Efficiency versus effectiveness in targeting scaled social assistance programs: Evidence from a nationwide policy experiment

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Abstract

Aid agencies and their donors are highly attuned to simultaneous goals of targeting efficiency and program impact. We analyze the relationship between these two aspects of program design through a high-value unconditional cash transfer program that tested the implications of targeting different types of deprivation in a nationwide randomized field experiment. Changing the targeted deprivation results in prioritizing assistance for households with distinct demographics and economic constraints. Despite these differences, targeting efficiency does not necessarily lead to program effectiveness, even when the program's success is narrowly defined as alleviating the specific type of deprivation it aims to address. There is modest heterogeneity in program effectiveness related to targeting choices, but more substantial heterogeneity across locations – highlighting the primacy of local market conditions for program effectiveness. Data typically available to program designers can be used effectively to prioritize specific populations, but this does not necessarily lead to an allocation of resources where the program will be more effective.

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

JEL Classification: I38, I32, O12, D74

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1 Introduction

Social protection programs often face the dual goals of reaching those most in need while maximizing the impact of resources expended. Beneficiary targeting is a core component of achieving these objectives. Antipoverty programs in developing countries make use of various targeting methods including proxy-means testing, community input, self-targeting, and geographic allocation rules. These methods, whether used alone or in combination, aim to balance administrative costs with targeting efficiency (Coady et al., 2004; Banerjee et al., 2022). Targeting efficiency, defined as the rate at which a program reaches the poorest, may not necessarily result in the largest reductions in poverty, however, with tradeoffs in program effectiveness across targeting rules depending on the market failures faced by beneficiaries (Ravallion, 2009; Hanna and Karlan, 2017; Haushofer et al., 2022a). Targeting links beneficiary selection with potential heterogeneities in program effectiveness, and is therefore critical for tailoring programs according to implementers' goals and the needs of the target population.

In this paper, we quantify how allocating a fixed budget for social assistance across communities according to alternative poverty targets changes both the set of program beneficiaries and program effectiveness. We study a year-long, nationwide randomized evaluation initiated in mid-2021 by humanitarian agencies in Lebanon that targeted a high-value unconditional cash transfer (UCT) program to the full population of Syrian refugees using a two-tiered system: initial geographic targeting followed by a proxy-means test (PMT). The program is structured such that the geographic allocation rule in the first step distributes the assistance budget across 26 administrative districts based on each area's share of the national poor. Following this, aid is allocated to the poorest households within each district based on a PMT of per capita expenditure.

During the evaluation year, the implementing agencies concurrently employed four distinct geographic allocation rules and randomized the full population of potentially eligible households to one of them. These targeting arms served similar populations by design and targeted the same aggregate number of beneficiaries, and varied only in how they distributed an identical amount of aid across 26 districts in the country. Each arm used a different poverty indicator for geographic targeting — specifically, monetary poverty, food insecurity, nutritional deficiency, or multidimensional deprivation. Due to differences in the experience and expression of poverty throughout the country, this generated significant variation in assistance eligibility across similar households randomized to different targeting arms. The research design therefore features multiple treatment arms and no control group, which is typical in studies investigating the implications of alternative targeting strategies (Premand and Schnitzer, 2020; Alatas et al., 2012; Hanna and Karlan, 2017).

Unique among studies in the targeting literature, we simulated the application of each allocation rule to every household *ex ante*, allowing us to directly observe four counterfactual transfer amounts at the household level. We are therefore able to identify households for whom the randomized targeting rule changes the amount of assistance received and those for whom it does not – whom we refer to throughout as marginal and inframarginal beneficiaries, respectively. In our setting, the geographic targeting rule does not change beneficiary status or assistance received for approximately 65% of households. The remaining 35% comprise marginal beneficiaries who receive a different assistance amount in at least one of the counterfactual targeting arms. These households are precisely those among whom the program reallocates resources when changing the poverty target. As one objective is to examine changes in program outcomes induced by changing the targeting rule, marginal beneficiaries represent the policy-relevant population in such settings.

We first document marked differences in vulnerability indicators as well as demographics and market access such as household sizes, dependency ratio, household head's gender, informal lending/borrowing, risk sharing, and ability to smooth consumption for the marginal beneficiaries across treatment arms. Despite these differences, we observe minimal net gains in economic well-being outcomes favoring any particular targeting strategy. We also show similar levels of households' reported levels of satisfaction, fairness, and perceptions of accuracy across targeting strategies.

The experimental design enables a direct estimation of local average treatment effects (LATE) of the program among marginal beneficiaries. In line with our pre-registered analysis plan, these households were over sampled and surveyed specifically for this study to power pre-specified effect sizes. We find that an additional 1MM LBP monthly, equivalent to the median household benefit, positively affects the targeted poverty outcomes. Effect size vary between statistically insignificant 0.02SD and 0.21SD depending on the poverty outcome and targeting arm. Surprisingly, targeting efficiency does not yield effectiveness *per se*, even when narrowly defined as alleviating the type of deprivation that the program targets. For example, we find that marginal households in monetary poverty or multidimensional deprivation targeting improve food insecurity, with effect sizes between .1 and .15 SD. However, we find no effects on food security among those who would be included in the program when targets food insecurity. Additionally, we do not observe a clear link between beneficiary profiles and the heterogeneity in program effects. Even when targeting demographically distinct households, different targeting arms yield similar program outcomes among marginal beneficiaries.

In terms of non-targeted outcomes, we observe sizable gains in children's school enrollment ranging from 7 to 9 percentage points exclusively among beneficiaries targeted by our two consumption-based poverty measures. Qualitative interviews suggest that these households face high costs of to school attendance, rather than institutional or supply-side constraints.

Finally, our sample of marginal beneficiaries allows us to estimate separate program effects for each district, quantifying the variation in program effect heterogeneity across locations. These estimates set an upper-bound benchmark for the effectiveness achievable through geographic targeting when program designers have strong preferences for effectiveness. In contrast to effect variation across targeting methods, we find substantial variation in program effects by location. When we decompose the variation in treatment effects, the explanatory power of location-specific effects is much larger than the type of poverty that beneficiaries are targeted by or other beneficiary specific features. We conclude that local economic conditions and the implementation context of a social assistance program likely governs the effectiveness of such programs more so than does targeting design. Data typically available to program designers can be used to increase the efficiency of prioritizing specific populations, but this does not necessarily lead to an allocation of resources where the program will be more effective. This finding is consistent with the notion that variations in program effects are likely attributable to area-specific shocks, or aggregate shocks interacting with existing location-specific constraints.

Our findings contribute to a large literature on the targeting of social assistance, which has focused on both the determinants of targeting efficiency and the relationship between targeting and program effectiveness, including but not limited to Ravallion (2009); Alatas et al. (2012, 2016); Stoeffler et al. (2016); Brown et al. (2018); Hanna and Olken (2018); Karlan and Thuysbaert (2019); Basurto et al. (2020); Premand and Schnitzer (2020) and Haushofer et al. (2022b). These studies either compare the efficiency of different targeting approaches or quantify the overall program effects of a specific program design. Our study is unique in its ability to both characterize populations affected by a key program rule and subsequently estimate the program's impact on these households. This allows us to compare both beneficiary characteristics and program effectiveness estimates across multiple strategies and quantify the trade-offs. The methodology we propose is easily adaptable to other environments that wish to assess tradeoffs across alternative design parameters, offering a general, scaleable framework that existing cash programs can adopt to test the implications of targeting parameters within a causal context.

We also contribute to the study of geographic targeting – the prioritization of resources across localities – for program effectiveness. Geographic targeting is commonly used in conjunction with proxy means tests (PMTs) and other traditional targeting methods.[1] With the growing availability of spatial data and advancements in predictive tools, the use of this approach as a targeting strategy is

[1] Mexico's PROGRESA, for example, serves as a prime example. Initiated with the aim of alleviating rural poverty in Mexico, the program first identified economically disadvantaged regions in the country using aggregate poverty metrics, then further narrowed focus to communities with limited access to infrastructure and basic services within those regions. The final step of the targeting process involved a PMT, leveraging observable household assets and features to gauge relative household poverty, and by extension, the economic vulnerability of individual households in those communities.

rapidly expanding in settings with limited data (Abelson et al., 2014; Aiken et al., 2022; Asher et al., 2021; Blumenstock et al., 2015; Smythe and Blumenstock, 2022). Because localities face varied market inefficiencies and possess distinct capacities to share risks and mitigate shocks (Kinnan et al., 2020), their ability to smooth consumption is closely tied to the local economic environment (Hanna and Karlan, 2017). Consequently, prioritizing certain areas over others through geographic targeting necessarily results in variation in the beneficiary set and may change program effectiveness. Ours is the first study to document these tradeoffs in an experimental setting.

Finally, we relate to a growing program evaluation literature on the effectiveness of social protection in humanitarian settings. Experimental studies of social protection programs in displacement contexts have remained rare until relatively recently (Quattrochi et al., 2020), partly due to fast-paced program implementations that intend to quickly identify and reach the most needy in emergency situations being unconducive to designing and implementing randomized evaluations (Hanna and Karlan, 2017). Early studies such as Hidrobo et al. (2014) and Aker (2017) were able to examine the effectiveness of cash versus in-kind or voucher transfer modalities among refugee populations, while recent work by Schwab (2019); Sterck and Delius (2020); Sterck et al. (2020); Lehmann and Masterson (2020); Masterson and Lehmann (2020); MacPherson and Sterck (2021); Aygün et al. (2021); Kurdi (2021) and Altındağ and O'Connell (2022), among others, use either experimental or quasi-experimental methods to evaluate impacts of humanitarian aid programs on refugees' economic and social well-being.

Understanding the impact of targeting on humanitarian aid programs in particular is important for two reasons. First, like many organizations, aid agencies and their donors are highly attuned to joint programmatic goals of targeting efficiency and program impact, as well as being under pressure to increase either or both whenever possible. Second, contemporary displacement crises are extensive in scale, leading to a diverse range of vulnerabilities. These differences may emerge due to exposure to conflict, the pre-existing characteristics of the displaced populations, or varying conditions across host communities. This makes program implementers' decisions over targeting rules highly salient for prioritization and impact. We add to the body of work above as the first to evaluate the impact of alternative targeting strategies on the beneficiary population and program effectiveness in a humanitarian context.

2 Institutional Setting

As of 2022, more than 1.5 million forcibly displaced Syrians reside in Lebanon (Govt. of Lebanon & United Nations, 2023). Refugees 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, and health care, among others. In collaboration with international and local NGOs, the UN agencies' primary form of assistance is through targeted monthly unconditional cash-based transfers (UCTs). These programs annually disburse over $250 million USD, reaching between 40% and 90% of the refugee population in recent years.

The assistance cycle operates on an annual basis, and beneficiary assignment uses a proxy-means test (PMT) targeting household expenditure per capita. Since 2016, the PMT has been based on an econometric model that uses survey and administrative data held by UNHCR (Altındağ et al., 2021). In 2021-22, the program benefit structure had three tiers. The poorest eligible households (roughly 40% of the population) received 800,000 Lebanese Pounds (LBP) per month, plus 300,000 LBP per family member (up to six). Depending on a set of programmatic background factors, the middle tier reaches approximately 45% of households and provides either 800,000 LBP in cash or 300,000 LBP per person (up to six) in food voucher credit per month.[2] Those in the least-poor quintile receive no assistance. These transfer values are substantial: a household of five eligible for the highest transfer value would receive approximately 153 USD per month. According to our survey data, the median monthly expenditure for a refugee household of five in June 2021 was 90 USD. Each of the treatment arms provides more than 65 USD million over the course of the study period, reaching more than 250,000 refugee families.[3]

A central issue in implementing a program of this size is the degree of heterogeneity throughout the country in living conditions and structural constraints to economic well-being. For example, households face different price levels and varied opportunities to access food, housing, and services depending on where they live. Qualitative fieldwork indicated that those living in informal settlements, which exist throughout the country, have stronger social support networks than those living in separate dwellings more prevalent in urban areas. Households near the Syrian border typically have access to markets and community networks in their native country, which reduce food and income insecurity. In remote areas, limited access to school and medical services primarily stems from high transportation costs. Conversely, in urban areas, this limited access is often a result of congestion and challenges related to legal documentation for enrollment. These varying causes highlight the complexities in defining poverty as a uniform indicator of deprivation. As a result, the

[2]Neither potential beneficiaries nor implementing field staff were able to manipulate eligibility scores or randomization outcomes. Access to the data on scores and treatment arm assignment was highly restricted and, beyond ourselves, was available only to a small number of UN personnel tasked with program implementation. Altındağ and O'Connell (2022) confirm there is no evidence of manipulation in eligibility around score thresholds in the same setting in multiple prior annual cycles.

[3]All conversions in this paper use an exchange rate of 15,000 LBP per USD from June 2021. From 2021 onward, the Lebanese pound depreciated substantially leading to reductions in the real value of transfers. Nominal transfer values were adjusted by implementing organizations throughout 2022 to offset reductions in real values. The nominal values cited in the text refer specifically to transfers being made from September 2021 to March 2022, after which they were increased to offset currency depreciation.

relevance of using poverty measures as comprehensive indicators of deprivation and for allocating social assistance may be undermined by the varied opportunities, capacities, and livelihoods among the target household population.

During the 2021-2022 assistance cycle, humanitarian agencies evaluated the geographic targeting of their UCT program to alternative metrics of poverty as part of their program improvement efforts to inform future program design. At the outset of the assistance cycle, the agencies designed four geographic targeting criteria. The first one targeted traditional monetary poverty measured by expenditure per capita, with a poverty threshold set to the survival minimum expenditure basket, which is an expenditure-based poverty line determined by an group comprised of experts from humanitarian agencies in Lebanon.[4] The second arm targeted severely food insecure households via 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. The poverty threshold is a score of 18 or greater (out of 56), indicating high food insecurity. The third arm was 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. A score of 42 or lower (out of 112) indicates inadequate food consumption. The last arm targeted a multidimensional deprivation index (MDI) that aims to reflect multiple deprivations across basic needs of 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 is considered multidimensionally deprived.[5] These measures are frequently used by international organizations, governments, and humanitarian agencies to assess vulnerability and structure social assistance programs.

3 Study design and Empirical Framework

3.1 Beneficiary Selection

The program selects recipients in a multi-step process, depicted in Figure 1. Each year, a representative sample of refugee households is drawn from UNHCR administrative records to collect detailed information on various measures of well-being and deprivation. UNHCR and WFP use these survey data to train a predictive model of expenditure per capita using demographic information from administrative records as predictors.This prediction ranks households by their relative poverty level. Agencies select beneficiaries starting with the lowest predicted expenditure

[4] 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 basic needs such as food, shelter, heating, water, and clothing; see UNHCR (2023).

[5] See World Food Programme (2008a), World Food Programme (2008b), and World Food Programme (2023) for official definitions and guidance on the construction of the rCSI, FCS, and MDI, respectively.

per capita, and assignment to tiers of eligibility continues until resources (quotas) for that specific locality are exhausted. The implementing agencies have historically set beneficiary quotas based on an analysis of internally collected data.[6]

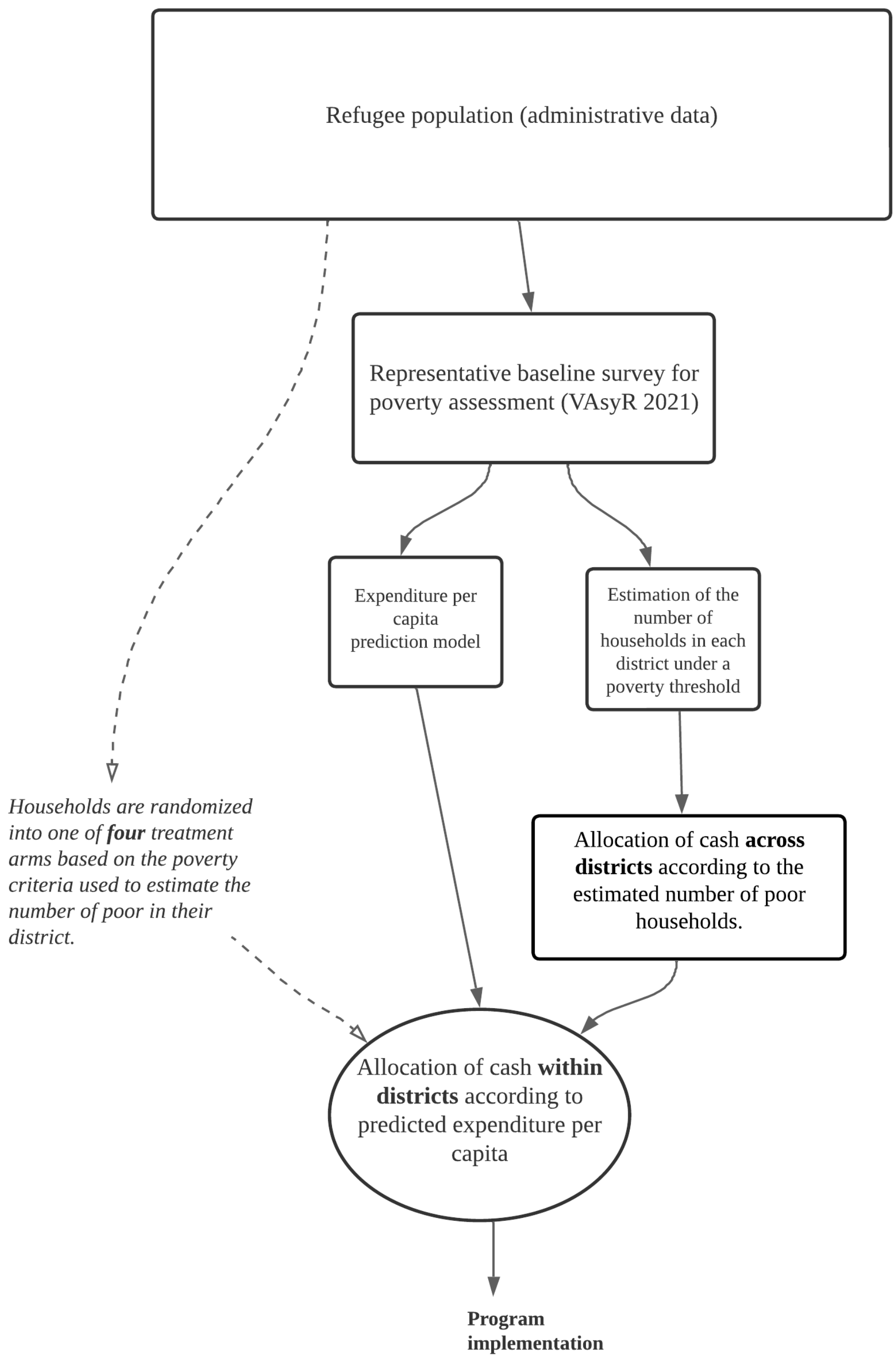
This beneficiary selection process formed the basis for the current study. In our study year, the agencies first calculated the national share of vulnerable families in each district for each of the four deprivation indicators. These figures then provided the share of district-level program resource allocations for each targeting (study) arm. Households were randomly assigned to one of four targeting arms. At the same time, a national proxy means test calculated the predicted per capita expenditure for each household. This method ranked families within each district and study arm according to their need for assistance. Finally, beneficiaries were selected based on vulnerability, starting with the most vulnerable. This selection process continued in each district and study arm until all available assistance was fully allocated.

Figure 2 depicts resource allocation variations across districts and poverty targets. Zahle, for example, hosts 15% of the refugee population, 15.6% of the refugee population living under the nominal poverty line, but just 4.9% of the population experiencing food insecurity. High expenditure poverty but low food insecurity results from a substantial proportion of households living in informal settlements in the rural agricultural heartland of Lebanon: the Bekaa Valley produces much of the wheat, barley, fruit, vegetables, and livestock; and rental prices are low or non-existent. As a result, households in Zahle are more likely to experience monetary deprivation, but less likely to engage in food coping mechanisms indicating food insecurity.

The values in Figure 2 are used to determine the assistance eligibility threshold in each district. The treatment arm targeted to monetary poverty, for example, allocates 15.6% of its resources to Zahle; the arm targeted to food insecurity allocates only 4.9%. These resources within each arm are then distributed in the typical way, starting with the households most deprived according to the PMT and ending when the allocated resources (15.6% of total in the monetary poverty arm, but only 4.9% of the total in the food insecurity arm) are exhausted.

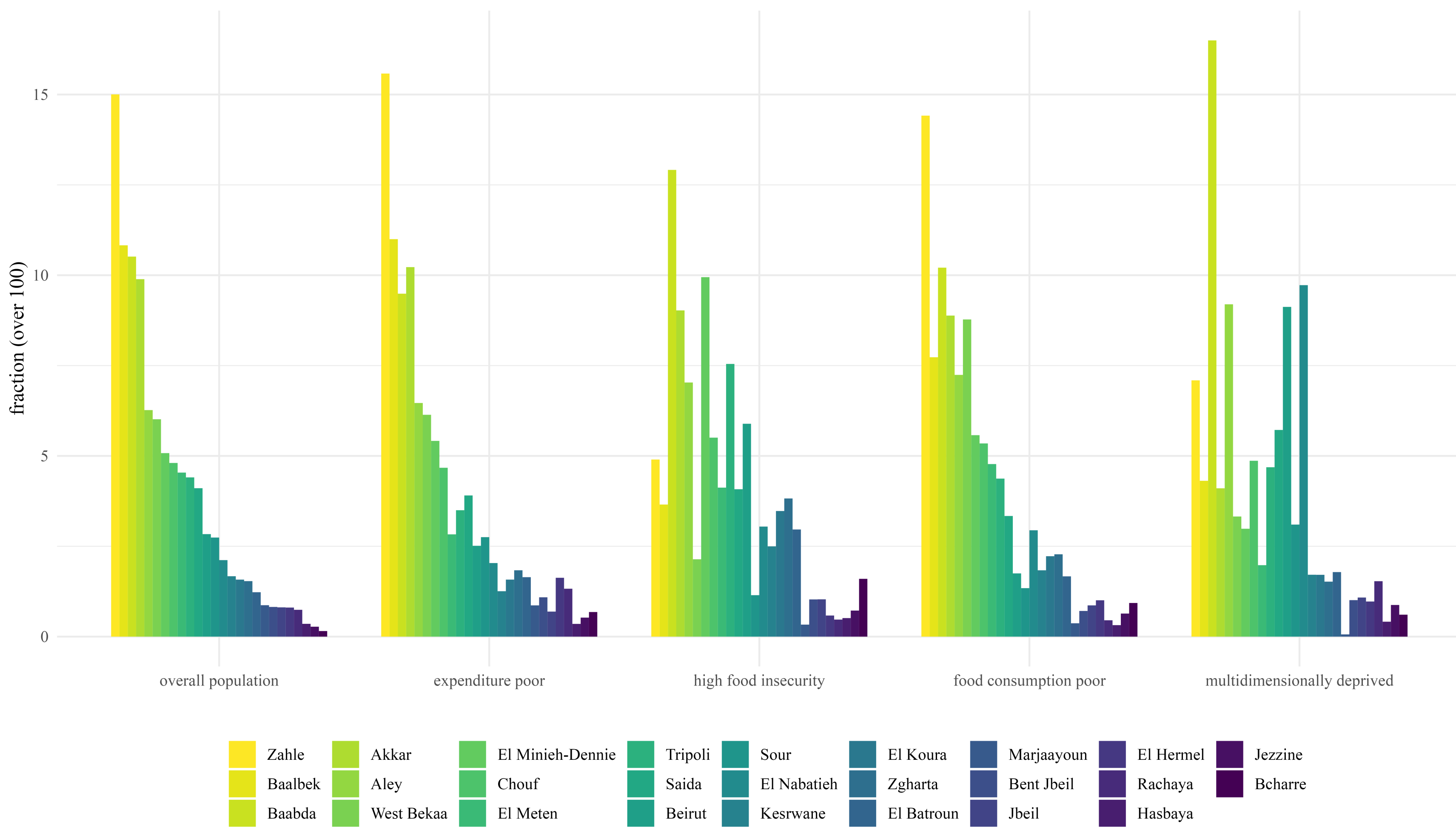
[6]This process is similar to the combination of proxy means test and geographic targeting studied in Alatas et al. (2012). For more detailed information on program structure in previous assistance cycles, see Altındağ et al. (2021) and Altındağ and O'Connell (2022).

Figure 1: Conceptual map of beneficiary selection



Note: Diagram depicts the conceptual mapping of the beneficiary selection and the implementation of the study into the ongoing program structure. **Reading:** The final allocation of assistance across the country follows a multi-stage process that considers both local poverty incidence and each household's predicted absolute poverty level.

Figure 2: District level variation by type of deprivation



Note: The graphic shows the distribution of the poor by district and poverty definition. The sum of all populations are normalized to 100. The first group of columns on the *x*-axis shows the benchmark population distribution by district. The second group, "expenditure poor" shows each district's share of the total population who lives under the survival minimum expenditure basket (SMEB) threshold. The third group, "food insecure" shows districts' share of severe or moderate food insecure households. The fourth group, "food consumption poor" shows the districts' share of households with poor or borderline food consumption, and the fifth group shows districts' share of households moderately or severely multidimensionally deprived. **Reading:** The treatment arm targeted to monetary poverty allocates 15.6% of its resources to Zahle; the arm targeted to food insecurity allocates only 4.9% to Zahle.

Due to the nature of the design, each household had four potential assistance statuses that were observable to us and program implementers, and were independent of their assignment to treatment arm. The experimental variation is therefore introduced by randomly assigning households to one of four allocation arms, which determine the targeting score threshold under which the households in a given district are eligible for assistance. In our Zahle example, the family that is marginally eligible for a given level of assistance based on their targeting score in the monetary poverty arm would receive a lower assistance amount if assigned to the food insecurity targeting arm — as 15.6% of the monetary poverty targeting arm's resources were allocated there, compared to 4.9% of the resources for the food insecurity arm. A household's eligibility is thus determined by where their targeting score falls relative to the eligibility threshold specific to their district and targeting arm. By observing the counterfactual assistance packages for each household, we determine whether the assigned targeting rule leads to a change in the assistance amount relative to being assigned to other potential targeting rules. Such households who would have had different assistance amounts had they been assigned to a different treatment arm become the focus of the subsequent analysis, as they are ones who generate tradeoffs faced by designers in the beneficiary set and program effects. These marginal beneficiaries are in the subset of families for whom the assignment to treatment arm changed the amount of assistance they received over the course of the 2021-2022 cycle.

3.2 Data and Timeline

The foundation of the administration of the assistance program, as well as our analysis, is a database of all refugee households that have made themselves known to UNHCR in Lebanon.[7] The data shared by UNHCR include demographic information, targeting scores, and both past and current assistance records, and is otherwise not unlike a basic social register. The demographic characteristics held in these data are used as features in the annual proxy-means test, and our pre-analysis plan presented tests of balance across treatment arms in an array of background characteristics in a pre-intervention snapshot of these data for the entire refugee population.

Every year, UNHCR, WFP, and their partners administer a nationally representative vulnerability survey that collects data from a sample of households on an array of living conditions, protection concerns, employment, income, and other measures of well-being and deprivation. This survey, the Vulnerability Assessment of Syrian Refugees in Lebanon (VASyR) serves as the primary data source for the empirical analysis. The VASyR survey has been collecting comprehensive data on the well-being and expenditures of refugee families since 2016, and typically surveys 4,000 to 5,000

[7] Individuals and families interested in being enrolled with UNHCR are provided with appointments to collect biographical data and vulnerability information. These data are used to provide individuals or families to programs and services for protection and assistance. Records of those found to no longer be in the country are inactivated in the database, and verification of refugees' whereabouts, family composition, and vulnerabilities takes place on an ongoing basis.

households per year across Lebanon. VASyR 2021 was conducted in May and June of 2021. For the programs, this survey provides household expenditure and district-level deprivation incidence indicators used to generate the poverty targeting model used in 2021-22.

The 2021 survey provides pre-intervention outcomes for a representative sample of households. We used these data in our pre-registered analysis plan to show strong balance across treatment arms in initial levels of the targeted measures of deprivation, as well as to confirm that targeting each alternative measure of deprivation includes more households into the program who were vulnerable according to the targeted measure.[8] Following the process described in Section 3, the agencies selected and informed beneficiaries in October 2021 of their eligibility status. Cash transfers under the studied assistance cycle were first distributed in November 2021. The agencies collected the post-intervention household survey in June and July of 2022. Subsequent to our power calculations for this study, the implementing agencies surveyed 2,091 additional households during the same period, randomly drawn from the marginal beneficiaries.

Additionally, we gathered data on beneficiary perceptions of the cash program using administrative records and a phone survey. The administrative data are derived from the Grievance Redress Mechanism (GRM) claims. This process allows refugee households to file claims for reconsideration during the initial stages of an assistance cycle. Implementing agencies select a subset of claimants for inclusion in assistance programs based on predefined criteria. A GRM claim often indicates dissatisfaction with the targeting strategy, where households file claims if they believe they are entitled to assistance but were excluded due to the targeting system. Our data encompasses all claimants who submitted a GRM claim during the application period from October 6 to November 19, 2021. The subsequent phone survey, conducted between February 7 and 18, 2022, included a random sample of 1,904 families. In addition to administrative questions about their knowledge and experience of the GRM, it also asked general questions about their overall satisfaction, perceptions of fairness, and accuracy of the program's targeting choices. Finally, we conducted 12 focus group discussions with 114 marginal beneficiaries from each targeting arm over the course of July 21 to 29, 2022 to gather detailed qualitative data about the constraints faced by these different populations and their uses of the transfers.

3.3 Potential outcomes framework

Consider household i with potential outcomes $Y_i(Z)$ and the transfer amount $T_i(Z)$ that household i would receive if assigned to arm Z. Here, i ranges over the set $\{1,2,3,\ldots,N\}$, and T can vary between zero and a positive amount. The random assignment arm Z can take values from the

[8] Out pre-registered analysis plan specifies hypotheses, data and survey sampling, power analysis, additional data collection, variable definitions, baseline balance tests, and other study-specific baseline tests. See SSR #9725, which forms an integral part of this study and which we hereby incorporate by reference.

set $Z \in \{1,2,3,4\}$ which correspond to targeting rules for monetary poverty, food insecurity, low food consumption, and multidimensional deprivation, respectively. Random assignment ensures exogeneity, as expressed in the following equation:

$$\forall j \in \{1,2,3,4\}, \quad Y_i(j), T_i(j) \perp Z_i$$

For household i assigned to arm $Z \in \{j,k\}$ where $j \neq k$, we can observe the transfer amounts the family would receive under each assignment. Based on this, we can categorize the household into one of two distinct scenarios:

$$T_i = \begin{cases} \text{Inframarginal beneficiary} & \text{when } T_i(j) = T_i(k) \text{ (equivalently, } \Delta T = 0). \\ \text{Marginal beneficiary} & \text{when } T_i(j) \neq T_i(k) \text{ (equivalently, } \Delta T \neq 0), \end{cases} \tag{1}$$

The difference between the means of the two outcome distributions provides the difference in average economic well-being between households due to the assigned targeting rule:

$$\tau_{jk} = E[Y|Z = j] - E[Y|Z = k] \tag{2}$$

which can be further decomposed by the beneficiary types described in equation 1:

$$\tau_{jk} = \begin{cases} \tau_{jk|\Delta T=0} = E[Y|Z = j, \Delta T = 0] - E[Y|Z = k, \Delta T = 0] \\ \tau_{jk|\Delta T\neq 0} = E[Y|Z = j, \Delta T \neq 0] - E[Y|Z = k, \Delta T \neq 0] \end{cases} \tag{3}$$

where the aggregate difference is a weighted average of the two:

$$\tau_{jk} = \tau_{jk|\Delta T=0} \times Pr(\Delta T = 0) + \tau_{jk|\Delta T\neq 0} \times Pr(\Delta T \neq 0) \tag{4}$$

If the assignment is exogenous and the exclusion restriction holds, $\tau_{jk|\Delta T=0}$ is zero in expectation for all outcomes. This is because the targeting rule assignment to j or k does not affect the assistance amount for these households. On the other hand, the differential impact of targeting j over k, denoted as $\tau_{jk|\Delta T\neq 0}$, can be positive, negative, or zero. This variation stems from the differences in program effects when reallocating resources among marginal beneficiaries.

To further decompose this differential impact, consider the following equation:

$$\tau_{jk|\Delta T \neq 0} \begin{cases} \tau_{jk|\Delta T > 0} = E[Y|Z = j, \Delta T > 0] - E[Y|Z = k, \Delta T > 0] \\ \tau_{jk|\Delta T < 0} = E[Y|Z = j, \Delta T < 0] - E[Y|Z = k, \Delta T < 0] \end{cases} \tag{5}$$

Where the first term represents the change in average economic well-being for beneficiaries experiencing an increase in assistance due to the assigned targeting rule. Conversely, the second term captures the change for beneficiaries with a decrease in assistance resulting from that the targeting rule assignment. We anticipate that $\tau_{jk|\Delta T>0}$ and $\tau_{jk|\Delta T<0}$ will exhibit opposite signs. A particular geographic targeting rule is therefore deemed superior to another only if the combined effect results in a net gain for one targeting approach, meaning $\tau_{jk|\Delta T \neq 0}$ is not zero. The differences expressed in equations 2, 3, and 5 quantify tradeoffs in economic well-being that arise when reallocating identical resources among various locations and beneficiaries. These quantities can easily be estimated using Ordinary Least Squares (OLS).

In addition, the study design enables us to directly estimate a local average program effect for each targeting arm. Consider a beneficiary assigned to arm $j \in Z$ where $Z = \{1,2,3,4\}$ denotes the set of targeting arms available for beneficiary allocation. Let $\mathscr{T}$ as the set of all pairs (j,k) such that $T_i(j) > T_i(k)$ or equivalently $\Delta T > 0$ for any distinct j and k. We then pool all counterfactual cases that belong to $\mathscr{T}$. Given that we can observe the counterfactual assistance amounts, a standard instrumental variables estimation yields the impact of assistance on economic well-being for households. Specifically, this impact is measured for households whose transfer amounts under the assigned treatment arm j exceed those under a counterfactual arm k. To quantify the causal impact of assistance on economic well-being for households whose transfer amounts are influenced by the assigned treatment arm j, we use the Wald estimator. This estimator is given by:

$$\tau_{j,LATE} = \frac{E[Y|Z = j, \Delta T > 0] - E[Y|Z \neq j, \Delta T > 0]}{E[T|Z = j, \Delta T > 0] - E[T|Z \neq j, \Delta T > 0]} \tag{6}$$

which can be estimated with two-stage least squares (2SLS). Given the stacked observations, we cluster the standard errors, accounting for households for whom the assigned arm yields larger amounts across various counterfactual scenarios. Due to randomization, the standard exogeneity and exclusion restrictions of the 2SLS estimator are satisfied. In our particular context, we can validate these assumptions by estimating $\tau_{jk|\Delta T=0}$. Given the significant number of cases where $\Delta T = 0$, this estimation serves as a powerful placebo test.

The 2SLS estimates in equation 6 represent the weighted average program effect for marginal beneficiaries prioritized for a larger assistance amount under targeting arm j versus other allocation rules, where the weights correspond to the relative sizes of the marginal beneficiary groups for each alternative. It's important to note that pooled sample comprises separate experimental subsamples corresponding to each district-by-poverty target cell. This allows us later to estimate Equation 6 for each district, for each poverty target, or by the combination of these to yield insights into the relative importance of location or poverty target to program effects among marginal beneficiaries. The variation in program effects derived from these estimates delineates the maximum potential that geographic targeting can achieve for these beneficiaries if the program could exclusively target effectiveness, and is discussed in Section 4.3.1.

4 Results

4.1 Balance and Validation

Population demographics by assigned targeting rule

Table 1 provides balance tests in the post-intervention sample that forms the basis of our analysis. We confirm again that post-intervention survey households were balanced across treatment arms in pre-intervention characteristics. Panels indicate the sample used, and the means of each variable in rows are presented across columns for each treatment arm. The final two columns contain the F statistic and its p-value from a joint hypothesis test based on a specification that regresses each demographic variable of interest on indicators for three of four treatment arms. Panel A contains the full post-intervention sample, and Panel B contains these tests for the marginal beneficiary population. For both samples, all background characteristics are strongly balanced across treatment arms.

Counterfactual assistance levels by targeting rules

Table 2 shows the count of households by counterfactual and the actual assignment groups. For instance, the first panel categorizes households within the counterfactual population of the expenditure program, referred to as the EPC program. These households were either assigned to treatment arm 1 with $Z_i = 1$ or to another arm $k \in \{2,3,4\}$, with counterfactual assignments indicating whether they are marginal with $T_i(1) > T_i(k)$ or $T_i(1) < T_i(k)$, or inframarginal with $T_i(1) = T_i(k)$. In total, marginal beneficiaries comprise nearly 35% of the refugee population. The first column indicates the treatment arm sample and type of beneficiary counts across rows in the treatment arm assignment columns to the right, and the row indicate the count of households in each relevant treatment arm.

The size of the population differs markedly among counterfactual pairs, which is attributed to the varying degrees of overlap among targeted poverty measures. For instance, since monetary poverty ($Z_i = 1$) and food consumption ($Z_i = 3$) are both consumption-based measures targeting similar households, there are only about 15,000 marginal households in the population. In contrast,

Table 1: Means and tests of baseline balance in endline sample

Measure	Means by targeting treatment arm: Monetary poverty	Food insecurity	Food consumption	Multidim. deprivation	Tests: F	p-value
Panel A: Full endline sample						
Household size	4.43	4.50	4.43	4.50	0.65	0.58
% aged 0-5	17.42	17.92	18.52	18.07	0.87	0.46
% aged 50 +	8.26	8.82	8.75	7.79	0.88	0.45
% male aged 18-50	19.12	19.07	18.48	19.37	0.57	0.64
% female headed	21.34	20.14	20.31	21.65	0.59	0.62
% has disabled member	15.93	15.85	15.07	15.30	0.23	0.87
% no education	11.93	12.59	12.60	11.57	0.62	0.60
% secondary education	31.44	30.87	30.66	30.97	0.15	0.93
pred. exp. p.c. (000 LBP)	285.18	283.65	283.41	284.01	0.20	0.90
Panel B: Marginal beneficiaries in endline sample						
Household size	4.22	4.24	4.16	4.23	0.32	0.81
% aged 0-5	17.55	18.57	18.96	18.70	0.89	0.45
% aged 50 +	8.37	9.20	9.65	8.05	1.13	0.33
% male aged 18-50	18.79	18.55	18.84	18.77	0.05	0.99
% female headed	20.91	20.41	18.98	20.82	0.49	0.69
% has disabled member	14.10	14.82	14.18	14.51	0.09	0.97
% no education	10.52	11.20	11.92	10.86	0.55	0.65
% secondary education	32.44	31.85	30.42	31.81	0.56	0.64
pred. exp. p.c. (000 LBP)	287.61	286.11	289.49	286.46	0.58	0.63

Note: Table presents means and tests of covariate balance among marginal beneficiaries. Data come from administrative records prior to treatment assignment. The F statistic and its corresponding p value come from the joint hypothesis tests that mean differences across all subgroups relative to the monetary poverty arm are zero. **Reading:** Among marginal beneficiaries assigned to the monetary poverty targeting arm, the average household size was 4.22 (see Panel B). Average household size among marginal beneficiaries assigned to other targeting arms were 4.24, 4.16, and 4.23. An F-test fails to reject the joint null hypothesis that the latter three means are equal to 4.22. Overall, randomized assignment to targeting arms achieved balance in baseline covariates among the full endline sample and among marginal beneficiaries in the endline sample. Baseline tests in the full sample are available in Table 3 of the pre-analysis plan, and similarly showed strong balance across targeting arms.

comparing monetary poverty with food insecurity reveals a significantly greater discrepancy in targeted populations, resulting in approximately 40,000 households whose assistance eligibility could change when choosing between those targets. The looking across rows, sample sizes are approximately equivalent due to the randomization process, which enables the empirical estimation of the causal parameters outlined in equations 4, 5 and 6.

Table 2: Marginal and inframarginal beneficiaries by treatment arm and assistance change

Pairwise margin and sample	Monetary poverty arm ($Z_i = 1$)	Food insecurity arm ($Z_i = 2$)	Food consumption arm ($Z_i = 3$)	Multidim. deprivation arm ($Z_i = 4$)
$T_i(1) < T_i(2)$	9,301	9,075		
$T_i(1) = T_i(2)$	62,633	63,671		
$T_i(1) > T_i(2)$	10,787	9,859		
$T_i(1) < T_i(3)$	3,778		3,825	
$T_i(1) = T_i(3)$	74,976		75,159	
$T_i(1) > T_i(3)$	3,967		3,836	
$T_i(1) < T_i(4)$	8,674			8,687
$T_i(1) = T_i(4)$	63,104			64,063
$T_i(1) > T_i(4)$	10,943			10,034
$T_i(2) < T_i(3)$		7,852	8,619	
$T_i(2) = T_i(3)$		67,742	67,155	
$T_i(2) > T_i(3)$		7,011	7,046	
$T_i(2) < T_i(4)$		7,701		8,076
$T_i(2) = T_i(4)$		66,693		66,555
$T_i(2) > T_i(4)$		8,211		8,153
$T_i(3) < T_i(4)$			8,495	8,463
$T_i(3) = T_i(4)$			63,888	64,565
$T_i(3) > T_i(4)$			10,437	9,756

Note: Table presents count of households in population by treatment arm and assistance comparisons. **Reading:** There are 62,633 households for whom the assistance amount is unaffected by assignment to monetary poverty targeting of food insecurity targeting.

Validation of intervention

For a targeting strategy to be efficient, it must include in the program populations that are more likely to experience the specific type of deprivation that it intends to address. Empirical evidence supporting this in our setting is provided in Table 3. The first column presents four baseline poverty rates for populations in each targeted group, derived from a simple Ordinary Least Squares (OLS) model. In this model, we regress the outcome poverty measures on random assignment, using the Monetary Poverty arm as the reference group, using the baseline representative survey. As expected, the first column reveals no significant difference in initial poverty rates across experimental targeting arms. The results in column 3, based on the same regression but limited to households eligible for the most generous assistance package, show substantial differences in poverty levels across targeting arms, indicating that targeting indeed changes the average beneficiary profile. For instance, the second panel in Table 3 indicates that 46.42% of the population in the Monetary Poverty arm is food insecure, a rate similar across other targeting arms. However, among beneficiaries, those assigned to the Food Insecurity arm are 13.18 percentage points (30 percent) more likely to be food insecure. This pattern is consistent across all targeting arms, demonstrating that each arm allocates more resources to the type of beneficiary they aim to assist.

It is important to note that targeting efficiency varies across different arms, resulting in varying inclusion and exclusion errors. Although this variation does not compromise the causal identification of the program's effects, it highlights a limitation of our study. Our results reflect the heterogeneity in local average treatment effects, which arise from targeting different types of poverty and the accuracy of the targeting method. Our study design, however, does not allow to distinctly estimate the impact of each of these factors.

4.2 Program effects

Intent to treat comparisons of targeting strategies

We first present the intent-to-treat comparisons for our pre-specified main outcomes using the monetary targeting arm as the reference group for both marginal and inframarginal beneficiaries. The figure shows estimated coefficients alongside 95% confidence intervals based on standard errors that account for a general form of heteroskedasticity. We expect to estimate relatively precise null effects for the inframarginal beneficiaries, as the randomized targeting arm does not affect their assistance eligibility. Therefore, any differences in endline economic wellbeing should predominantly be due to the marginal beneficiaries. These tests are presented in the estimates of targeting arm assignment among inframarginal beneficiaries in Figure 3, and Appendix Tables 1 - 3. The results in Figure 3 indicate that none of the differences in poverty outcomes (z-standardized indices) are substantial enough to differentiate the three alternative targeting strategies from monetary poverty targeting. This holds true for both inframarginal and marginal beneficiaries, regardless of any changes in the assistance amount dictated by the targeting rule. Similarly, no significant differences were found in secondary outcomes of livelihood coping strategies, shelter, or WASH indices (See Appendix Table 3). Importantly, these results are not due to a lack of precision. For most of our outcomes, even with the smallest sample (i.e., marginal beneficiaries), we can reject a null hypothesis of a

Table 3: Tests of balance and targeting effect on beneficiary profiles

	Full sample	std. err.	Beneficiaries	std. err.
Outcome: % expenditure poor				
Monetary poverty arm mean	85.28		95.1	
I[Z=Food insecurity arm]	-0.76	(1.44)	-6.16***	(1.52)
I[Z=Food consumption arm]	-1.55	(1.46)	-0.88	(1.34)
I[Z=Multidimensional deprivation arm]	0.1	(1.41)	-5.72***	(1.47)
Outcome: % food insecure				
Monetary poverty arm mean	46.42		46.27	
I[Z=Food insecurity arm]	0.76	(2.00)	13.18***	(2.79)
I[Z=Food consumption arm]	0.58	(2.00)	4.29	(2.95)
I[Z=Multidimensional deprivation arm]	2.07	(1.98)	8.49***	(2.76)
Outcome: % with inadequate food consumption				
Monetary poverty arm mean	41.88		39.45	
I[Z=Food insecurity arm]	3.13	(1.98)	5.6**	(2.78)
I[Z=Food consumption arm]	-0.56	(1.98)	6.28**	(2.92)
I[Z=Multidimensional deprivation arm]	-1.01	(1.95)	0.61	(2.71)
Outcome: % multidimensionally deprived				
Monetary poverty arm mean	11.23		11.36	
I[Z=Food insecurity arm]	-1.4	(1.23)	0.25	(1.8)
I[Z=Food consumption arm]	-0.92	(1.24)	-0.03	(1.88)
I[Z=Multidimensional deprivation arm]	-0.89	(1.23)	3.91**	(1.87)

Note: Table contains means of pre-intervention poverty rates and differences relative to households in the monetary poverty targeting arm among all sampled households (Columns 1 and 2) and post-intervention beneficiaries (Columns 3 and 4). **Reading:** The Full Sample column tests for balance in the indicated outcome across treatment assignment. 85.28% of households assigned to the monetary poverty targeting arm were expenditure poor, and this rate is statistically indistinguishable across all targeting arms. The Beneficiaries column tests for the effect on targeting: 95.1% of beneficiaries under the monetary poverty targeting arm were expenditure poor, which is statistically significantly higher than beneficiary households subject to food insecurity or multidimensional deprivation targeting by 6.16 and 5.72 percentage points, respectively. Targeting poor food consumption results in beneficiaries who are no less likely to be expenditure poor, however.

0.1SD difference for most economic well-being indices. We therefore conclude that none of the targeting strategies led to economically meaningful differences in a wide range of poverty outcomes. It is crucial to note that these null results do not imply the program's ineffectiveness, but rather suggest that differences in effectiveness for these outcomes are not substantial enough to generate meaningful aggregate differences in well-being. The full set of pre-registered specifications with corrections for multiple hypothesis testing are available in Appendix Table 5.

In Appendix Table 2, we present the differences in child wellbeing between the three alternative targeting strategies compared to monetary poverty targeting. Notably, we observe significant differences in average child wellbeing between the Monetary Poverty and Food Insecurity targeting arms. For example, the likelihood of having a child aged 7-15 *not* enrolled in school is nine percentage points higher among marginal beneficiaries in the Food Insecurity targeting arm. Additionally, the likelihood of a boy aged seven to 15 working is four percentage point increase in the same subgroup. These differences are substantial, representing 40 percent and 51 percent increases, respectively, considering the low prevalence of these outcomes. Consequently, these results suggest that targeting monetary poverty, a consumption-based measure of vulnerability, leads to net gains in child wellbeing compared to targeting food insecurity.

From the perspective of a program planner, the intent-to-treat comparisons provide two insights. First, the strategy employed for geographic targeting does not yield enhanced poverty outcomes for any specific arm compared to standard expenditure targeting. This holds true even for marginal beneficiaries who are exclusively targeted by the choice of targeting tool. Second, child wellbeing is differentially improved through geographic targeting of consumption-based measures of poverty.

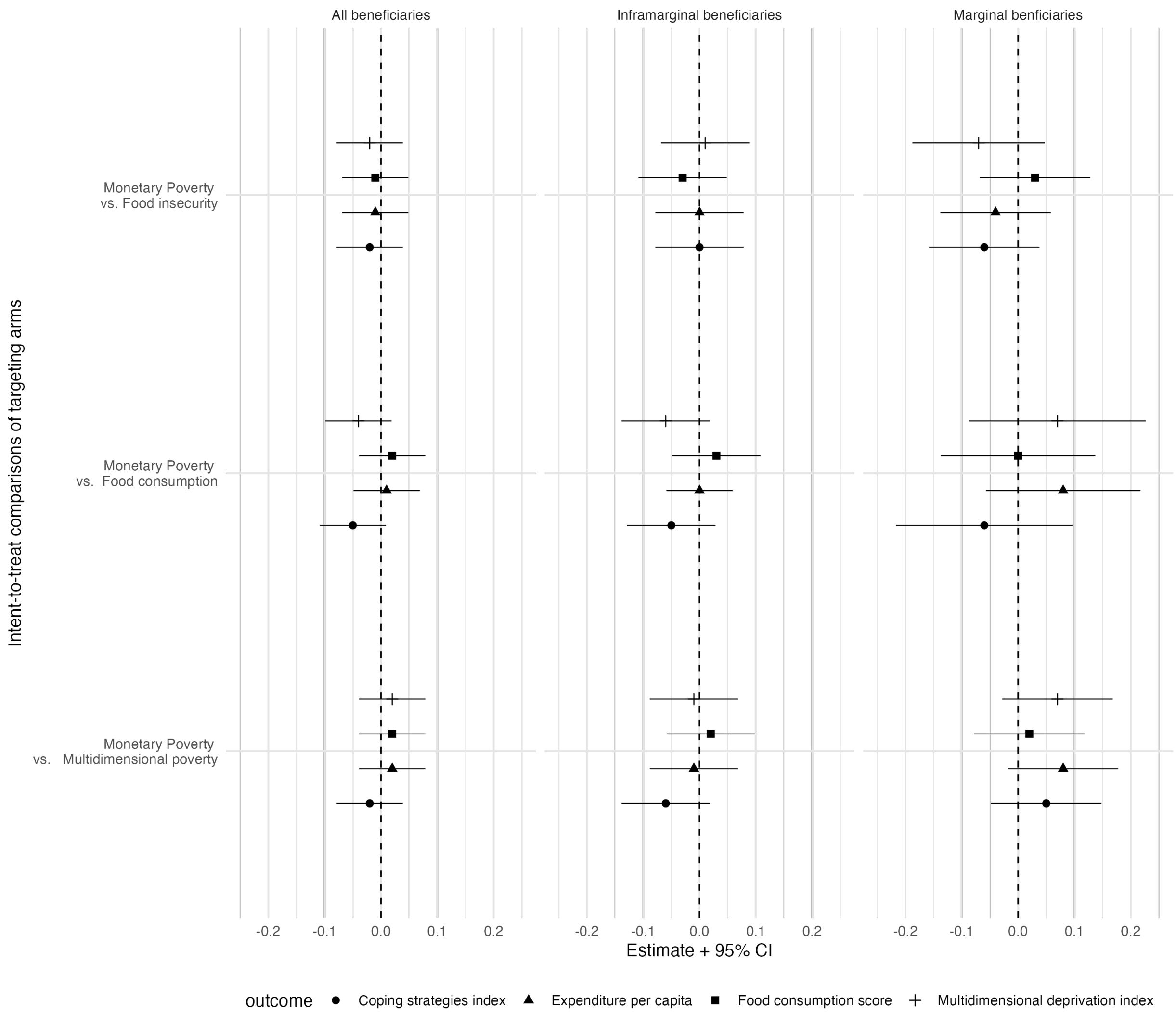
2SLS program effects among marginal beneficiaries

Given that the only difference among targeting strategies is the allocation of resources across localities and households, analyzing the local average program effects by marginal beneficiary groups and districts is crucial for understanding the heterogeneity in program effects driven by the program planner's targeting choices. We begin by identifying all beneficiaries assigned to treatment arm j or k, where treatment arm j offers a larger transfer amount, i.e., $T(j) > T(k)$. These families represent the population prioritized by the targeting approach of j. It is important to note that one family may be marginally prioritized in up to three such counterfactual scenarios. Therefore, we estimate equation 6 using a stacked sample and cluster the standard errors at the household level.

Tables 4, 5, and 6 present the 2SLS estimates for all pre-specified primary and secondary outcomes.[9] The estimated coefficients reflect the program's effect of an additional 1 million

[9] Appendix Table 4 contains the first stages, and Appendix Figures 1 - 3 contain graphical analogs of these results. The full set of pre-registered specifications with corrections for multiple hypothesis testing are available in Appendix Table 6.

Figure 3: Program ITT estimates by marginal subsample, main outcomes



Note: Figure depicts ITT effects for each sample and across four primary outcomes. The panels in rows indicate the pairwise targeting arm comparisons, and effects are estimated in columns for all beneficiaries in either arm, and separately for marginal and inframarginal beneficiaries. Coefficients are in units of standardized outcome z-score, and relate the effect of being in the alternative targeting arm relative to being assigned to monetary poverty targeting. **Reading:** No targeting strategy yields a statistically significant difference in outcomes among marginal beneficiaries relative to monetary poverty targeting.

Lebanese pounds (equivalent to 66 USD during the study period) for the marginal beneficiaries whose assistance increased due to the random assignment. The program effects have all positive signs, suggesting improvement in all primary poverty indicators. Targeting methods yield variable impacts: monetary poverty targeting enhances expenditure per capita (0.21SD), coping strategies (0.15SD), and food consumption scores (0.16SD). Multidimensional deprivation targeting uniformly mitigates all deprivation outcomes (effects ranging from 0.11SD to 0.15SD). However, targeting based on food insecurity or inadequate nutrition shows limited and often statistically insignificant benefits (0.02SD to 0.1SD), except in the case of expenditure per capita, underscoring the nuanced efficacy of different targeting approaches in poverty alleviation.

Importantly, there is no single targeting strategy that generates the highest program improvements across all outcomes, nor is the largest effect across outcomes consistently aligned with the targeted type of poverty. For example, targeting food related deprivations have limited impacts on food insecurity or food consumption despite the fact these targeting arms capture households with significantly worse food deprivation.

Table 4: LATE effects across marginal beneficiaries

	Monetary poverty targeting	Food insecurity targeting	Poor nutrition targeting	Multidimensional deprivation targeting
Outcome: Expenditure per capita				
coef.	0.21***	0.09*	0.17***	0.15***
(se)	(0.05)	(0.05)	(0.06)	(0.04)
N	1735	1377	1398	1934
Outcome: Coping strategies index				
coef.	0.15***	0.06	0.03	0.1**
(se)	(0.06)	(0.05)	(0.06)	(0.04)
N	1739	1382	1405	1938
Outcome: Food consumption score				
coef.	0.16***	0.09*	0.1	0.11**
(se)	(0.06)	(0.05)	(0.06)	(0.04)
N	1739	1382	1405	1938
Outcome: Multidimensional deprivation				
coef.	0.03	0.02	0.02	0.13***
(se)	(0.05)	(0.05)	(0.06)	(0.05)
N	1729	1303	1395	1819

Note: Table contains estimates of τ from Equation (6) in the text. **Reading:** Households marginal to the monetary poverty targeting arm have .21 standard deviation higher ln(expenditure per capita) when they receive a higher transfer due to being assigned to the monetary poverty targeting arm.

In accordance with the intent-to-treat comparisons, Table 5 and Appendix Figure 2 display substantial improvements in school enrollment for marginal beneficiaries within the monetary poverty and food consumption targeting arms, ranging between 7 and 9 percentage points, corresponding to a 32% to 42% increase in school enrollment. We observe a statistically significant increase (5 percentage points) in child labor among marginal beneficiaries of the food insecurity targeting group. Apart from this, no significant changes are noted in other child well-being outcomes across the different types of targeted marginal beneficiaries. In Table 6 and Appendix Figure 3, there are directional indications of improvements in livelihood coping strategies, shelter, and Water, Sanitation, and Hygiene (WASH) indices across all targeting arms. However, we lack statistical power to conclusively document these improvements, with the exception of enhanced shelter conditions and water and sanitation conditions (0.16SD and 0.12SD, respectively) for the marginal beneficiaries in the food consumption targeting arm.

Table 5: LATE effects across marginal beneficiaries

	Monetary poverty targeting	Food insecurity targeting	Poor nutrition targeting	Multidimensional deprivation targeting
Outcome: Child working				
coef.	-0.02	0.05**	-0.02	0.02
(se)	(0.02)	(0.02)	(0.02)	(0.01)
N	1129	568	842	859
Outcome: Child not in school				
coef.	-0.07***	0.02	-0.09***	-0.02
(se)	(0.03)	(0.03)	(0.03)	(0.03)
N	1129	568	842	859
Outcome: Child sick				
coef.	0.02	0.00	0.06*	-0.01
(se)	(0.03)	(0.03)	(0.04)	(0.03)
N	1046	750	874	1076
Outcome: Underage marriage				
coef.	0.01	0.02	-0.01	0.01
(se)	(0.02)	(0.05)	(0.02)	(0.03)
N	455	190	279	303

Note: Table contains estimates of τ from Equation (6) in the text. See Table 4 for reading.

These results overall suggest that the program is generally effective in alleviating the primary poverty outcomes it aims to address. While the program's effectiveness exhibits some heterogeneity without a clear direction, varying by the poverty measurement outcome and the specific marginal group each arm targets, the results regarding child well-being are more definitive. Specifically, we observe substantially better outcomes in this area when the program uses a consumption-based targeting measure. Next, we analyze the underlying reasons for observed heterogeneity in program effectiveness, or the absence thereof.

Table 6: LATE effects across marginal beneficiaries

	Monetary poverty targeting	Food insecurity targeting	Poor nutrition targeting	Multidimensional deprivation targeting
Outcome: Livelihood coping index				
coef.	-0.03	0.00	-0.05	-0.03
(se)	(0.05)	(0.06)	(0.06)	(0.05)
N	1739	1382	1405	1938
Outcome: WASH index				
coef.	-0.04	-0.02	-0.12*	-0.02
(se)	(0.06)	(0.05)	(0.07)	(0.04)
N	1739	1382	1405	1938
Outcome: Shelter quality index				
coef.	-0.08	-0.05	-0.16**	-0.02
(se)	(0.06)	(0.05)	(0.06)	(0.04)
N	1739	1382	1405	1938

Note: Table contains estimates of τ from Equation (6) in the text. See Table 4 for reading.

4.3 Targeting and Program Effect Heterogeneity

Characterizing marginal beneficiaries

We relate the observed program outcomes to the characteristics of households targeted by each strategy. Table 7 presents descriptive statistics on demographics, poverty levels, and well-being measures for marginal beneficiaries in the "control" condition across targeting strategies, with each column testing mean differences relative to those in the monetary poverty column. Three key insights emerge from this analysis. First, households targeted by expenditure or food consumption are relatively similar to each other, with few small differences from marginal beneficiaries in the monetary poverty targeting arm – and when differences are statistically significant, they are often of a small economic magnitude. Second, geographic targeting, through the use of these poverty indicators, prioritizes starkly different demographic groups compared to the other vulnerability-based targeting strategies. Marginal beneficiaries in consumption-based targeting groups have higher ability to borrow and have higher baseline assets. Despite higher nominal consumption, beneficiaries in the food insecurity and multidimensional deprivation groups have lower baseline ability to smooth consumption due to smaller household sizes, lower assets, greater financial market exclusion, and limited social support. They are less likely to have close friends, feel reliant on social connections for credit, or perceive their community as supportive and cohesive compared to the beneficiaries in consumption-based targeting groups. Third, there appears to be no clear connection between the demographic background, baseline economic constraints, and the effectiveness of the program. For instance, while beneficiaries of multidimensional targeting experience significant improvements in all deprivation measures, similar profiles targeted by food insecurity see little or no effect from the program. The differences in program effects, despite comparable beneficiary profiles, also applies to consumption-based measures, underscoring the unpredictability of program impact heterogeneity by the demographic and economic profiles of targeted groups.

Table 7: Control means for marginal beneficiaries by targeting arm

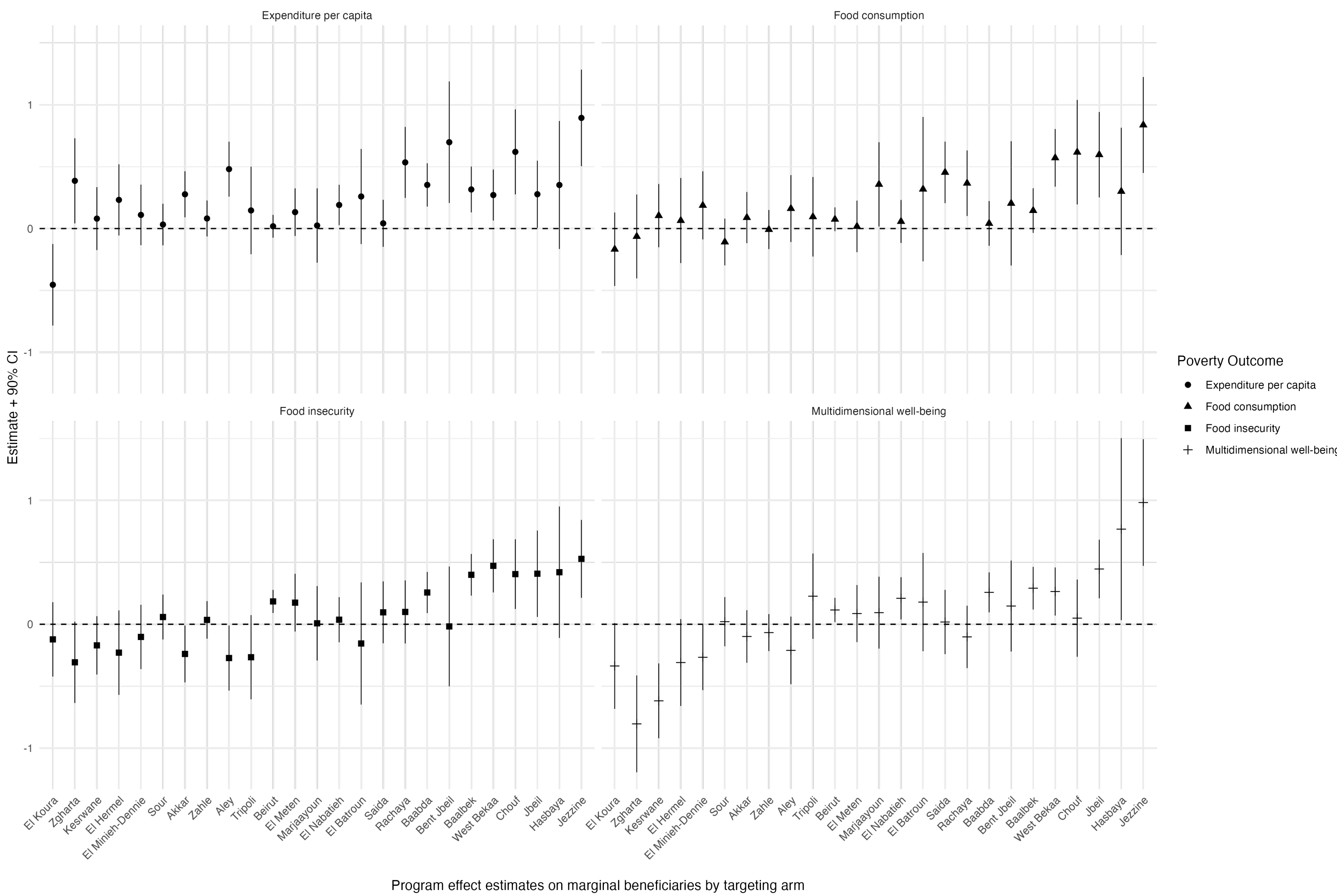
Measure	Monetary poverty	Food insecurity	Food consumption	Multidimensional deprivation
Demographics				
Household size	4.897	3.707***	4.797	3.645***
Share HH age 0-5	0.212	0.155***	0.192*	0.160***
Share HH age 50+	0.084	0.091	0.085	0.095
Share of nondisabled working-age males	0.127	0.224***	0.141**	0.246***
Female-headed household	0.284	0.158***	0.266	0.150***
Disability in household	0.159	0.154	0.156	0.109***
Share with no education	0.145	0.074***	0.125	0.081***
Share with high school education or above	0.304	0.327	0.317	0.331
Targeting score (predicted exp. per cap.)	2.500	3.128***	2.582***	3.233***
Well-being measures				
Livelihood coping strategies index (z-score)	5.338	5.216	5.353	5.369
WASH index (z-score)	-0.083	0.025***	-0.017***	0.006***
Shelter condition index (z-score)	0.010	-0.042*	0.004	0.038
Rental debt (MM LBP)	1.081	1.238*	1.118	1.186
Durable goods index	0.014	-0.027***	0.016	-0.008
Productive assets index	0.075	-0.016***	0.045	-0.016***
Social cohesion				
Has close friends	0.857	0.782***	0.843	0.828
Neighbors could care for children	0.628	0.645	0.601	0.604
Could borrow from social circle	0.804	0.751***	0.800	0.795
Willing to assist others	0.102	0.093	0.086	0.103
Community is supportive	0.602	0.533***	0.552**	0.486***
Community helps in emergency	0.685	0.597***	0.616***	0.540***

Note: Table contains means of control group marginal beneficiaries under each targeting strategy. **Reading:** Households marginally prioritized by targeting monetary poverty have 4.89 people, on average. Households marginally prioritized by food insecurity targeting have 3.7 members on average, and the difference between this mean and that in the monetary poverty column is statistically significant at the 1% level.

4.3.1 2SLS effects by location

Next, we pool all marginal beneficiaries and estimate a program effect for each locality in the country independent of how the beneficiaries are targeted. Figure 4 shows the distribution of the estimated program effects on our main poverty indicators. Effects are plotted based on the rank of the average effect magnitude in each outcome panel, and shapes indicate the outcome of interest. Variation in location-specific heterogeneity is substantial, ranging from a few negative point estimates to 0.5 SD treatment effects or larger. Importantly, the same subset of districts tend to drive the overall improvements in well-being across all outcomes. Effects in the upper tail are large enough to statistically reject zero, and there is a substantial positive correlation between effects across outcomes. That is, the districts with the largest improvements tend to overlap across all outcomes that we investigate; the median Pearson correlation coefficient of effect sizes by district across outcomes is .58, with a range from .33 to .75. These results suggest location-specific factors are important predictors of the program outcomes, independent of how the beneficiaries are selected. They also suggest that targeting effectiveness relates to the local market conditions, which might or might not be observed by the program designers. In the next section, we quantify the relative contribution of these location-specific factors and benchmark them against the demographic information and poverty assessments that are often available to program planners.

Figure 4: Local average treatment effect estimates by district



Note: Figure depicts local average treatment effects for each district sample and across four primary outcomes. Districts are ordered according to average coefficient size.

4.3.2 Decomposition of treatment effect heterogeneity

The experimental design allows us to estimate a treatment effect for each marginal beneficiary type-by-district cell, of which there are more than 150 separate samples. Using 2SLS program effect estimates from these samples, we decompose the overall variation in treatment effects into components explained by the poverty target, location-specific effects, the predicted expenditure per capita from the PMT, and basic demographic information using a random forest classifier, which allows us to quantify the relative importance of predictors of treatment effects while protecting against overfitting. Figure 5 contains the relative importance of each treatment heterogeneity predictor in our model, calculated by the how much the model's accuracy decreases when the information provided by the indicated variable is not available to the researcher. For example, the estimated program effect's location accounts for 29-34% of the total improvement in node purity across all trees in the forest, compared to all other variables. The type of poverty targeted by the program contributes to 9-11% of the total improvement. Medical conditions, disabilities, and the share of retired/elderly are significant predictors of the program's effects, whereas the remaining demographic information has uniformly low predictive power on treatment heterogeneity. These results once again underscore the importance of location-specific factors in explaining the heterogeneity of program effects net of all other features of program design and operation, including the type of poverty targeted by the program, demographics of the beneficiaries, or their starting poverty level.[10]

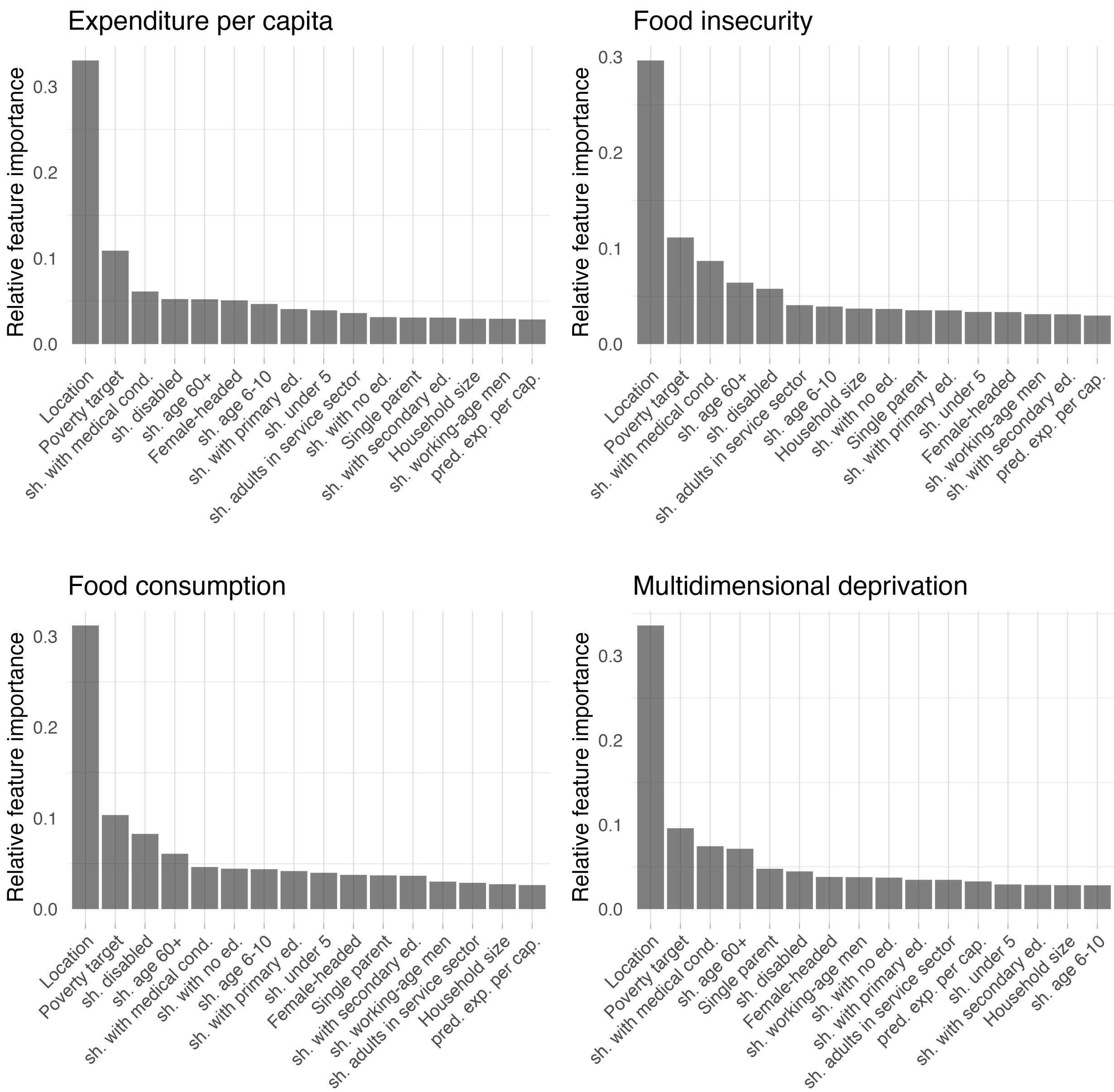
4.4 Program Satisfaction

Even if a given targeting strategy could yield aggregate improvements in effectiveness, program implementers must consider the potential for higher overall costs if one method results in greater aggregate dissatisfaction. In Table 8, we present an empirical analysis of satisfaction, perceived fairness, and accuracy among households subjected to varying targeting strategies. The first four columns of the table display the proportion of respondents who expressed individual dissatisfaction, community dissatisfaction, perceived the targeting approach as unfair, or found targeting inaccurate. Notably, over 50% of the refugee population expressed dissatisfaction with the targeting system, and approximately 40% perceived it as unfair and inaccurate.[11] In a joint hypothesis test that all means are equal to that of the monetary poverty targeting arm, we find no systematic differences in perceptions across populations in different treatment arms. The final column considers grievance redress claim rates in the entire population, which fluctuate between 25% and 27%. Although

[10] In the Appendix Table 7, we include an ANOVA variance decomposition exercise, demonstrating that location accounts for a substantial portion of the variation in treatment effects, specifically 28-36%, across our poverty outcomes using estimates from marginal beneficiary type-by-district cell.

[11] There are significant differences in program satisfaction responses based on beneficiary status, resulting in individuals being three to four times more likely to report dissatisfaction with the targeting strategy if assigned to a less generous targeting arm.

Figure 5: Predictors of treatment effects across experimental subsamples



Note: Figure depicts predictor importance from random forest classifier applied to a vector of treatment effect estimates.

we can differentiate these rates statistically across arms, the largest gap between two arms does not exceed two percentage points. This margin is not economically significant for programming purposes, leading us to conclude that there are no pronounced differences in household perceptions with respect to the study arms.

Table 8: Program satisfaction and grievance claimancy across targeting arms

Targeting arm	Is dissatisfied	Community is dissatisfied	Selection is unfair	Selection is inaccurate	Filed claim
Monetary poverty	0.55	0.52	0.43	0.4	0.25
Food insecurity	0.52	0.52	0.36	0.38	0.26
Food consumption	0.51	0.54	0.4	0.37	0.25
Multidimensional depriv.	0.52	0.51	0.42	0.35	0.27
F-stat., all means equal	0.43	0.31	1.84	0.62	32.96
p-value	0.73	0.82	0.14	0.61	<0.01
N	1,899	1,899	1,899	1,899	$\approx 300,000$

Note: Table contains means satisfaction survey outcomes and results of a joint hypothesis test that all arms equal the value in the monetary poverty targeting row. Indicators constructed from either of two negative response values from four-point Likert scale questions. **Reading:** 55 percent of households in the monetary poverty targeting arm report being either very dissatisfied or somewhat dissatisfied with the assistance programs. 52 percent of households in the food insecurity targeting arm respond similarly.

5 Discussion

In an experimental setting, we demonstrate that reallocating cash transfers across locations based on varying vulnerability measures effectively prioritizes populations with distinct demographic and socioeconomic backgrounds. However, our findings reveal that neither the targeting criteria nor the beneficiary background characteristics exhibit a strong association with the variation in program effects. Additionally, our analysis of heterogeneity in program effects reveals that targeting choices have relatively minor potential to increase program effectiveness when compared to the pronounced differences in program outcomes across various locations. We then compare the variation in program effects attributable to the poverty target, location, baseline poverty level, and demographics. Location emerges as the strongest determinant of treatment effect heterogeneity, shaping program outcomes more than any other factor by a wide margin. Altering the program's target population therefore has a limited impact on its effectiveness compared to the influence of the economic environment where the program is implemented. These findings underscore those in the broader program evaluation literature, particularly concerning the role of site-specific factors in program effect heterogeneity (Allcott, 2015), context dependence (Pritchett and Sandefur, 2015), and the limited external validity of experimental estimates across different program sites or scales (Banerjee et al., 2017; List, 2022).

The main implication of these results for the design of cash transfer programs is that while program objectives can be wide-ranging, effectiveness largely does not depend on the type of beneficiary to which the program is targeted or the outcome measure it aims to improve. The local market environment is the most important feature influencing how beneficiaries use unconditional cash to enhance well-being. Therefore, knowledge of local context is crucial in understanding and possibly predicting program effectiveness. For instance, our focus group discussions revealed that the improvement in school enrollment among the beneficiaries in consumption-based targeting groups is likely related to the parents' ability to afford commuting to school. The study period coincided with a notable shift in the Lebanese government's policy, leading to a discontinuation of energy subsidies, which in turn resulted in a steep rise in petrol prices. This change significantly affected many poor families in remote areas who were unable to send their children to school due to increased transportation costs. The cash transfers were crucial for these families in relaxing liquidity constraints that are both location and period-specific. In contrast, parents in urban areas faced congestion in schools as well as changing rules about children's legal documentation which prevented school enrollment and attendance, which were not possible to address with cash transfers. From another angle, marginal beneficiaries supported by targeting food insecurity were found to have the highest existing debt levels, as well as a high prevalence of costly medical conditions which were not noted from the discussions with other groups. These existing conditions constrained the use of transfers in alleviating food insecurity as recipients used a higher share of their payments to reduce their debt burden. Without such detailed information on local environment, our results suggest that the potential for enhancing program effectiveness by changing the poverty target is likely limited.

Appendix Table 1: ITT effects across targeting comparisons and beneficiary marginality

	Full sample	Inframarginal beneficiaries	Marginal beneficiaries
Outcome: Expenditure per capita			
Comparison: Monetary poverty vs. food insecurity			
coef.	-0.01	0.00	-0.04
(se)	(0.03)	(0.04)	(0.05)
N	3710	2318	1392
Comparison: Monetary poverty vs. food consumption			
coef.	0.01	0.00	0.08
(se)	(0.03)	(0.03)	(0.07)
N	3628	2954	674
Comparison: Monetary poverty vs. multidimensional deprivation			
coef.	0.02	-0.01	0.08
(se)	(0.03)	(0.04)	(0.05)
N	3625	2265	1360
Outcome: Coping strategies index			
Comparison: Monetary poverty vs. food insecurity			
coef.	-0.02	0.00	-0.06
(se)	(0.03)	(0.04)	(0.05)
N	3800	2369	1431
Comparison: Monetary poverty vs. food consumption			
coef.	-0.05	-0.05	-0.06
(se)	(0.03)	(0.04)	(0.08)
N	3706	3020	686
Comparison: Monetary poverty vs. multidimensional deprivation			
coef.	-0.02	-0.06	0.05
(se)	(0.03)	(0.04)	(0.05)
N	3715	2324	1391
Outcome: Food consumption score			
Comparison: Monetary poverty vs. food insecurity			
coef.	-0.01	-0.03	0.03
(se)	(0.03)	(0.04)	(0.05)
N	3739	2334	1405
Comparison: Monetary poverty vs. food consumption			
coef.	0.02	0.03	0
(se)	(0.03)	(0.04)	(0.07)
N	3623	2956	667
Comparison: Monetary poverty vs. multidimensional deprivation			
coef.	0.02	0.02	0.02
(se)	(0.03)	(0.04)	(0.05)
N	3651	2282	1369
Outcome: Multidimensional deprivation			
Comparison: Monetary poverty vs. food insecurity			
coef.	-0.02	0.01	-0.07
(se)	(0.03)	(0.04)	(0.06)
N	3629	2253	1376
Comparison: Monetary poverty vs. food consumption			
coef.	-0.04	-0.06	0.07
(se)	(0.03)	(0.04)	(0.08)
N	3556	2880	676
Comparison: Monetary poverty vs. multidimensional deprivation			
coef.	0.02	-0.01	0.07
(se)	(0.03)	(0.04)	(0.05)
N	3538	2210	1328

Note: Table contains estimates of effects from pairwise targeting arm comparison. Panel labels indicate outcome set.

Appendix Table 2: ITT effects across targeting comparisons and beneficiary marginality

	Full sample	Inframarginal beneficiaries	Marginal beneficiaries
Outcome: Child working			
Comparison: Monetary poverty vs. food insecurity			
coef.	0.00	-0.02	0.04**
(se)	(0.01)	(0.01)	(0.02)
N	2127	1368	759
Comparison: Monetary poverty vs. food consumption			
coef.	0.00	0	0.01
(se)	(0.01)	(0.01)	(0.02)
N	2058	1667	391
Comparison: Monetary poverty vs. multidimensional deprivation			
coef.	0.02*	0.02	0.03
(se)	(0.01)	(0.02)	(0.02)
N	2089	1343	746
Outcome: Child not in school			
Comparison: Monetary poverty vs. food insecurity			
coef.	0.02	-0.02	0.09***
(se)	(0.02)	(0.02)	(0.03)
N	2127	1368	759
Comparison: Monetary poverty vs. food consumption			
coef.	0.00	0	0.02
(se)	(0.02)	(0.02)	(0.04)
N	2058	1667	391
Comparison: Monetary poverty vs. multidimensional deprivation			
coef.	0.01	0.01	0.01
(se)	(0.02)	(0.02)	(0.03)
N	2089	1343	746
Outcome: Child sick			
Comparison: Monetary poverty vs. food insecurity			
coef.	-0.02	-0.03	-0.01
(se)	(0.02)	(0.02)	(0.03)
N	2196	1383	813
Comparison: Monetary poverty vs. food consumption			
coef.	0.00	-0.02	0.07
(se)	(0.02)	(0.02)	(0.04)
N	2169	1779	390
Comparison: Monetary poverty vs. multidimensional deprivation			
coef.	0.00	0.01	-0.01
(se)	(0.02)	(0.02)	(0.03)
N	2115	1309	806
Outcome: Underage marriage			
Comparison: Monetary poverty vs. food insecurity			
coef.	0.03*	0.03	0.03
(se)	(0.02)	(0.02)	(0.03)
N	827	541	286
Comparison: Monetary poverty vs. food consumption			
coef.	0.00	0.01	-0.02
(se)	(0.02)	(0.02)	(0.03)
N	808	660	148
Comparison: Monetary poverty vs. multidimensional deprivation			
coef.	-0.01	-0.02	0.01
(se)	(0.02)	(0.02)	(0.02)
N	807	536	271

Note: Table contains estimates of effects from pairwise targeting arm comparison. Panel labels indicate outcome set.

Appendix Table 3: ITT effects across targeting comparisons and beneficiary marginality

	Full sample	Inframarginal beneficiaries	Marginal beneficiaries
Outcome: Livelihood coping index			
Comparison: Monetary poverty vs. food insecurity			
coef.	-0.02	-0.03	-0.01
(se)	(0.03)	(0.04)	(0.05)
N	3775	2351	1424
Comparison: Monetary poverty vs. food consumption			
coef.	0.00	0.01	-0.06
(se)	(0.03)	(0.04)	(0.08)
N	3689	3006	683
Comparison: Monetary poverty vs. multidimensional deprivation			
coef.	0.02	0.05	-0.03
(se)	(0.03)	(0.04)	(0.06)
N	3697	2313	1384
Outcome: WASH index			
Comparison: Monetary poverty vs. food insecurity			
coef.	-0.03	-0.02	-0.04
(se)	(0.03)	(0.04)	(0.05)
N	3768	2353	1415
Comparison: Monetary poverty vs. food consumption			
coef.	-0.02	-0.04	0.07
(se)	(0.03)	(0.04)	(0.07)
N	3676	2992	684
Comparison: Monetary poverty vs. multidimensional deprivation			
coef.	0.02	0.02	0.02
(se)	(0.03)	(0.04)	(0.05)
N	3690	2310	1380
Outcome: Shelter quality index			
Comparison: Monetary poverty vs. food insecurity			
coef.	-0.02	-0.02	-0.01
(se)	(0.03)	(0.04)	(0.05)
N	3800	2369	1431
Comparison: Monetary poverty vs. food consumption			
coef.	-0.01	-0.02	0.04
(se)	(0.03)	(0.04)	(0.08)
N	3706	3020	686
Comparison: Monetary poverty vs. multidimensional deprivation			
coef.	0.00	0.01	-0.02
(se)	(0.03)	(0.04)	(0.05)
N	3715	2324	1391

Note: Table contains estimates of effects from pairwise targeting arm comparison. Panel labels indicate outcome set.

Appendix Table 4: First stage among marginal beneficiaries

	Monetary poverty targeting	Food insecurity targeting	Poor nutrition targeting	Multidimensional deprivation targeting
Outcome: Assistance received in MM LBP				
coef.	0.86***	0.99***	0.84***	1.02***
(se)	(0.02)	(0.02)	(0.03)	(0.02)
N	1739	1382	1405	1938
F-stat.	2032	1861	1389	2056

Note: Table contains first-stage estimates of the effect of randomized assignment into a higher-benefit targeting arm among marginal beneficiaries.

Appendix Table 5: Pre-registered specification estimates (ITT)

Outcome	Food insecurity targeting	Poor nutrition targeting	Multidimensional deprivation targeting
Domain: Poverty measures			
Expenditure per capita	-0.021	-0.002	0.002
	(0.02)	(0.021)	(0.02)
Coping strategies index	-0.25	-0.683	-0.283
	(0.445)	(0.458)	(0.448)
Food consumption score	-0.086	0.406	0.339
	(0.528)	(0.543)	(0.532)
Multidimensional deprivation	-0.001	-0.003	0.004
	(0.005)	(0.005)	(0.005)
Domain: Child well-being			
Child working	0.002	0.005	0.022
	(0.011)	(0.011)	(0.012)
Child not in school	0.015	0.004	0.009
	(0.018)	(0.018)	(0.018)
Child sick	-0.024	-0.001	0.003
	(0.018)	(0.019)	(0.019)
Underage marriage	0.034	0.004	-0.009
	(0.018)	(0.016)	(0.016)
Domain: Living conditions			
Livelihood coping index	-0.027	-0.015	0.058
	(0.104)	(0.104)	(0.104)
WASH index	-0.011	-0.007	0.015
	(0.014)	(0.015)	(0.014)
Shelter quality index	-0.012	-0.008	0.00
	(0.021)	(0.022)	(0.022)
Domain: Property rights			
Rental debt stock in 000s	0.665	-67.103	-18.197
	(60.041)	(59.38)	(60.302)
Benefits card ever used as collateral	0.009	0.007	0.002
	(0.008)	(0.008)	(0.008)
Benefits card currently with lender	0.003	0.001	-0.001
	(0.003)	(0.002)	(0.002)
Domain: Social support and networks			
Has any close friends	-0.007	0.008	-0.014
	(0.013)	(0.012)	(0.013)
Neighbors could care for children	-0.025	-0.014	-0.009
	(0.016)	(0.016)	(0.016)
Can borrow from social circle	-0.029	-0.012	-0.018
	(0.013)	(0.013)	(0.013)
Have been asked to assist financially	0.00	0.003	0.006
	(0.01)	(0.01)	(0.01)
Lives in a supportive community	0.00	-0.011	-0.011
	(0.016)	(0.016)	(0.016)
Community support for household emergencies	0.001	0.011	-0.008
	(0.016)	(0.016)	(0.016)
Domain: Productive assets			
Consumer durable assets index	-0.02	-0.007	-0.002
	(0.011)	(0.011)	(0.011)
Productive assets index	-0.003	0.013	0.023
	(0.012)	(0.013)	(0.013)
Domain: Savings			
Has no savings	0.00	-0.001	-0.002
	(0.003)	(0.003)	(0.003)
Had to spend savings to cope	-0.009	0.00	-0.001
	(0.015)	(0.015)	(0.015)

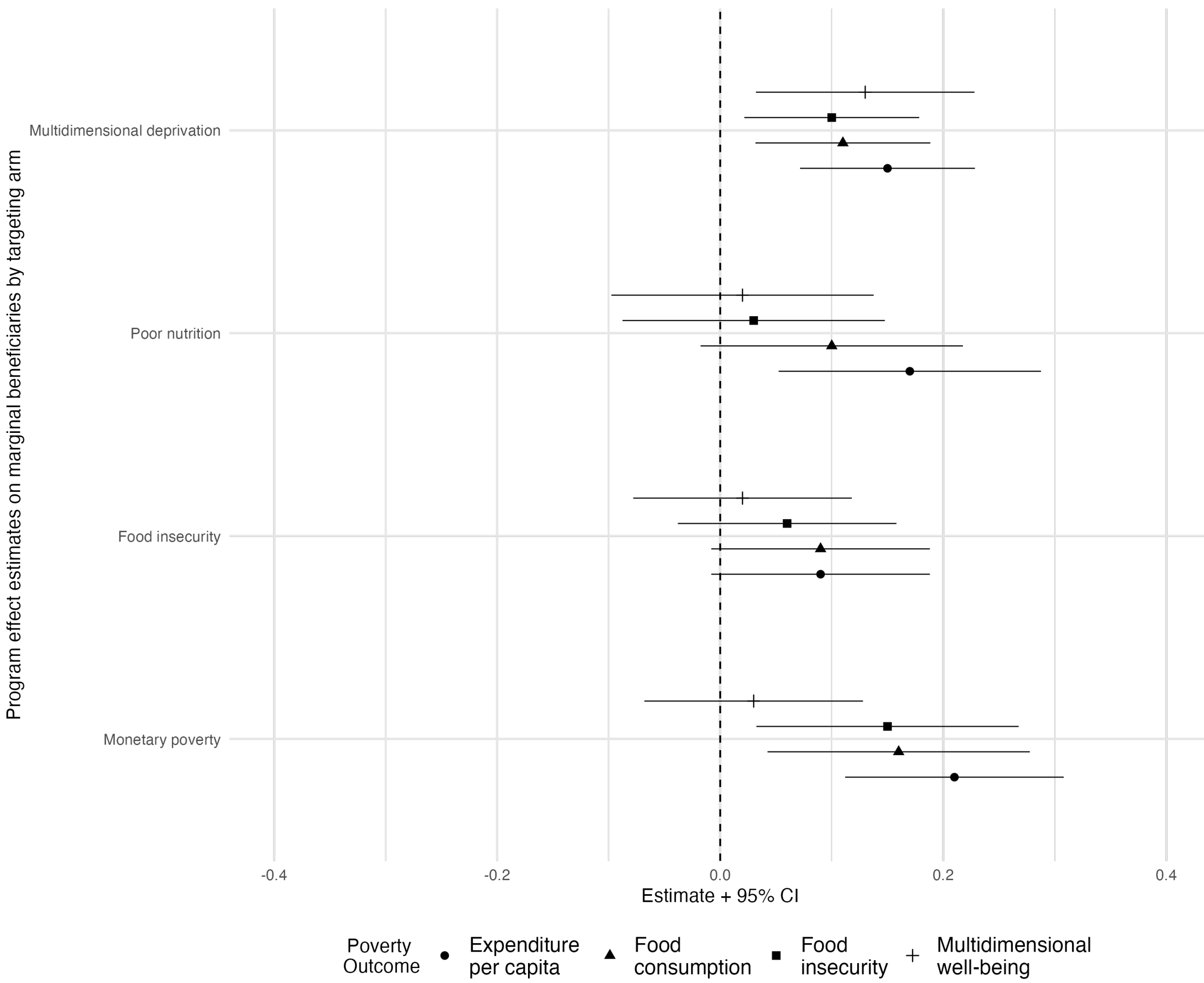
Note: Table contains estimates of the τ from Equation (1) of the pre-registered analysis plan. Standard errors in parentheses. P-values corrected for multiple hypothesis testing within domain. *q < .10; **q < .05; ***q < .01

Appendix Table 6: Pre-registered specification estimates (LATE)

Outcome	Monetary poverty targeting	Food insecurity targeting	Poor nutrition targeting	Multidimensional deprivation targeting
Domain: Poverty measures				
Expenditure per capita	0.207***	0.09	0.171**	0.149***
	(0.048)	(0.054)	(0.058)	(0.039)
Coping strategies index	0.147**	0.062	0.03	0.104***
	(0.057)	(0.055)	(0.064)	(0.043)
Food consumption score	0.158***	0.093	0.1	0.109***
	(0.057)	(0.055)	(0.064)	(0.045)
Multidimensional deprivation	0.029	0.019	0.024	0.13***
	(0.054)	(0.054)	(0.062)	(0.045)
Domain: Child well-being				
Child working	-0.021	0.05*	-0.023	0.02
	(0.017)	(0.02)	(0.018)	(0.014)
Child not in school	-0.074**	0.023	-0.09**	-0.018
	(0.026)	(0.034)	(0.031)	(0.027)
Child sick	0.015	0.001	0.061	-0.01
	(0.032)	(0.029)	(0.036)	(0.026)
Underage marriage	0.005	0.022	-0.008	0.013
	(0.017)	(0.048)	(0.02)	(0.027)
Domain: Living conditions				
Livelihood coping index	-0.03	0.00	-0.053	-0.028
	(0.055)	(0.057)	(0.062)	(0.047)
WASH index	-0.038	-0.022	-0.116*	-0.022
	(0.057)	(0.054)	(0.065)	(0.044)
Shelter quality index	-0.078	-0.052	-0.155**	-0.021
	(0.056)	(0.051)	(0.061)	(0.043)
Domain: Property rights				
Rental debt stock in 000s	-227.618**	-194.618	-173.065	-49.738
	(86.627)	(106.815)	(107.519)	(90.956)
Benefits card ever used as collateral	-0.023	0.013	-0.018	0.001
	(0.016)	(0.013)	(0.014)	(0.009)
Benefits card currently with lender	-0.002	-0.002	-0.003	-0.006
	(0.004)	(0.003)	(0.005)	(0.003)
Domain: Social support and networks				
Has any close friends	0.004	-0.023	-0.004	-0.023
	(0.02)	(0.023)	(0.023)	(0.017)
Neighbors could care for children	-0.011	-0.014	-0.008	0.02
	(0.027)	(0.026)	(0.031)	(0.021)
Can borrow from social circle	0.044	-0.03	-0.037	0.005
	(0.021)	(0.024)	(0.026)	(0.018)
Have been asked to assist financially	0.005	-0.01	0.014	0.037*
	(0.017)	(0.016)	(0.019)	(0.014)
Lives in a supportive community	0.012	-0.009	-0.039	0.031
	(0.027)	(0.027)	(0.032)	(0.022)
Community support for household emergencies	-0.013	-0.017	0.031	0.04
	(0.026)	(0.027)	(0.031)	(0.022)
Domain: Productive assets				
Consumer durable assets index	0.012	-0.061	0.022	0.068
	(0.056)	(0.055)	(0.064)	(0.047)
Productive assets index	-0.096	-0.025	0.003	-0.007
	(0.049)	(0.055)	(0.058)	(0.044)
Domain: Savings				
Has no savings	0.00	0.003	-0.001	0.006
	(0.005)	(0.004)	(0.004)	(0.003)
Had to spend savings to cope	0.027	0.029	0.028	0.016
	(0.026)	(0.025)	(0.03)	(0.02)

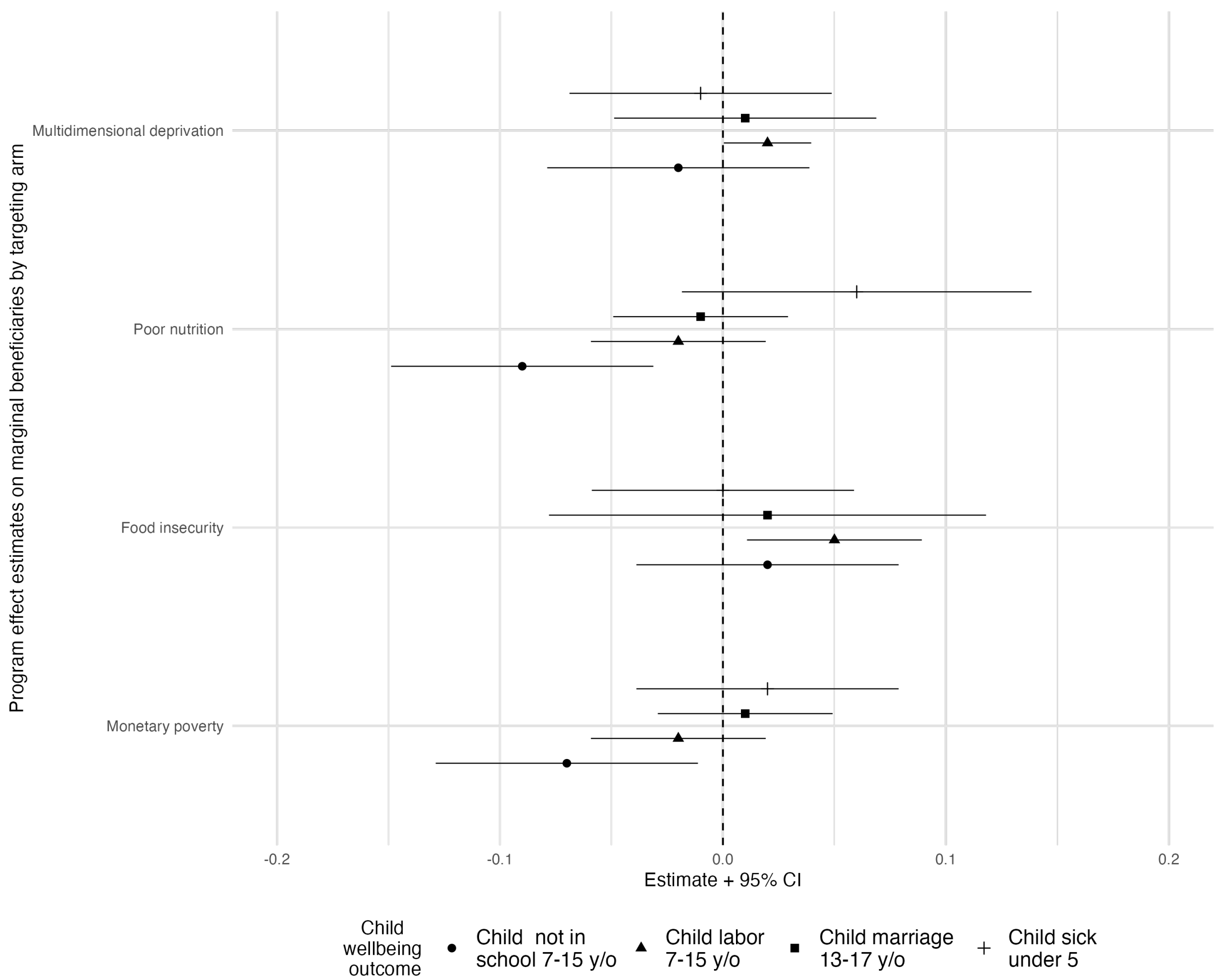
Note: Table contains estimates of the β_j from Equation (3) of the pre-registered analysis plan. Standard errors in parentheses. P-values corrected for multiple hypothesis testing within domain. $^{*}q < .10$; $^{**}q < .05$; $^{***}q < .01$

Appendix Figure 1: Program LATE estimates by marginal subsample, main outcomes



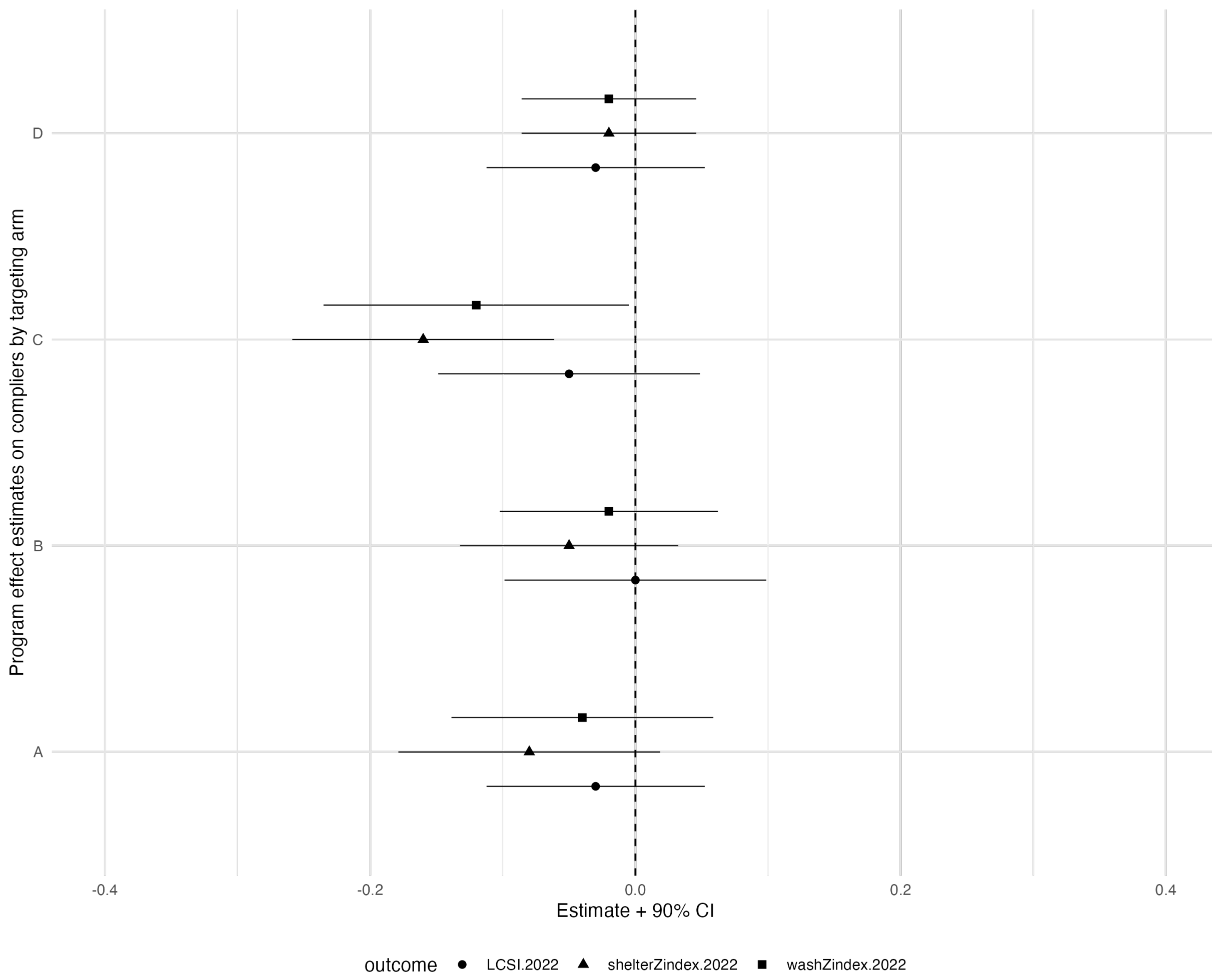
Note: Figure depicts LATE effects for each sample and across four primary outcomes. The groups on the vertical axis indicate the targeting arm to which the sample households are marginal beneficiaries. Coefficients are in units of standardized outcome z-score. **Reading:** Households marginal to the multidimensional deprivation arm are positively impacted in all four outcome measures when receiving a higher transfer as a result of being assigned to that arm. Households marginal to food consumption targeting increase expenditures, with effects on other outcomes remaining statistically indistinguishable from zero.

Appendix Figure 2: Program LATE estimates by marginal subsample, children's outcomes



Note: Figure depicts LATE effects for each sample and across four children's outcomes. The groups on the vertical axis indicate the targeting arm to which the sample households are marginal beneficiaries. Coefficients are in units of standardized outcome z-score.

Appendix Figure 3: Program LATE estimates by marginal subsample, secondary outcomes



Note: Figure depicts LATE effects for each sample and across four children's outcomes. The groups on the vertical axis indicate the targeting arm to which the sample households are marginal beneficiaries. Coefficients are in units of standardized outcome z-score.

Appendix Table 7: Treatment effect variance decomposition

Outcome measure	Location	Household demographics	Poverty target	Baseline expenditure	Residual
Expenditure per capita	0.34	0.12	0.05	0.03	0.45
Food security	0.28	0.10	0.07	0.00	0.56
Food consumption	0.35	0.21	0.05	0.02	0.38
Multidimensional well-being	0.36	0.09	0.06	0.00	0.49

Notes: Table presents partial R^2 from ANOVA analysis of treatment effect estimates across district-by-complier set cells. The poverty target set contains three indicators, location contains 25 district fixed effects, baseline expenditure is a single measure of the control group mean expenditure per capita, and household demographics include a vector of the control group means of 14 background characteristics related to demographics, dependency, migration history, protection measures, headship, disability, and education.

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