Questionnaire Reference**
Poverty measures	
ln(expenditure per capita, excluding debt repayment)	P32, Sec. 6.3.1
Reduced Coping Strategies Index (z-score)	P36, Sec. VIII
Food Consumption Score (z-score)	P35, Sec. VII
Multidimensional Deprivation Index (z-score)	(various throughout)
Child well-being	
Any child 7-15 y.o. not in school	P11, Sec. 4.7
Any child 7-15 y.o. working for wage or otherwise	P11, Sec. 4.7.3
Any girl 13-17 y.o. married	P4, Sec. 4.3.9
Any child under 5 y.o. sick	P16, Sec. 4.10.1
Living conditions (Livelihoods, Housing, and Sanitation)	
Livelihood Coping Index (z-score)	P36-38, Sec. VIII
WASH Index (z-score)	P23, Sec. 5.6
Shelter conditions (z-index)	P28-29, Sec. 5.9
Channels: Property rights	
Rental debt stock	Question added to VASyR 2022
Card ever used as collateral	Question added to VASyR 2022
Card currently with debtor	Question added to VASyR 2022
Channels: Social support and networks	
Has any close friends	Question added to VASyR 2022
Neighbors could care for children if needed	Question added to VASyR 2022
Can get or borrow money from social circle	Question added to VASyR 2022
Have been asked by others to assist financially	Question added to VASyR 2022
Lives in a supportive community	Question added to VASyR 2022
Community support for household emergencies	Question added to VASyR 2022
Channels: Productive assets	
Consumer durable assets index (z-score)	Question added to VASyR 2022
Productive assets index (z-score)	Question added to VASyR 2022
Channels: Savings and Financial Inclusion	
Amount of savings	Question added to VASyR 2022
Had to spend savings to cope	P36, Sec. VIII.7

Notes: Table describes outcome measures for hypotheses to be tested, by domain. Definition reference column refers to official guidance documents (for indices) or specific questions in the 2021 VASyR survey, available at https://www.dropbox.com/s/t8w4169z0m27kop/VASyR%202021%20%28Eng%20-%20Print%20version%29.pdf?dl=0. Any *force majeure* departures from the above definitions will be documented in the manuscript appendix.

5 Empirical strategy and tests

5.1 Framework and econometric approach

Our empirical analysis is based on comparing outcomes across four treatment arms and estimating the program effect within each treatment arm for the complier populations.

Let Z_i be a binary variable that indicates the randomly assigned treatment arm for household i, which can take values $Z \in \{1,2,3,4\}$, reflecting programs targeted to poverty measure by expenditure per capita (EPC), reduced coping strategies (rCSI), food consumption scores (FCS), or a multidimensional deprivation index (MDI), respectively.

We begin with potential outcomes $Y_i(Z_i)$ for any household i depending on the program treatment arm to which they are assigned. Additionally, $W_i(Z_i)$ is the counterfactual assistance amount when household i is assigned to program arm Z_i.

$$\mathrm{Y}_i = \begin{cases} Y_i(1) & \text{if } Z_i = 1 \\ Y_i(2) & \text{if } Z_i = 2 \\ Y_i(3) & \text{if } Z_i = 3 \\ Y_i(4) & \text{if } Z_i = 4 \end{cases} \qquad \mathrm{W}_i = \begin{cases} W_i(1) & \text{if } Z_i = 1 \\ W_i(2) & \text{if } Z_i = 2 \\ W_i(3) & \text{if } Z_i = 3 \\ W_i(4) & \text{if } Z_i = 4 \end{cases}$$

The net differential program targeting effect for any one program $j \in \{2,3,4\}$ relative to the expenditure targeting reference arm ($Z_i = 1$) is given by:

$$\tau_j = E[Y_i(j)] - E[Y_i(1)]$$

We define Z_{ij} as an indicator for household i being assigned to treatment arm j, and zero otherwise. The corresponding regression that captures outcome differences across randomly assigned treatment arms relative to the reference arm can be expressed as:

$$y_i = \alpha + \sum_{j=2}^{4} \tau_j \times Z_{ij} + \varepsilon_i \tag{1}$$

where τ_j capture the differences in outcomes for the rCSI, FCS, and MDI treatment arms relative to EPC and α is a common intercept. Because Z_{ij} is randomly assigned for the full population and we use a nationally representative sample, τ_j yields the average differences in y_i across programs that target alternative measures of deprivation.[7,8] This allows us to

[7]In our empirical analysis, we plan to control for the natural log of predicted expenditure per capita of the household from the national PMT to increase precision.

[8]Based on verification from program records and survey data, assistance disbursement and use follows the eligibility criteria precisely: nearly all households deemed eligible for assistance receive and spend their allocated transfers, implying almost perfect assistance take-up.

formalize a joint hypothesis test, structured as:

$$H_0 : \beta_2 = \beta_3 = \beta_4 = 0$$
$$H_1 : \beta_j \neq 0 \text{ for at least one } j \in \{2,3,4\}$$

Moreover, within each treatment arm, we can further decompose the population by households' counterfactual treatment status. Our ability to directly observe these households – whom we refer to as compliers – derives from the fact that each household in the population has four potential eligibility statuses observed by us. One of these is realized for any household i due to assignment to a treatment arm Z_i, and there are three unrealized (but known) counterfactual assistance amounts $W_i(j)$, where $j \neq Z_i$.

The counterfactual potential outcomes for any given household i is given as a function of assignment Z_i, which determines the assistance amount:

$$Y_i(Z_i) = Y_i(W_i(Z_i)) \tag{2}$$

where the variation across the potential outcomes in our setting is exclusively induced by the random assignment Z_i, which determines the assistance amount W_i.

In this framework, compliers are households that would receive more assistance under one treatment arm relative to another, and were assigned to either of the treatment arms for which they have these discordant counterfactual assistance amounts. Always-takers are those whose assistance amount would not change across two arms, and never-takers are those who would be unassisted across paired arms. For a household i who is assigned to either of the treatment arms j or k, compliers, always-taker, and never-taker types are indicated by C_i, A_i, and N_i, respectively, where

$$\mathrm{C}_i = \begin{cases} 1 & \text{if } W_i(j) > W_i(k) \\ 0 & \text{otherwise} \end{cases}$$

$$\mathrm{A}_i = \begin{cases} 1 & \text{if } W_i(j) = W_i(k) \text{ and } W_i(j) > 0 \\ 0 & \text{otherwise} \end{cases}$$

$$\mathrm{N}_i = \begin{cases} 1 & \text{if } W_i(j) = 0 \text{ and } W_i(k) = 0 \\ 0 & \text{otherwise} \end{cases}$$

The complier average treatment effect for program j can be expressed in potential outcomes as:

$$\tau_j^{CATE} = E[Y_i(j) - Y_i(k) | C_i = 1].$$

Because Z_i is randomly assigned and we directly observe C_i, we can recover the complier average treatment effect empirically via:

$$\tau_j^{CATE} = E[Y_i | C_i = 1, Z_i = j] - E[Y_i | C_i = 1, Z_i = k]$$

For the complier households, assistance amount W_i changes with their random group assignment Z_i and unconfoundedness is satisfied. Given that the households and the field staff are both blind to the randomized treatment arm, we expect no anticipatory or experimenter effects, and the exclusion restriction is therefore also satisfied.

Once we pool the samples of program j compliers across all k counterfactual arms, the following specification captures the complier average treatment effect for program j:

$$y_i = \alpha + \beta_j \times W_i + \varepsilon_i \tag{3}$$

Equation 3 will be estimated using separate samples of program compliers for $j \in 1,2,3,4$. The coefficient β_j captures the complier average treatment effect of an additional dollar of assistance on economic well-being for program j.

Table 2 shows the count of complier households in the population by treatment arm and complier type, which form the frame for our additional complier data collection. For example, the first panel tabulates the population of compliers for the expenditure program. These are households who were assigned either to treatment arm 1 (such that $Z_i = 1$) or another arm k in which $W_i(1) > W_i(k)$. The column label "T" indicates the "treated" group: the set of households who were assigned to the arm in which their assistance amount was greater than the counterfactual arm. Accordingly, the column label "C" indicates "control" groups: those who were assigned to the arm in which they were assisted with less. There are more than 115,000 unique complier households, which comprise more than one third of the refugee population. To achieve appropriate statistical power in our analyses, these households are surveyed specifically in the additional complier data collection mentioned in Section 3. Section 5.3 discusses our power calculations and direct sampling of these households during the annual survey period.

Table 2: Complier Populations by Treatment Arm

Treatment arm:	EPC $Z_i = 1$	rCSI $Z_i = 2$	FCS $Z_i = 3$	MDI $Z_i = 4$
EPC compliers	T	C	C	C
$W_i(1) > W_i(2)$	10,787	9,859	-	-
$W_i(1) > W_i(3)$	3,967	-	3,836	-
$W_i(1) > W_i(4)$	10,943	-	-	10,034
rCSI compliers	C	T	C	C
$W_i(2) > W_i(1)$	9,301	9,075	-	-
$W_i(2) > W_i(3)$	-	7,011	7,046	-
$W_i(2) > W_i(4)$	-	8,211	-	8,153
FCS compliers	C	C	T	C
$W_i(3) > W_i(1)$	3,778	-	3,825	-
$W_i(3) > W_i(2)$	-	7,852	8,619	-
$W_i(3) > W_i(4)$	-	-	10,437	9,756
MDI compliers	C	C	C	T
$W_i(4) > W_i(1)$	8,674	-	-	8,687
$W_i(4) > W_i(2)$	-	7,701	-	8,076
$W_i(4) > W_i(3)$	-	-	8,495	8,463

Notes: Table presents count of compliers by treatment arm and complier type. Panels indicate the sample available to estimate the complier ATE for the expenditure, rCSI, FCS, and MDI targeting treatment arms, respectively. Within panels, "T" indicates the beneficiaries (treated) for whom $W_i(j) > W_i(k)$ and "C" indicates non-beneficiaries (controls) for whom $W_i(j) < W_i(k)$.

5.2 Balance tests at baseline

We use multiple sources of data to confirm that randomization achieved baseline balance in observed characteristics among the full population and the 2021 survey sample. The baseline survey sample in 2021 also allows us to confirm that each arm was successful in assisting households who are classified as poor according to targeted poverty measure. Moreover, we use the 2022 sampling frame for the endline data to characterize the first stage among compilers. That is, we show that the randomization induces meaningful variation in the amount of assistance receipt by these households without inducing meaningful differences in other observed characteristics.

Table 3 contains the balance tests for the full population across available demographics (Panel

A) and protection/background indicators (Panel B), as well as measures of well-being (poverty targets) available in the 2021 survey sample (Panel C). Across all measures, we find no meaningful imbalances, as expected.

In Panel D of Table 3, we confirm that each arm is more effective than others at allocating assistance to those who were poor according to the poverty target in that arm. To do this, we use the 2021 survey (baseline) data to first construct an indicator for households who are eligible for full assistance (F_i), which is the highest value assistance package targeted by the programs. Next, we generate another indicator (P_i) for being poor according to the targeted measure of poverty within each treatment arm (Z_i). If the targeting design is successful, then the households who are poor according to the targeted type of poverty should be substantially more likely to be eligible for full assistance compared to the households who experience the same type of poverty but were randomly assigned to other treatment arms. For example, the full assistance eligibility rate among the households who are expenditure poor should be much higher in the expenditure targeting treatment arm compared to the share of expenditure poor who are eligible in other treatment arms.

We test for differential targeting in a regression using the following specification:

$$F_i = \gamma + \sum_j \pi_j Z_{ij} + \theta P_i + \lambda(T_i \times P_i) + \upsilon_i \tag{4}$$

where T_i is an indicator that equals one for households who are assigned to the treatment arm that targets the type of poverty indicated with P_i, and zero otherwise. In this setting, λ captures the change in likelihood of receiving full assistance for the poor if they are assigned to the treatment arm that targets them, relative to households that are assigned to a treatment arm that targets a different type of poverty.

Panel D shows how a family randomly assigned to a targeting arm under which they are identified as poor has a higher likelihood of receiving full assistance. For example, the first column of Panel D assesses expenditure targeting. For families who are expenditure poor and were assigned to a control arm (rCSI, FCS, or MDI targeting arms), the baseline likelihood of receiving full assistance is around 54 percent. If a family is expenditure poor and was subject to expenditure targeting, the likelihood of receiving full assistance additionally increases by 15 percentage points. The same pattern holds for rCSI, FCS, and MDI targeting arms where random assignment to a treatment arm that aligns with the household's poverty status differentially increases full assistance eligibility by 20, 11, and 23 percentage points, respectively.

We next present further tests related to compliers. As of the writing of this analysis plan, the 2022 survey (endline) sample data collection has not been completed. However, we have access to the list of households in the 2022 sampling frame. We use these households to show the effects of compliance on assistance receipt, as well as confirm pre-assignment covariate balance from the administrative data among compliers in the endline sample frame. Table 4 contains the effect of compliance on assistance receipt in Panel A, showing increases in monthly assistance received as a result of being assigned to the treatment arm with a greater counterfactual assistance amount. The magnitudes closely match expectations, as the increases in assistance across packages varies between 800,000 to 1,000,000 LBP. Panels B and C of Table 4 contain baseline balance tests of demographics and background measures from the administrative data, again showing no meaningful imbalances. These tests show that randomization achieved balance among the complier population and also induced the expected variation in amount of assistance received.

5.3 Power and sample size calculation

Using the 2021 survey data, we simulate aggregate effects of targeting arms (Equation 1) to quantify statistical power in an equivalently-sized endline survey. In this exercise, we simulate one treatment arm to have effect sizes ranging from .025 to .3 standard deviations for each of our four primary outcomes, assuming zero effect in all other arms. In each of 10,000 draws at each effect size, we conduct the F-test implied by H_0 for Equation 1, as well as a single parameter t-test for the arm with the simulated effect. The power series for each of the four primary outcomes is plotted in Figure 1 and shows that using the sample that we expect in the planned 2022 annual survey would allow us to detect single-program ITT effects of 0.1 to 0.125 SD with 80 percent power.

For the complier analysis (Equation 3), the sample from the planned annual survey is unlikely to provide sufficient power to detect small but meaningful effect sizes: less than 500 compliers per arm would be found in a random sample of 4,500 households, necessitating additional sample collection in order to power the estimation of program CATE effects.

For this study, implementing partners provided the capacity to collect up to 2,000 complier households in addition to the annual survey. We conducted a power analysis by simulation and determined that the additional sample would allow us to power equation 3 for an effect size of 0.175 SD (0.11 in natural log units of expenditure per capita) for the four primary outcomes. For comparison, Altındağ and O'Connell (2022) estimated regression discontinuity-based local average treatment effects of .17 in the natural log of expenditure per capita for the same cash transfer program in Lebanon from 2016 to 2019. This effect was based on transfers of 175 USD per month, when average monthly expenditure was approximately 440 USD – the transfer being 40 percent of mean expenditure. The Lebanese economy has since experienced severe recession over the past few years. As of the June 2021 baseline survey, mean household expenditure was 1.3 million LBP (87 USD). The randomization increased assistance by 800,000 to 1 million LBP (53

Table 3: Household characteristics prior to assignment: balance and test of targeting

Panel A: Balance in population: demographics	HH size	share children under 5	share adults over 50	share men 18-50
	(1)	(2)	(3)	(4)
Food insecure arm	0.02	-0.002^{*}	0.001	0.002
	(0.01)	(0.001)	(0.001)	(0.001)
Poor food cons. arm	0.01	-0.001	0.001	0.001
	(0.01)	(0.001)	(0.001)	(0.001)
Multidim. deprived arm	0.02	-0.001	0.001	0.0001
	(0.01)	(0.001)	(0.001)	(0.001)
Expenditure poor targeting group mean	4.266	0.166	0.086	0.21
p-value, all coef = 0	0.19	0.14	0.87	0.31
N	>300,000	>300,000	>300,000	>300,000
R^2	0.0000	0.0000	0.0000	0.0000

Panel B: Balance in population: background measures	female-headed	any disability	share no educ.	share secondary educ.
	(1)	(2)	(3)	(4)
Food insecure arm	-0.003	0.002	0.002	-0.001
	(0.002)	(0.002)	(0.001)	(0.002)
Poor food cons. arm	-0.002	0.001	0.002	-0.0003
	(0.002)	(0.002)	(0.001)	(0.002)
Multidim. deprived arm	-0.002	0.001	0.0005	0.0001
	(0.002)	(0.002)	(0.001)	(0.002)
Expenditure poor targeting group mean	0.235	0.14	0.131	0.302
p-value, all coef = 0	0.49	0.81	0.31	0.93
N	>300,000	>300,000	>300,000	>300,000
R^2	0.0000	0.0000	0.0000	0.0000

Panel C: Balance in 2021 survey sample	Is expenditure poor	Is food insecure	Has poor food cons.	Is multidim. deprived
	(1)	(2)	(3)	(4)
Food insecure arm	-0.01	0.01	0.03	-0.01
	(0.01)	(0.02)	(0.02)	(0.01)
Poor food cons. arm	-0.02	0.01	-0.01	-0.01
	(0.01)	(0.02)	(0.02)	(0.01)
Multidim. deprived arm	0.001	0.02	-0.01	-0.01
	(0.01)	(0.02)	(0.02)	(0.01)
Expenditure poor targeting group mean	0.853	0.464	0.419	0.112
p-value, all coef = 0	0.64	0.76	0.15	0.71
N	4,953	5,017	5,017	5,017
R^2	0.0003	0.0002	0.001	0.0003

Panel D: Test of targeting allocation to poor, by treatment arm				
Outcome: receives full assistance	Monetary poverty	Food insecurity	Food consumption	Multidim. poverty
	(1)	(2)	(3)	(4)
Poor $\times$ in treatment arm	0.15^{***}	0.20^{***}	0.11^{**}	0.23^{***}
	(0.04)	(0.03)	(0.03)	(0.05)
Outcome mean, poor in control arms	0.54	0.53	0.51	0.53
Outcome SD, poor in control arms	0.5	0.5	0.5	0.5
N	4,953	4,953	4,953	4,953
R^2	0.05	0.03	0.01	0.01

Note: Panels A-C of this table report the results of a test of balance given by equation 1 using pre-assignment characteristics available for the entire population. Panel D reports the test of targeting allocation given by equation 4. $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 4: First-stage estimates of assistance received and covariate balance among compliers in 2022 outcomes sampling frame

Panel A: Compliance effects on assistance received in 2022 sampling frame

	Expenditure targeting compliers assistance (LBP)	rCSI targeting compliers assistance (LBP)	FCS targeting compliers assistance (LBP)	MDDI targeting compliers assistance (LBP)
	(1)	(2)	(3)	(4)
Assisted in j ($W_i(j) > W_i(k)$)	877,408.60***	900,852.70***	835,370.50***	1,002,401.00***
	(25,334.63)	(20,641.58)	(28,657.29)	(21,608.37)
N	2,877	3,285	2,281	2,943
R^2	0.29	0.37	0.27	0.42

Panel B: Balance in 2022 sampling frame (compliers): demographics

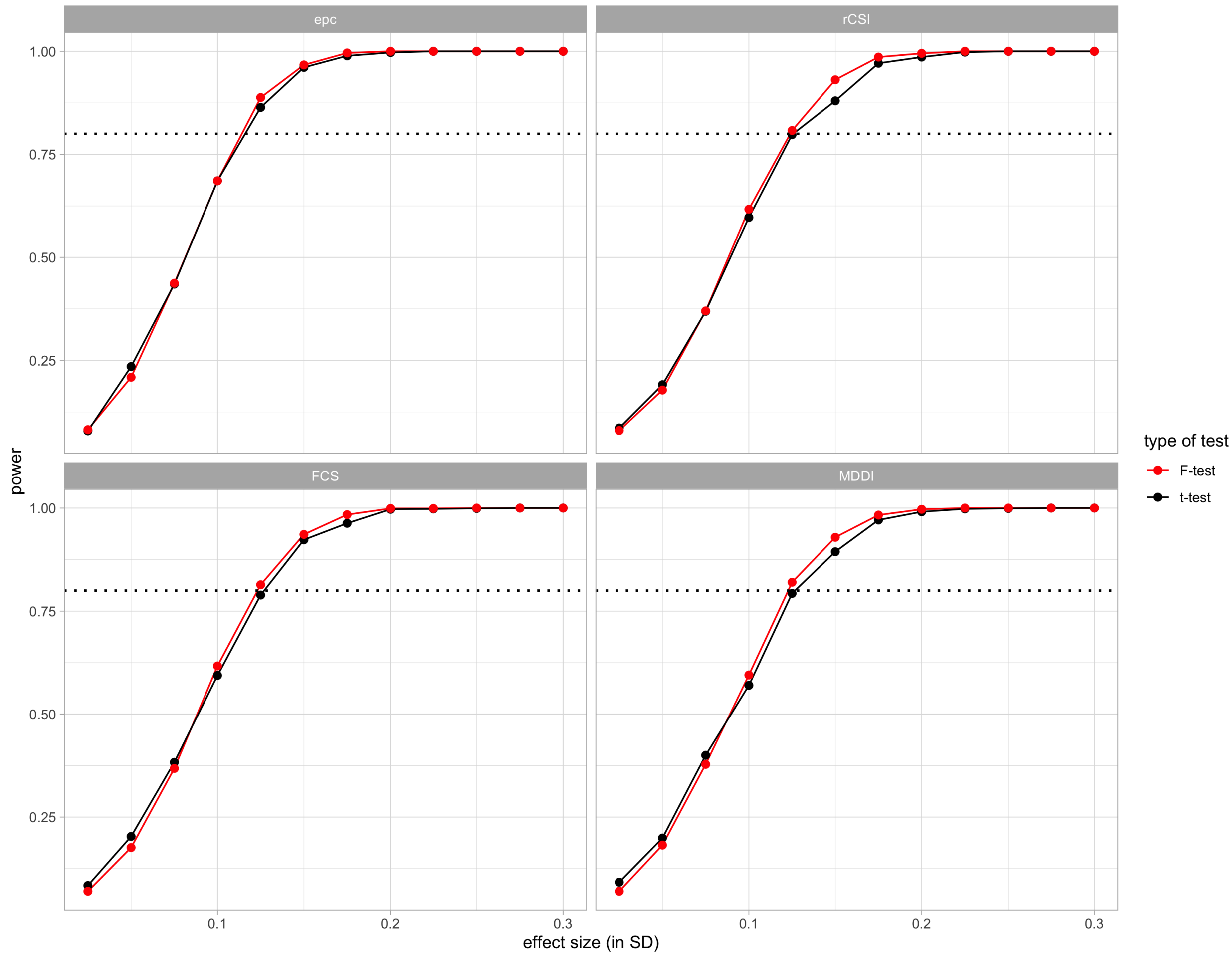
	HH size	share children under 5	share adults over 50	share men 18-50
	(1)	(2)	(3)	(4)
Assisted in j ($W_i(j) > W_i(k)$)	0.02	0.004	−0.01	−0.003
	(0.04)	(0.004)	(0.004)	(0.004)
N	11,386	11,386	11,386	11,386
R^2	0.0000	0.0001	0.0001	0.0001

Panel C: Balance in 2022 sampling frame (compliers): protection measures

	female-headed	any disability	share no educ.	share secondary educ.
	(1)	(2)	(3)	(4)
Assisted in j ($W_i(j) > W_i(k)$)	−0.001	0.01	−0.0003	−0.01
	(0.01)	(0.01)	(0.01)	(0.01)
N	11,386	11,386	11,386	11,386
R^2	0.0000	0.0001	0.0000	0.0001

Note: This table reports the results of tests of balance given by equation 3 using pre-assignment characteristics available for compliers in the 2022 VASyR sampling frame (main sample and compliers sample). The third panel reports first stage estimates of assistance amount on assignment to the larger assistance package.
*p < .05; **p < .01; ***p < .001

Figure 1: Power analysis of aggregate program design effects



Note: Graphic depicts power series for Equation 1 across simulated single-arm effect sizes. The horizontal dotted line reflects 80% power.

to 66 USD, given by Panel A of Table 4) – approximately 60 to 75 percent of mean expenditure. Figure 2 contains the power series across within-group complier sample sizes.

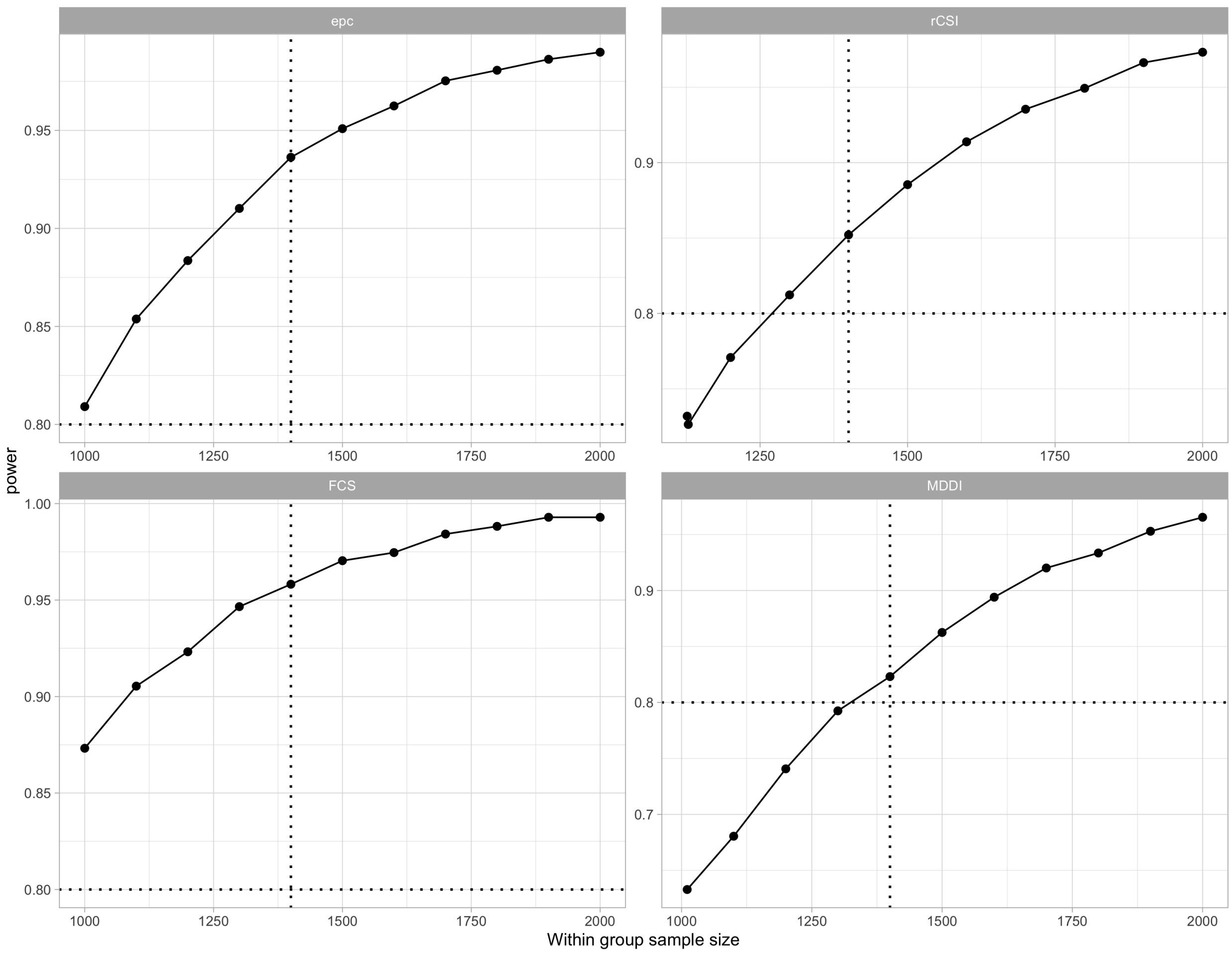
5.3.1 Data processing procedures

We will follow standard data cleaning and processing procedures for household survey data. We commit to not impute values into missing data, nor infer variable corrections at the household level from other variables.

Our data cleaning procedures may require:

- inspection of outlier observations to adjust for apparent errors in data entry by enumerators, particularly for (but not limited to) measures of expenditure; for these, we reserve the right to

Figure 2: Power analysis of CATE effects simulating 0.175 SD effect



Note: This graphic depicts power across sample sizes for compliers of each targeting arm. The vertical dashed line indicates the projected total sample size with an additional random sample of 2,000 complier households added to the annual survey. The total sample size (horizonal axis) contains compliers from the main and additional survey sample for compliers to each treatment arm. The horizontal dotted line reflects 80% power.

impute missing values for outliers, defined as those being in the 1% tails of the distribution of a given variable,

- treating zeros in variables as missing values, thus removing these records from the analysis sample,
- other sample restrictions or variable cleaning that would indicate poor data quality to reasonable and experienced empirical researchers.

Implementation of any of these procedures will be fully and clearly documented in the manuscript.

6 Qualitative focus group discussions

To complement our understanding of the effects of using alternative poverty measures to target social assistance, the research team will conduct focus group discussion meetings with select group of refugees in different locations. We hypothesize that the differential effects of alternative measures of poverty can materialize via (1) coping strategies adopted by non-beneficiaries, and/or (2) spending practices adopted by beneficiaries. If a demographic characteristic makes a group less likely to receive assistance in a treatment arm and if this demographic characteristic is simultaneously associated with adoption of healthier coping strategies, this would explain marginally higher welfare in this treatment group. Similarly, a demographic characteristic can also make a group more likely to receive assistance and be at the same time associated with welfare-enhancing expenditure opportunities. Hence, the main aim of qualitative focus group discussions is to collect data on coping strategies and spending practices, and to explore how these relate to demographic characteristics and contextual factors.

We will conduct at least 12 focus group discussion meetings over July - August 2022. Half of these meetings will take place in urban settings, while the other half will be held in rural settings. Beneficiaries and non-beneficiaries will not be brought together in the meetings. Participants will be randomly selected from among the compliers by the research team. The implementing agencies will then contact and invite potential participants to the meetings. Both during the invitation stage and just before the focus group meeting, the participants will be extensively informed about the research, and their informed consent will be sought. At least two members of the research team will be present during the meetings.

7 Other concerns

7.0.1 Anticipatory effects

Because the targeting algorithm (a) changes year to year, and (b) is not shared in detail with field staff or potentially eligible households, and (c) the assignment of households to treatment arms was not shared with implementing agencies, there is no way for households to know which treatment arm they were placed into prior to the cycle, not could they know during or after the assistance cycle began. These programmatic features make anticipation of treatment arm, complier status, or eligibility effectively impossible.

7.0.2 Attrition

Because the analysis is cross-sectional, we do not face traditional concerns about follow-up panel survey attrition. We will use administrative records, to the extent possible/available, to understand whether patterns of lost contact between agencies and refugee households is at all related to assignment to treatment arms. Should there be any evidence of this, we will use bounding techniques to correct for the effects of attrition in the discussion of any point estimates.

References

ALATAS, V., BANERJEE, A., HANNA, R., OLKEN, B. A., PURNAMASARI, R. and WAI-POI, M. (2016). Self-targeting: Evidence from a field experiment in indonesia. *Journal of Political Economy*, **124** (2), 371–427.

—, —, —, — and TOBIAS, J. (2012). Targeting the poor: Evidence from a field experiment in indonesia. *American Economic Review*, **102** (4), 1206–40.

ALTINDAĞ, O. and O'CONNELL, S. (2022). The short-lived effects of unconditional cash transfers to refugees.

—, —, ŞAŞMAZ, A., BALCIOĞLU, Z., CADONI, P., JERNECK, M. and KUNZE FOONG, A. (2021). Targeting humanitarian aid using administrative data: model design and validation. *Journal of Development Economics*, **148**.

ANDERSON, M. L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevaluation of the abecedarian, perry preschool, and early training projects. *Journal of the American Statistical Association*, **103** (484), 1481–1495.

BANERJEE, A., NIEHAUS, P. and SURI, T. (2019). Universal basic income in the developing world. *Annual Review of Economics*, **11** (1), 959–983.

BASURTO, M. P., DUPAS, P. and ROBINSON, J. (2020). Decentralization and efficiency of subsidy targeting: Evidence from chiefs in rural malawi. *Journal of Public Economics*, **185**, 104047.

BROWN, C., RAVALLION, M. and VAN DE WALLE, D. (2018). A poor means test? econometric targeting in africa. *Journal of Development Economics*, **134**, 109–124.

DEATON, A. (2006). *Measuring Poverty*, Oxford: Oxford University Press.

HAUSHOFER, J., NIEHAUS, P., PARAMO, C., MIGUEL, E. and WALKER, M. (2022). *Targeting impact versus deprivation*. Tech. rep., UC San Diego.

KARLAN, D. and THUYSBAERT, B. (2019). Targeting Ultra-Poor Households in Honduras and Peru. *The World Bank Economic Review*, **33** (1), 63–94.

MEYER, B. D. and SULLIVAN, J. X. (2012). Identifying the disadvantaged: Official poverty, consumption poverty, and the new supplemental poverty measure. *Journal of Economic Perspectives*, **26** (3), 111–36.

PREMAND, P. and SCHNITZER, P. (2020). Efficiency, Legitimacy, and Impacts of Targeting Methods: Evidence from an Experiment in Niger. *The World Bank Economic Review*, **35** (4), 892–920.

RAVALLION, M. (2009). How Relevant Is Targeting to the Success of an Antipoverty Program? *The World Bank Research Observer*, **24** (2), 205–231.

SEN, A. (1981). *Issues in the Measurement of Poverty*, London: Palgrave Macmillan UK, pp. 144–166.

— (1999). *Development As Freedom*. New York: Knopf.

YOUNG, A. (2012). The african growth miracle. *Journal of Political Economy*, **120** (4), 696–739.

A Appendix: Targeting methodology detail

The program we study has three assistance packages which are used to create the overall assistance schedule. One is a flat-rate transfer of 800,000 LPB per month, which we refer to here as flat-rate cash. The next is a transfer of 300,000 LBP per month per family member, up to a maximum of six members (180,000 LBP), referred to as scaling cash. The third is a transfer of 300,000 LBP per family member, up to six, on a food voucher card redeemable at contracted shops, referred to as the food voucher. In general, the poorest households receive both the flat-rate and scaled packages, which we refer to as "full assistance." The next poorest segment receives either the flat-rate cash or the food voucher, and the least poor receive nothing.

The targeting of cash assistance comprises a two-layer system to differentiate the type of poor targeted by each treatment arm in our experiment. The first step involves developing a LASSO regression model using all the demographic information available in the administrative data set except the location information to predict the expenditure per capita for all households in the administrative data base.[9]

Next, we calculate the district share of cases that are defined as vulnerable according to four commonly used measures of vulnerability. Specifically, for each district i, we calculate using the VASyR 2021 and survey weights:

$$\mathrm{EPC}_i = \frac{\text{N cases under SMEB in district } i}{\text{Total N cases living under SMEB}}$$

$$\mathrm{rCSI}_i = \frac{\text{N cases that have an rCSI score over 18 in district } i}{\text{Total N cases that have an rCSI score over 18}}$$

$$\mathrm{FCS}_i = \frac{\text{N cases that have an FCS score below 42 in district } i}{\text{Total N cases that have an FCS score below 42}}$$

$$\mathrm{MDI}_i = \frac{\text{N cases that have an MDI score over 1/3 in district } i}{\text{Total N cases that have an MDI score over 1/3}}$$

Where the number of cases (N) in these calculations is based on sample-weighted estimates from the VASyR survey.

In Figure A.1 and Table A.1, we present the shares of vulnerable according to the metrics

[9] The expenditure per capita targeting model has exclusion error of 17.8% and inclusion error of 34.2%. These error rates are comparable to the accuracy rates of the prediction models that are used in previous years for the same programs as well as the accuracy rate of the median cash assistance programs implemented in other countries Altındağ *et al.* (2021).

above, by district. We then project an allocation of the full assistance caseload to each district proportional to each of the four measures described above. The idea is to simulate the geographic distribution of the full package assistance if we used any of the vulnerability share distributions alone to allocate caseloads at the district level.

Within each district, we rank the households based on their predicted expenditure per capita as calculated in step 1. Using a bottom-up approach, we determine the case with the highest predicted expenditure score who would be eligible for the full package according to the simulated beneficiary quota. This household then gives the threshold score for full assistance eligibility under the district level allocation determined in step 2. Each district then has four threshold scores, one for each treatment arm (poverty target). Importantly, there are four eligibility outcomes (counterfactuals) for each household in the population based on these threshold scores.

We next randomize cases to EPC, rCSI, FCS, and MDI targeting arms. For each family, we calculate the difference between the baseline score of the household in step 1 and the threshold score calculated in step 2, as determined by their district and group assignment. This adjustment is where the research design is implemented. At this step, the adjustment to the original score makes households more (or less) likely to be eligible for assistance depending on their location and the treatment arm to which they were assigned. The final scores reflect (i) the geographic prioritization based on the alternative measures of vulnerability, which (ii) still allocates resources to the households with the lowest predicted expenditure within a district.

Finally, there are three groups in the population who are subject to slightly different assistance schedules. The first group are those who were not assisted by any of the WFP's programs in the prior year, who comprise approximately 40 percent of the refugee population. The poorest 20 percent of these households receive the full package, the next 40 percent receive the flat-rate cash, and the remainder are not supported. The next group includes those who were assisted by any WFP program in the prior cycle, who comprise approximately 55 percent of the population. The poorest 50 percent receive the full package, and the remainder receive the food voucher. Finally, there are households who are not eligible for consideration for the full package due to a lack of verification regarding the household's status. These households comprise only 7.5 percent of the population. Approximately 85 percent are supported with flat-rate cash (from UNHCR), and the remainder are unsupported.

Counterfactual assistance calculations simulate a given household being assigned to a different treatment arm, which may cause a change in their relative position in the assistance schedule. Because we have access to the procedure for generating these assistance schedules, we can precisely calculate assistance amounts for every household under any counterfactual targeting arm.

B Administrative information

B.1 Funding

This study has not received external funding.

B.2 Institutional Review Board

This study was approved by Emory University Institutional Review Board on 23 December 2021 (#3157). Primary data collection in the qualitative focus group discussions is undergoing a separate review (Emory IRB #4634).

B.3 Declaration of interest

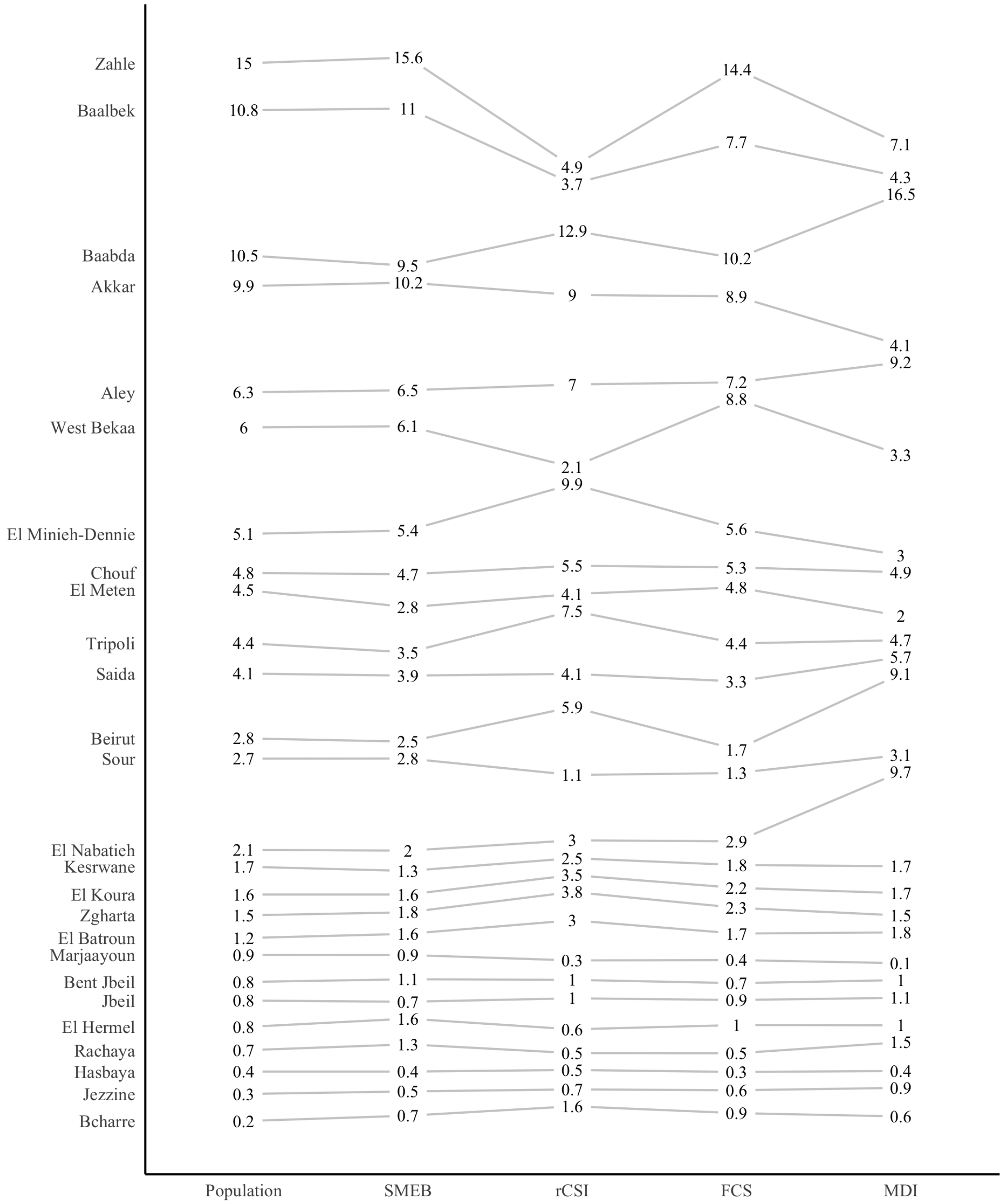
This study was reviewed for financial conflict of interest by Emory University Institutional Review Board. The authors have no competing interests to declare.

B.4 Acknowledgements

We are grateful to partner organizations in Lebanon (UNHCR and WFP), particularly Rim Achour, Susanne Carl, and Marco Principi.

C Additional Tables and Figures

Figure A.1: Share of total vulnerable by district and measure



Note: This figure shows the variation in the share of vulnerable populations defined by four treatment arms in the study. The first column reflects the population shares by district. The second column shows each district's share of the total population who lives under the survival minimum expenditure basket (SMEB) threshold. The third column shows districts' share of severe or moderate food insecure households. The fourth column shows the districts' share of households with poor or borderline food consumption, and the fifth column shows districts' share of households moderately or severely multidimensionally deprived. The figure has a separate unlabeled *y*-axis within each district.

Table A.1: Means of poverty measures and share below poverty threshold, by district

District	mean log EPC	mean rCSI	mean FCS	mean MDI	share EPC vuln	share rCSI poor	share FCS poor	share MDI poor
Akkar	12.26	-18.12	47.11	-0.13	0.94	0.37	0.42	0.04
Aley	12.51	-16.94	41.41	-0.16	0.87	0.40	0.53	0.12
Baabda	12.49	-21.64	43.35	-0.21	0.88	0.54	0.49	0.16
Baalbek	12.31	-10.48	49.08	-0.14	0.94	0.15	0.34	0.04
Bcharre	12.77	-29.54	42.85	-0.17	0.78	0.81	0.50	0.07
Beirut	12.81	-30.26	55.45	-0.23	0.71	0.75	0.26	0.26
Bent Jbeil	12.58	-16.43	58.17	-0.14	0.86	0.28	0.24	0.10
Chouf	12.59	-19.80	44.04	-0.16	0.84	0.46	0.51	0.09
El Batroun	12.57	-27.08	44.27	-0.16	0.89	0.75	0.47	0.08
El Hermel	12.09	-12.11	45.90	-0.12	0.97	0.19	0.44	0.10
El Koura	12.56	-32.12	39.51	-0.19	0.85	0.86	0.60	0.10
El Meten	12.98	-16.62	43.08	-0.14	0.60	0.41	0.52	0.04
El Minieh-Dennie	12.47	-31.20	41.47	-0.16	0.92	0.78	0.50	0.06
El Nabatieh	12.55	-23.73	43.23	-0.29	0.86	0.60	0.67	0.43
Hasbaya	12.72	-19.44	70.27	-0.16	0.76	0.56	0.20	0.10
Jbeil	12.67	-17.03	43.90	-0.16	0.79	0.51	0.47	0.10
Jezzine	12.70	-21.92	45.78	-0.15	0.83	0.49	0.50	0.07
Kesrwane	12.83	-19.95	44.95	-0.17	0.67	0.61	0.48	0.09
Marjaayoun	12.59	-9.26	64.12	-0.09	0.87	0.16	0.19	0.01
Rachaya	12.35	-13.68	55.85	-0.14	0.95	0.25	0.27	0.07
Saida	12.70	-17.54	49.67	-0.17	0.86	0.41	0.39	0.13
Sour	12.46	-9.92	57.05	-0.18	0.88	0.17	0.22	0.10
Tripoli	12.54	-30.19	42.65	-0.17	0.79	0.77	0.50	0.11
West Bekaa	12.36	-9.69	40.68	-0.11	0.90	0.14	0.65	0.05
Zahle	12.45	-12.30	49.22	-0.14	0.92	0.14	0.44	0.04
Zgharta	12.63	-31.88	41.68	-0.16	0.86	0.84	0.57	0.07

Notes: Table contains means of underlying poverty measures and the share in each district below threshold for poverty/vulnerability.