Geographic poverty targeting in social protection programs: Evidence from a nationwide policy experiment

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Abstract

We study how prioritization rules for a large-scale cash transfer program affect who is targeted, program effectiveness, and beneficiary satisfaction. In a nationwide policy experiment among Syrian refugees in Lebanon, we randomized households into different area-level allocation rules based on monetary poverty, food insecurity, food consumption, and multidimensional deprivation. We find that these rules select demographically distinct groups, but generate little difference in program impacts on key welfare outcomes. In contrast, there is substantial geographic heterogeneity in program effectiveness across districts, suggesting that local conditions, rather than targeting rules, are the primary drivers of variation in outcomes. Administrative data and household characteristics explain little of this location-based heterogeneity. Qualitative interviews highlight market-specific constraints, such as transport costs and debt burdens, that mediate program impacts. Our findings suggest that that the the poverty target used for budget allocation are crucial in determining who benefits from the transfers but may be less important for determining overall program effectiveness in a given locality.

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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Beneficiary targeting is a core component of social protection programs, which often face the dual goals of reaching those most in need while maximizing the impact of expended resources. Antipoverty programs in developing countries make use of various targeting methods including proxy-means testing, community input, self-targeting, and geographic targeting rules. These methods, whether used alone or in combination, aim to balance administrative costs with the accurate targeting of beneficiaries (Coady et al., 2004; Banerjee et al., 2022). Unconditional cash transfers (UCTs) are among the most popular social welfare programs worldwide, with well-established impacts on household consumption, income, labor supply, school enrollment, food security, and psychological well-being (Crosta et al., 2024). When implemented at scale, UCTs typically rely on proxy-means tests (PMTs) alongside an initial allocation of the national budget to local administrative units. Despite the extensive literature on UTCs, we know relatively little about how policymakers' decisions on where and who to target program resources affect program outcomes. Understanding this link important, as local market conditions and household-level constraints shape how transfers can be used.

In this paper, we examine how distributing a fixed social assistance budget across different localities using alternative allocation rules changes the pool of beneficiaries, overall program effectiveness, and beneficiary satisfaction. We study a year-long, nationwide randomized policy experiment initiated in mid-2021 by humanitarian agencies in Lebanon that targeted a high-value unconditional cash transfer (UCT) program to over 1.5 million Syrian refugees in the country. The goal of the policy experiment was to understand how using different aggregate poverty metrics to allocate district-level budgets affects program outcomes holding constant the total budget and the share of households supported. Historically, the program used a two-tiered system to target transfers: first, the total budget was geographically allocated across 26 administrative districts according to an area-level poverty metric; then, within each district, a proxy-means test (PMT) based on per capita expenditure identified the poorest households to receive the cash transfer. During the study year, implementing agencies divided the national budget into four equal parts, each distributing the same total amount of transfers but allocating funds differently across 26 districts in the first step. Each allocation rule used one of four poverty metrics — monetary poverty, food insecurity, nutritional deficiency, or multidimensional deprivation — to determine district-level program budgets. These metrics were chosen because they reflect the specific dimensions of poverty that the implementing agencies considered prioritizing in their efforts to support the refugee population. Each district was assigned four separate budgets corresponding to the four targeting strategies (monetary poverty, food consumption, food insecurity, multidimensional deprivation), based on the fraction of households below each corresponding threshold. Households within each district were randomized into one of these four arms. Within each arm, households were ranked according to a proxy means test (PMT) and enrolled in order of need until that arm's budget was exhausted. As a result, each household's

eligibility and assistance amount could vary based on the random allocation to a specific arm.

We simulated the application of each of the four allocation rules across all households *ex ante*, allowing us to directly observe four counterfactual transfer amounts at the household level. From this, we identify households for whom the budget allocation 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 poverty metric used for district-level budget allocation does not change the transfer amounts 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 allocation arms. These households are precisely those among whom the program reallocates resources. The experimental design enables the direct estimation of local average treatment effects (LATE) among marginal beneficiaries, whose assistance amounts depend on the generosity of the district-level budget to which they are randomized. In line with our pre-registered analysis plan, marginal beneficiaries were over sampled and surveyed specifically for this study to power pre-specified effect sizes. As one objective is to examine changes in program outcomes induced by reshuffling resources across districts, these households represent the policy-relevant population in such settings.

We have three findings. First, there are marked differences in the demographics and market access of marginal beneficiaries who are selected by different budget allocation rules, particularly in terms of household composition, access to informal insurance, types of poverty experienced, and the ability to smooth consumption. Second, we find that the program alleviates economic deprivation for all marginal beneficiary groups, with effect sizes ranging from a statistically insignificant 0.02 SD to 0.21 SD across pre-specified poverty outcomes and the marginal beneficiary population. Despite having a sample large enough to detect even small effect sizes, we do not find statistically significant differences in program effects across households that were targeted by alternative budget allocation rules. These results suggest that the type of beneficiary the program reaches in a given local market is unlikely to improve overall poverty alleviation through heterogeneous treatment effects. Third, in sharp contrast, we show substantial variation in program effects across districts, independent of the targeted beneficiaries' characteristics. This suggests that local conditions, rather than the beneficiary profile, play a critical role in determining the effectiveness of the UCT program. In line with these findings, we show that detailed household-level data typically available to policymakers perform poorly in predicting where the program will be more effective as district-specific effects explain a large share of the variation in program impacts even after accounting for household characteristics and other observable factors. Insights from twelve focus group discussions involving 114 marginal beneficiaries suggest that variation in program outcomes is mainly driven by market-specific shocks faced by households. In sum, our study shows that, for large-scale cash transfer programs, budget

allocation rules are critical for targeting intended beneficiary profiles but alternate rules do not generate substantial variation in program effectiveness. In our setting, prioritizing resources to populations with the highest program effectiveness would have required extensive knowledge of both current and future market conditions across program sites.

1 Literature and Contribution

A large literature on the targeting of social assistance focuses 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. (2022). These studies either compare the efficiency of different targeting approaches or quantify the overall program effects of a specific program design. We contribute to existing research by showing that initial budget allocations across localities—a common practice in large-scale national transfer programs using area-level poverty mapping[1] — significantly change the socio-demographic composition of beneficiaries. However, we observe only limited variation in program effectiveness among those whose cash transfer amounts are affected by these allocations, suggesting that the policy parameters—such as the poverty measure used for budget allocation—are crucial in determining who benefits from the transfers but may be less important for determining overall program effectiveness in a given locality.

We also contribute to the study of geographic targeting—the prioritization of resources across localities. Geographic targeting is commonly used in conjunction with proxy means tests (PMTs) and other traditional targeting methods.[2] 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). Therefore, local market conditions may play a crucial role in shaping the effectiveness of cash transfer programs. In line with this, we document substantial variation in program effects across localities for the UCT we study. This raises the question of whether implementers can predict and strategically allocate program resources to areas with better outcomes. Our findings suggest that they generally cannot, at least when relying on standard household-level datasets. This appears to

[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.

[2] Thanks to the growing availability of spatial data and advancements in predictive tools, the use of this approach as a standalone targeting strategy is rapidly expanding in settings with limited data (Elbers et al., 2007; Abelson et al., 2014; Aiken et al., 2022; Asher et al., 2021; Blumenstock et al., 2015; Smythe and Blumenstock, 2022).

be driven, in part, by the fact that program effectiveness is often shaped by local market dynamics and idiosyncratic shocks that are difficult to observe or anticipate ex-ante.

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. We contribute to this body of work by experimentally studying the link between targeting design, beneficiary selection, and program effectiveness in an at-scale humanitarian program.

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. 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 (53 USD), plus 300,000 LBP (20 USD) for each of up to six family members. 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 for each of up to six family members in food voucher credit per

month.[3] 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.[4]

A central issue in implementing a program of this size is the degree of heterogeneity throughout the country in living conditions and economic constraints. For example, households face different price levels and varied opportunities to access food, housing, and services depending on where they live. Our qualitative fieldwork and survey data both 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 resulting from higher capacity state institutions that relate to legal documentation requirements for enrollment. These varying factors highlight the complexities involved in allocating the national program budget across districts, as policymakers must account for the diverse economic conditions in the country.

During the 2021–2022 assistance cycle, humanitarian agencies considered four aggregate level poverty metric to determine district-level budget distribution in their nationwide UCT program. The first one served as a benchmark and used traditional monetary poverty, as measured by expenditure per capita, with a poverty threshold set to an expenditure-based poverty line determined by a group comprised of experts from humanitarian agencies in Lebanon.[5] The second arm used food insecurity 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

[3]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.

[4]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.

[5]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).

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.[6] These measures are frequently used by international organizations, governments, and humanitarian agencies to assess vulnerability and structure social assistance programs. In our setting, these outcomes are actively monitored and hold particular relevance for policy.

3 Data and Empirical Framework

3.1 Data

UNHCR database 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] This process provides refugees with official proof of identity, safeguarding them from forced return or detention while facilitating their enrollment in protection and assistance programs. Consequently, refugees have a strong incentive to be included in administrative records (Altındağ et al., 2021). The UNHCR database is regularly updated through mobile and in-person interactions with refugee families. These data include demographic details, past and current assistance records, and function similarly to a basic social register.

Vulnerability Assessment of Syrian Refugees in Lebanon 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 and can be linked to the UNHCR database using unique household IDs. 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 households per year across Lebanon. VASyR 2021 was conducted in May and June of 2021 and provides pre-intervention outcomes. The same data is

[6] 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.

[7] Individuals and families interested in being enrolled with UNHCR are provided with appointments to collect biographical data and vulnerability information. 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.

also used for training the PMT model of household expenditure per capita as well as district-level poverty indicators used for budget allocation. We use VASyR 2022, collected in June and July of 2022 to measure post-intervention outcomes. Subsequent to our power calculations for this study, the implementing agencies surveyed 2,091 additional households during the same period, randomly drawn from marginal beneficiaries.

Grievance Redress Mechanism Claims The Grievance Redress Mechanism (GRM) is a process by which refugee households can file claims for reconsideration during the initial stages of an assistance cycle. Implementing agencies then collect these claims to 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 should be supported with assistance. Our data encompasses the universe of claimants who submitted a GRM claim during the application period from October 6 to November 19, 2021.

Beneficiary Satisfaction Phone Survey We gathered data on beneficiary perceptions of the cash program using a phone survey. 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.The phone survey is conducted between February 7 and 18, 2022, included a random sample of 1,904 families.

Focus Group Discussions We conducted 12 focus group discussions with 114 study participants 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. The sample was randomly drawn from the beneficiaries for whom we can estimate a local average treatment effect of the program.

3.2 Study Design and Randomization

During the intervention, the program allocated funds to beneficiaries through a two-step process. First, the national program budget was divided into four equal parts. Each part was then distributed at the district level using a different allocation rule. These rules generally followed a simple principle: funds were allocated in proportion to each district's estimated share of the national poor. For example, if a district accounted for 20% of the national poor, it received 20% of the program's overall budget. The distinction between the four allocation rules was the poverty indicator used for distribution—monetary poverty, food insecurity, low food consumption, or multidimensional deprivation. Each district therefore received four different budgets, and *households* within a district were randomly assigned to one of them.

In the second step, each district applied a standard PMT separately for each budget, ranking households from the bottom up based on their predicted per capita expenditure.[8] Funds were then distributed progressively, with the poorest households receiving the highest value assistance package and the less poor receiving a lower tier of assistance, until the allocated district budget was exhausted leaving the least poor out of the program.

Each household had four potential assistance statuses that were observable to us *ex ante*, and their allocation to one arm therefore generated exogenous variation in assistance levels among otherwise identical households located in the same district. By examining the counterfactual assistance amounts for each household, we assess whether the assigned allocation rule changes the transfer value for each household across allocation rules. Households whose assistance amounts would have differed under a different allocation rule are the focus of the subsequent analysis, as they reveal the program trade-offs faced by policymakers. We refer to these households as *marginal* beneficiaries. In contrast, households for whom the district-level budget allocation rule had no impact on assistance levels are referred to as *inframarginal* beneficiaries. While eligibility simulations could be performed using administrative data alone, randomization was essential to causally identify differences in program effectiveness across targeting strategies. Without experimental variation, differences in treatment effects could not be separated from unobserved differences across household groups or districts.

3.3 Counterfactual Framework and Estimation Strategy

Consider a household i with potential outcomes $Y_i(Z)$ and a transfer amount $T_i(Z)$, representing the amount household i would receive if assigned to budget allocation rule Z. Here, Z takes values from the set $Z \in \{1,2,3,4\}$, corresponding to the four budget allocation rules. Household index i ranges over the set $\{1,2,3,...,N\}$, and transfer amount T_i can vary between zero and a positive amount. Since household i is randomly assigned to Z, the exogeneity assumption holds, meaning that potential outcomes and transfer amounts are independent of the assigned allocation rule:

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

For household i assigned to allocation rule $Z \in \{j,k\}$ where $j \neq k$, we can observe the transfer amounts the family would receive under each assignment. Based on this, we can categorize the

[8] See Altındağ et al. (2021) for further details.

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 budget allocation 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, $\tau_{jk|\Delta T=0}$ should be 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, which we can empirically test. 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 due program effect heterogeneity by allocation rule.

The intent-to-treat estimates in equations 2, 3, and 4 quantify the overall differences in economic well-being that result from a policymaker choosing allocation rule j over k, and are obtained using Ordinary Least Squares (OLS).

$$Y_i = \alpha + \tau_{jk} \cdot \mathbf{1}(Z_i = j) + \varepsilon_i \tag{5}$$

where Y_i is the outcome of interest for household i, $\mathbf{1}(Z_i = j)$ is an indicator variable that equals 1 if the household is assigned to allocation rule j and 0 if assigned to k, and ε_i is an error term. The coefficient τ_{jk} captures the ITT effect, representing the mean difference in outcomes between the two allocation rules. To identify τ_{jk}, we restrict the sample to households assigned to either

allocation rule j or k . To estimate $\tau_{jk}|\Delta T = 0$ and $\tau_{jk}|\Delta T \neq 0$, we further restrict the sample to households for which $\Delta T = 0$ and $\Delta T \neq 0$, respectively.

In addition, the study design enables us to directly estimate a local average program effect for households for which $\Delta T > 0$.

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

To estimate $\tau_{jk,LATE}$ using Two-Stage Least Squares (2SLS), we proceed with the following two-stage estimation among the sample of households for whom $\Delta T > 0$:

$$T_i = \pi_0 + \pi_1 \mathbf{1}(Z_i = j) + \eta_i, \tag{7}$$

where T_i is the transfer amount, $\mathbf{1}(Z_i = j)$ is an indicator for being assigned to allocation rule j, and η_i is an error term. The coefficient π_1 captures the first-stage relationship between allocation rule and the amount of transfer that a household receives.

The second-stage specification follows as:

$$Y_i = \alpha + \tau_{jk,LATE} \widehat{T}_i + \varepsilon_i, \tag{8}$$

where Y_i is the outcome of interest, and $\widehat{T}_i$ is the predicted transfer amount from the first stage. The coefficient $\tau_{jk,LATE}$ captures the causal effect of increased transfers on the outcome among marginal beneficiaries (households for which $\Delta T > 0$).

In this setting, $\tau_{jk,\text{LATE}}$ provides an unbiased estimate of the local average treatment effect of an additional transfer amount under three key assumptions: (i) Z is randomly assigned; (ii) PMT rankings and transfer amounts cannot be manipulated by potential beneficiaries; and (iii) Z affects outcomes only through its impact on cash assistance. We empirically test the validity of the random assignment in both the sample and population data in the next section. Because PMT scores and the eligibility assessment methodology are known only to a small number of central staff and researchers, manipulation of scores is highly unlikely and formal tests of this provide supporting empirical evidence.[9] Finally, our research design does not explicitly account for general equilibrium effects; therefore, these estimates do not differentiate direct treatment effects from those driven by market-level changes due to program implementation. However, we expect general equilibrium effects to be limited, given that the overall size of the cash assistance program is less than one percent of Lebanon's GDP.[10]

[9] See Altındağ and O'Connell (2022) for details.

[10] For context, the cash transfer program studied in Egger et al. (2022) amounted to over 15 percent of local GDP.

The 2SLS estimates in equation 6 represent the average program effect for marginal beneficiaries prioritized for a larger assistance amount under targeting arm j versus k. We pool the data across all marginal beneficiaries from each pairwise counterfactual with targeting arm j, which recovers the weighted average across the pairwise samples with the weights corresponding to the relative sizes of the marginal beneficiary groups for each comparison. The pooled sample therefore comprises separate experimental subsamples corresponding to each district-by-experimental arm cell.[11] This allows us later to estimate Equation 6 for each district, for each budget allocation arm, or by the combination of these.

3.4 Balance tests and validation of intervention and empirical design

Appendix Table 1 shows that households in the post-intervention survey were balanced across treatment arms in pre-intervention characteristics. F statistics and corresponding p-values from a joint hypothesis test based on a specification that regresses each demographic variable of interest on indicators for three of four treatment arms fail to reject equality across means in all cases.

Next we test whether the allocation rules include in the program populations that are more likely to experience the specific type of deprivation that it intends to target. Empirical evidence supporting this in our setting is provided in Table 1. The first column presents four baseline poverty rates for populations in each allocation rule, derived from a simple Ordinary Least Squares (OLS) model. Using the baseline survey data, we regress pre-intervention poverty indicators on the vector of indicators for assigned treatment arm with the monetary poverty arm as the reference group. The specification is the sample analog to Equation 2 allowing for multiple treatment groups:

$$\begin{aligned} Y_i = \beta_0 + \beta_1 * 1[Z_i = \{Food\ insecurity\ targeting\}] \\ + \beta_2 * 1[Z_i = \{Food\ consumption\ targeting\}] \\ + \beta_3 * 1[Z_i = \{Multidimensional\ deprivation\ targeting\}] + u_i \end{aligned} \quad (9)$$

The table reports $\beta_0, \beta_1, \beta_2$ and β_3 across rows, with the indicator for each type of poverty (Y_i) specified in panels. The outcome and coefficients are scaled to be in percentage points out of 100. As expected, the first column shows the randomization balance in initial poverty rates across experimental targeting arms. Column 3 presents the same regression results limited to households eligible for the most generous assistance package. This confirms that each experimental arm shifted

The study found large impacts on consumption and assets for recipients, significant positive spillovers to non-recipient households and firms, minimal price inflation, and estimated a local transfer multiplier of 2.4.

[11] In this specification we cluster standard errors by household to accounting for households that are marginal beneficiaries across more than one counterfactual comparison.

the beneficiary profile toward the type of poverty targeted by that arm, though also incidentally towards others. For instance, 46.3 percent of beneficiaries receiving the highest value assistance package under the monetary poverty allocation rule are food insecure–a share that would be 13.2 percentage points higher if district-level budgets had been allocated based on food insecurity rates instead. Overall, the observed beneficiary profiles in each arm indicate that targeting based on specific area-level poverty indicators successfully directs aid toward the intended poor population.

Characterizing marginal beneficiaries

Similar to the right-hand side of Table 1, we show the characteristics of marginal beneficiaries prioritized by each strategy. Table 2 presents descriptive statistics on demographics, poverty levels, and well-being measures for marginal beneficiaries in the "control" condition across budget allocation rules, with each column testing mean differences relative to those in the monetary poverty column. Two insights emerge from this analysis. First, households targeted by expenditure or food consumption poverty indicators are relatively similar to each other, with few small differences from marginal beneficiaries in the monetary poverty allocation arm – and when differences are statistically significant, they are often of a small economic magnitude. Second, consumption-based geographic allocation rules prioritize starkly different demographic groups compared to the other vulnerability-based allocation strategies. Marginal beneficiaries in consumption-based allocation arms have higher ability to borrow and have higher baseline assets. Despite higher nominal consumption, beneficiaries in the food insecurity and multidimensional deprivation arms 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.

4 Results

4.1 Program effects

2SLS program effects among marginal beneficiaries

We estimate the local average program effects by marginal beneficiary groups to compare the heterogeneity in program effects due to the area level poverty indicator used for reallocation of program resources across districts. As above, we identify all beneficiaries assigned to treatment arm j or k, where treatment arm j offers a larger transfer amount, i.e., $T(j) > T(k)$, representing the population prioritized by the allocation rule j. Among these households prioritized for assistance by allocation rule j, only approximately half would be assigned to the more generous allocation arm — yielding an experimental subsample of households marginal to inclusion under the prioritization of

poverty metric j, which is recovered by estimating equation 8 via 2SLS.[12]

Figure 1 presents the 2SLS estimates for our pre-specified outcomes on poverty and child well-being.[13] The estimated coefficients reflect the program's effect of an additional 1 million 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. Budget allocation methods yield variable impacts: using a monetary poverty indicator for prioritization increases per capita expenditure (0.21 SD), coping strategies (0.15 SD), and food consumption scores (0.16 SD) among beneficiaries who are marginally included in the program. Multidimensional deprivation uniformly mitigates all deprivation outcomes (effects ranging from 0.11 SD to 0.15 SD), whereas food insecurity or inadequate nutrition shows limited—and often statistically insignificant—benefits (0.02 SD to 0.10 SD), except in the case of per capita expenditure. This underscores the nuanced efficacy of different allocation strategies in poverty alleviation.

[12] Since a household can be marginally prioritized in up to three counterfactual pairings, we estimate equation 6 using a stacked sample and cluster standard errors at the household level.

[13] Appendix Table 5 contains the first stages, and the full set of pre-registered specifications with corrections for multiple hypothesis testing are available in Appendix Table 7.

Table 1: Tests of balance and targeting effect on beneficiary profiles

Targeting arm	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)	13.18***	(2.79)
I[Z=Food consumption arm]	0.58	(2)	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)

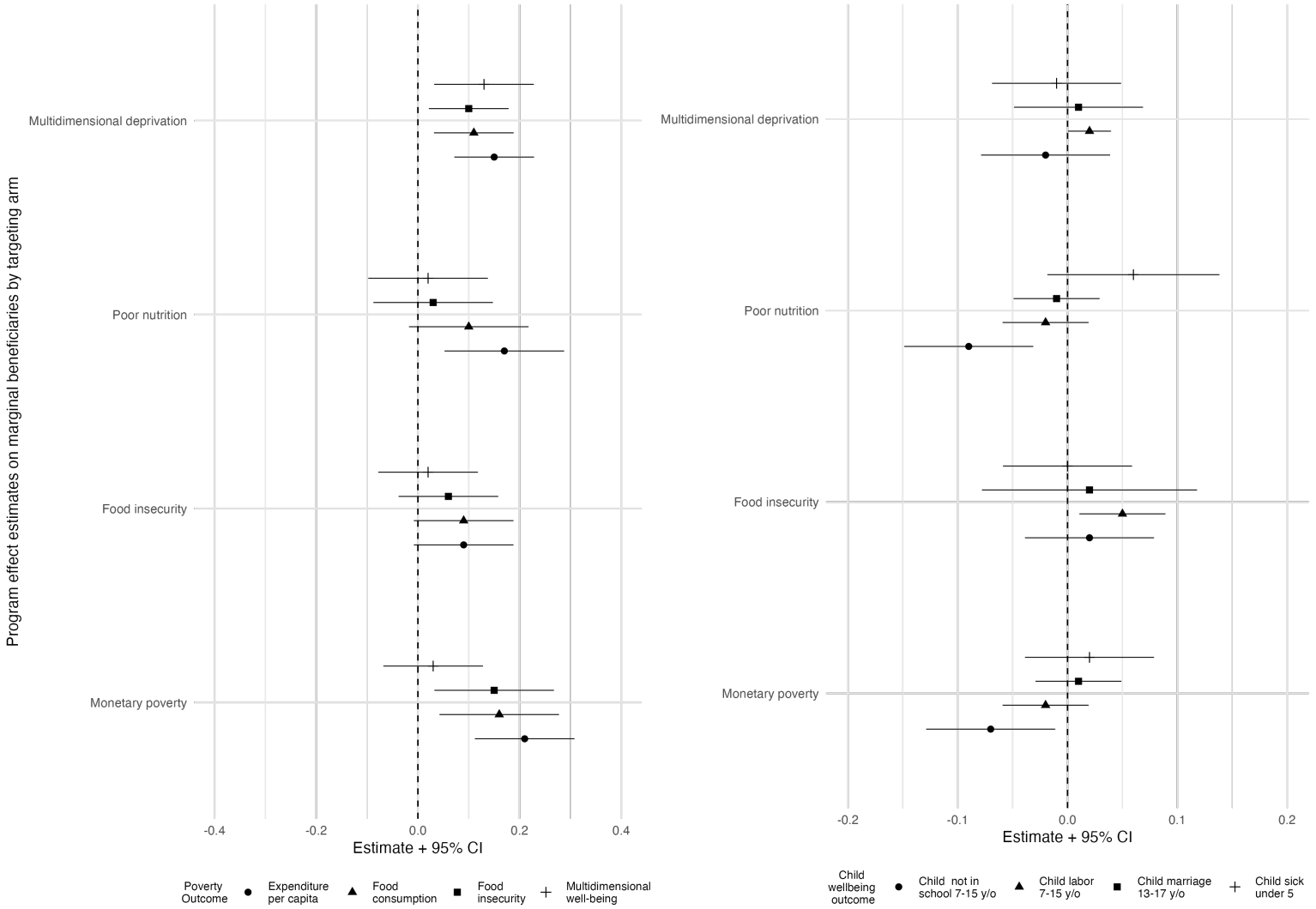
Note: Table contains means of pre-intervention poverty rates and differences relative to households in the monetary poverty targeting arm among all sampled households (Columns 1 and 2) and post-intervention beneficiaries (Columns 3 and 4). Results of the t-test of mean differences between beneficiaries and non-beneficiaries are indicated by $^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$. **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.

Table 2: 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. The results of t-tests of mean differences between the sample indicated in the column relative to the beneficiaries marginal to the Monetary Poverty arm are indicated by $^{*}p < .01$; $^{**}p < .05$; $^{***}p < .01$ **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.

Figure 1: Program LATE estimates by marginal subsample, primary + childrens' outcomes



Note: Figure depicts LATE effects for each sample and across four primary outcomes in the left panel, and four children's outcomes in the right panel. 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.

From the left panel of Figure 1, we find that there is no single allocation method that generates the highest program improvements across all outcomes. Only in some cases does the largest effect across outcomes align with the poverty indicator used for prioritization, occurring only when using monetary poverty and multidimensional deprivation for budget allocation. On the other hand, households marginally prioritized for inclusion in the program when targeting food-related measures has a limited effect on food insecurity or food consumption — despite the fact these allocation arms are more likely to include households with significantly worse food consumption or food security, respectively. Treatment effects differences across households prioritized by different allocation methods are not large enough to statistically reject each other, suggesting that no single budget allocation decision has the potential to meaningfully increase aggregate outcomes on any given metric of well-being.

It is clear, however, that households prioritized by multidimensional deprivation targeting are the only marginal beneficiaries that exhibit treatment effects that are statistically different from zero in all four primary poverty outcomes. This ends up being the case because it is the only strategy that yields treatment affects statistically distinguishable from zero on multidimensional deprivation outcomes.[14] Similar to this, households prioritized by monetary poverty targeting improve in three outcomes (expenditure poverty, food security and food consumption) excluding multidimensional deprivation. Targeting poor food consumption improves expenditure based poverty and food consumption (although the latter being marginally insignificant), and targeting food insecurity includes households that exhibit no statistically significant treatment effects.

The right panel of Figure 1 reveals substantial improvements in school enrollment for marginal beneficiaries within the monetary poverty and food consumption allocation arms, ranging between seven and nine percentage points, corresponding to a 32% to 42% increase in school enrollment. We observe a statistically significant increase (five percentage points) in child labor among marginal beneficiaries of the food insecurity allocation arm. Apart from this, no significant changes are noted in other child well-being outcomes across the different types of targeted marginal beneficiaries, nor are there substantial and systematic improvements in livelihood coping strategies, shelter, or water, sanitation, and hygiene (WASH) indices, living conditions, assets or savings, or other outcomes of social cohesion or property rights (see Appendix Table 7).

Overall, the program is generally effective in alleviating the primary poverty outcomes it aims to address, although there is not substantial treatment effect heterogeneity in effects on poverty outcomes across households marginal to different prioritization rules. Results for outcomes measuring child well-being are more definitive, as we observe substantially higher program effects

[14] This result is attributable to improvements to living quarters via lower physical crowding and reductions in physical insecurity.

among households prioritized by a consumption-based poverty metric.

Intent to treat estimates of overall differences in well-being across targeting strategies

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 are in Appendix Figure 2. The middle panel tests the above-noted prediction from the empirical design: null effects among the inframarginal beneficiaries, whose targeting arm does not affect their assistance eligibility. The third panel tests for differences in endline economic well-being among marginal beneficiaries. These estimates represent the combination of treatment effects from those who were included under the given prioritization strategy minus effects among those whom the prioritization excluded. Since the differences in effects were largely not statistically distinguishable from each other in Figure 1, we expect that effects among households included under a specific allocation rule will be largely offset by equivalent effects among those excluded by the rule. In Appendix Figure 2, we show that none of the differences in poverty outcomes are substantial enough to differentiate the three alternative allocation rules from monetary poverty targeting. Similarly, no significant differences were found in secondary outcomes of livelihood coping strategies, shelter, or WASH indices.[15] We therefore conclude that the decision of whom to allocate resources to does not lead to meaningful aggregate differences in poverty reduction.[16] In other words, there is no program effectiveness "arbitrage" across choices of poverty indicator for program planner's resource allocation rule. This holds true even for marginal beneficiaries, who are exclusively targeted by the choice of allocation rule.

4.2 Targeting and Program Effect Heterogeneity by Location

4.2.1 2SLS effects by location

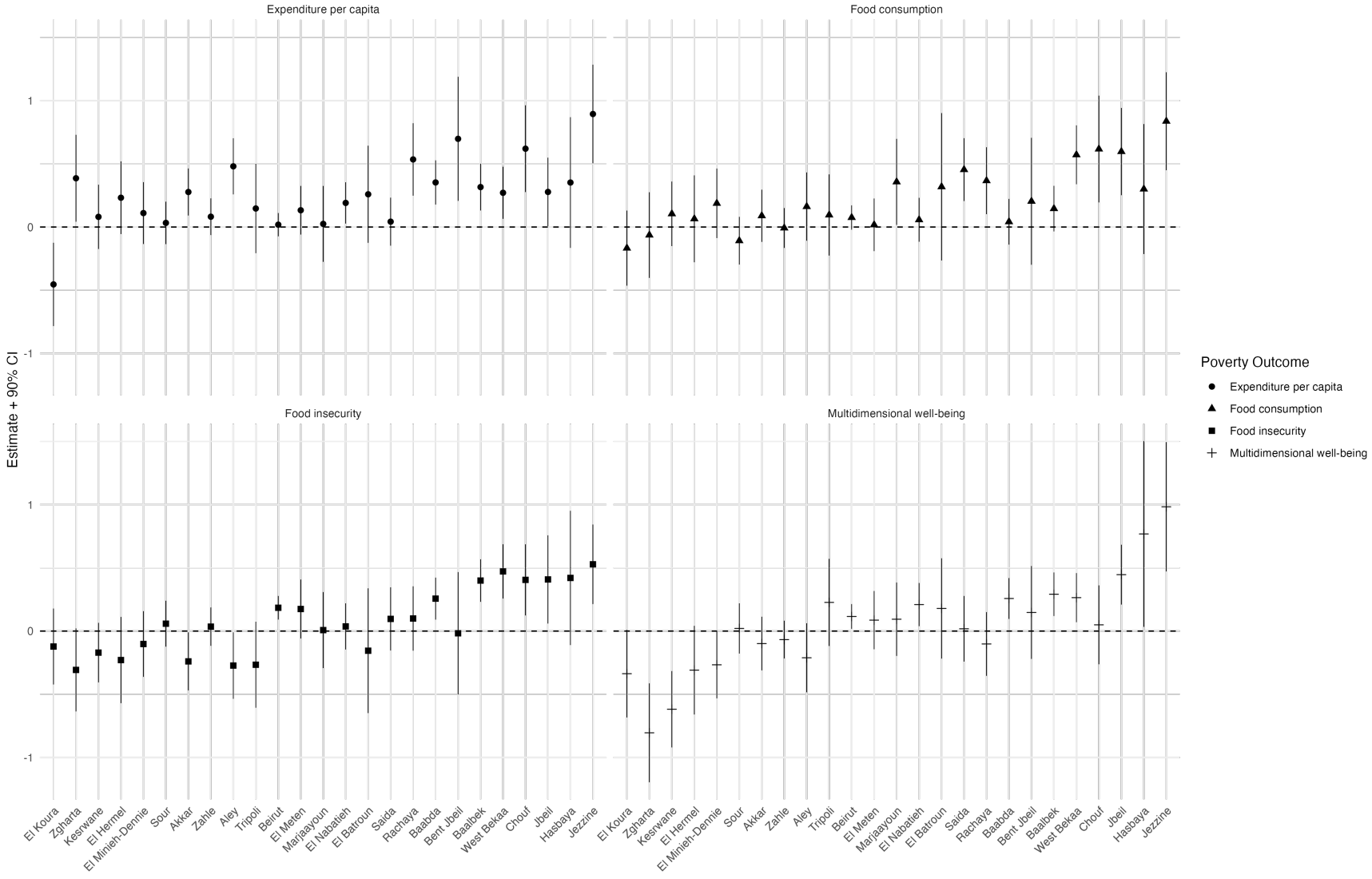
Next, we pool all marginal beneficiaries and estimate a program effect for each district in the country to measure the degree of heterogeneity in program effectiveness across localities. Figure 2 shows the distribution of the estimated program effects on our main poverty indicators. Effects are plotted 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, and this heterogeneity is not a result of sampling variation. Districts are ordered similarly across panels, where it becomes apparent that 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 on their own, and there is a substantial positive correlation in effect sizes across outcomes. That is, the districts

[15] See Appendix Tables Tables 2 - 4 for tabular results.

[16] These results are not due to a lack of precision, as our sample sizes provide a minimum detectable effect of approximately 0.1SD under standard test parameters. Note also that these results do not imply the program's ineffectiveness, but that the use of alternative targeting strategies does not yield differences in overall well-being across targetings arms.

with the largest effect sizes 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. A reallocation exercise in which we retain the total budget and assistance values by household but redistribute the entire program's resources to prioritize areas with higher treatment effects would raise overall program effectiveness from an overall average of .03 to .20 SDs to .27 to .41 SDs across outcomes. These results suggest location-specific factors are important determinants of program effects, and joint hypothesis tests reject a null of true differences across districts being zero with $p < 0.01$ for all four outcomes. 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 2: 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. Joint hypothesis tests reject a null of true differences across districts being zero with $p < 0.01$ for all four outcomes.

4.2.2 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 budget allocation rule, 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 3 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 program effect's location accounts for 29-34% of the total reduction in impurity across all splits, making it the most important feature in determining treatment effect heterogeneity.[17] The targeting rule contributes 9-11% improvement in prediction. 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 unobserved location-specific factors in explaining the heterogeneity of program effects net of all other features of program design and operation, including the budget allocation rule of the program, demographics of the beneficiaries, or their starting poverty level.[18]

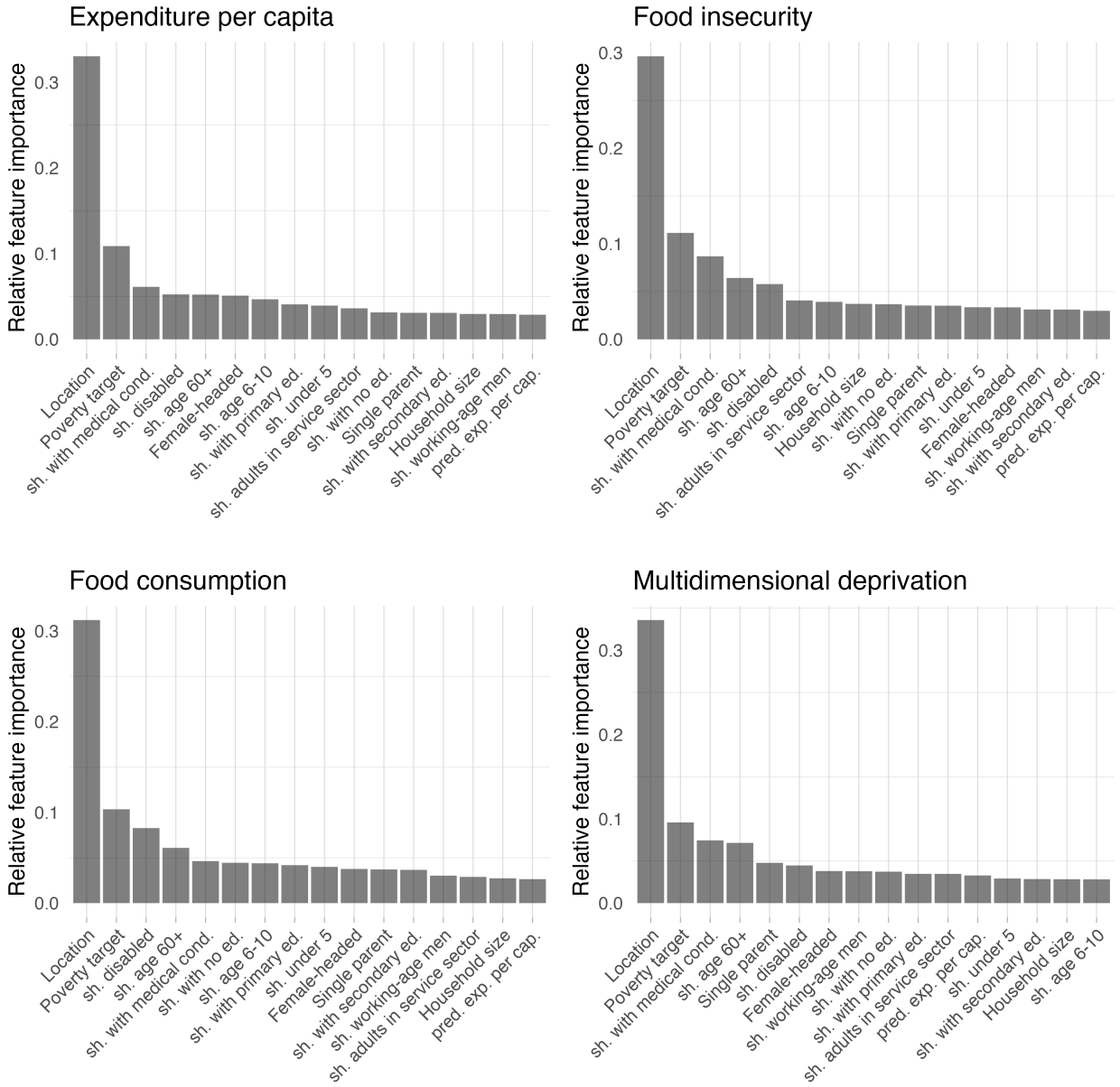
4.3 Program Satisfaction

Even if a given targeting strategy does not yield aggregate improvements in effectiveness, program implementers may also face higher costs if one method results in greater dissatisfaction. In Appendix Table 9, we present an ITT analysis of satisfaction, perceived fairness, and perceived accuracy among households subjected to different 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. 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. Using administrative records, the final column considers grievance redress claim rates in the entire population, which fluctuate between 25% and

[17] This metric reflects how much each variable improves the model's ability to partition the data, analogous to a variable's contribution to model fit in a regression context.

[18] In the Appendix Table 8, we also 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.

Figure 3: Predictors of treatment effects across experimental subsamples



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

27%. Although 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.[19]

5 Discussion

Budget allocation rules can be used to effectively prioritize populations with distinct demographic and socioeconomic backgrounds. However, the heterogeneity in program outcomes generated by changes to these allocation rules is limited. For a given locality, the program effects for marginally included households do not show substantial variation, despite their very different demographic backgrounds and economic constraints. In contrast, we find pronounced effect heterogeneity observed across locations independent of the beneficiary characteristics. Unobserved location-specific factors remain as a strong determinant of treatment effect heterogeneity, even after accounting for baseline poverty levels and demographics – which are the typical fields available to program implementers for targeting purposes. These findings are consistent with 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).

Qualitative evidence suggests that implementing agencies often have limited information to understand and predict the effectiveness of cash transfer programs. For instance, our focus group discussions revealed that the improvement in school enrollment among the beneficiaries in consumption-based targeting groups was related to parents' ability to afford commuting to school. The study period coincided with 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 no longer able 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, and were otherwise unpredictable at the time of program targeting. In contrast, parents in urban areas faced congestion in schools as well as changing rules about children's legal

[19] Appendix Table 10 shows that individuals' perceptions of fairness and satisfaction with the program are, perhaps unsurprisingly, heavily influenced by the level of benefits they receive. For example, while 69% of individuals randomized into the less generous arm express dissatisfaction with the targeting strategy, this figure decreases by 41 percentage points among those who receive an additional 1 million LBP per month in the more generous arm. Non-beneficiaries are approximately 1.7 to 2.7 times more likely to report dissatisfaction and perceive unfairness compared to beneficiaries. Similarly, beneficiaries randomized to the more generous targeting arm are substantially less likely to file grievance redress claims than those in the less generous arm. The effect of being prioritized for greater assistance on satisfaction survey outcomes is generally indistinguishable across targeting arms, although the larger sample of claimancy records allows us to show 10 to 15 percentage points higher claimancy among households prioritized and supported under food insecurity and multidimensional deprivation arms.

documentation – constraints that could not be alleviated with additional income. From another angle, marginal beneficiaries supported by food insecurity targeting were found to have the highest existing debt levels and a high prevalence of costly medical conditions – factors that were not present in survey data for or discussions with other marginal beneficiary groups. These existing conditions constrained the use of transfers in alleviating food insecurity and other forms of poverty, as recipients used a higher share of their payments to reduce their debt and expense burdens.

Our findings indicate that, when holding the program implementation site constant, beneficiary targeting alone is likely to yield limited variation in the effectiveness of cash transfer programs. This is because local market constraints—shared by all beneficiaries in a given area—play a central role in shaping program impacts. As a result, understanding the functioning and conditions of local markets is crucial for interpreting the effects and limitations of cash-based social protection programs.

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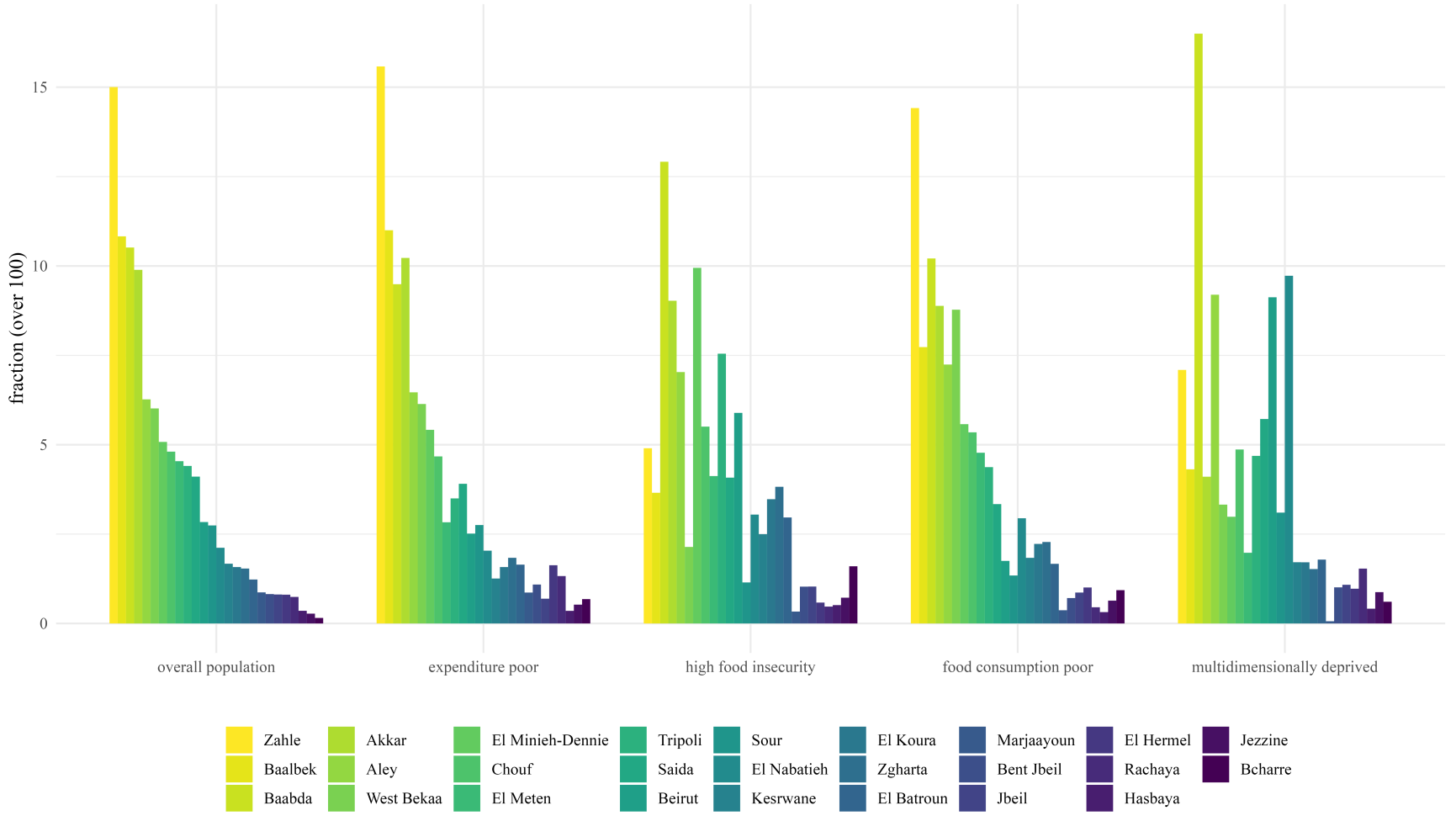
Appendix Figures and Tables

Appendix Figure 1: District level variation by type of deprivation



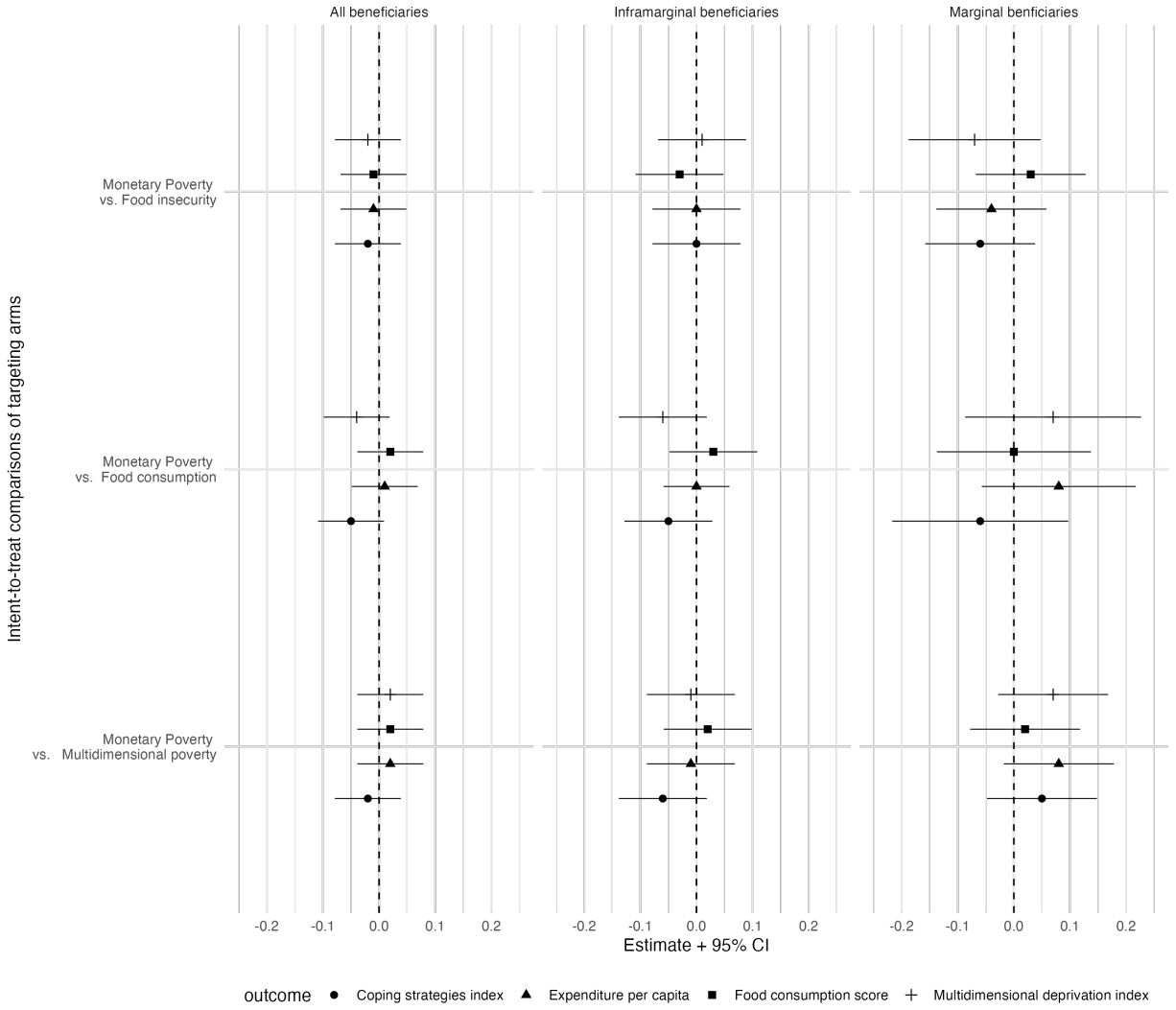
Note: The graphic shows the distribution of the poor by district and poverty definition. The sum of all populations are normalized to 100. The first group of columns on the *x*-axis shows the benchmark population distribution by district. The second group, "expenditure poor" shows each district's share 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.

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

Measure	Means by targeting treatment arm				Tests	
	Monetary poverty	Food insecurity	Food consumption	Multidim. deprivation	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. Panels indicate the sample used, and the means of each variable are presented across columns for each treatment arm sample. Panel A contains the full post-intervention sample, and Panel B contains these tests for the marginal beneficiary population. 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.

Appendix Figure 2: 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.

Appendix Table 2: 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 3: 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 4: 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 5: 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 6: 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 7: 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. $^{*}q < .10$; $^{**}q < .05$; $^{***}q < .01$ **Reading:** Households marginal to the monetary poverty targeting arm have .207 standard deviation higher ln(expenditure per capita) when they receive a higher transfer due to being assigned to the monetary poverty targeting arm.

Appendix Table 8: 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.

Appendix Table 9: 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.

Appendix Table 10: LATE effects on program satisfaction and grievance complaints

	Pooled sample	Monetary poverty targeting	Food insecurity targeting	Poor nutrition targeting	Multidimensional deprivation targeting
Outcome: Is dissatisfied					
coef.	-0.41***	-0.37***	-0.38***	-0.42***	-0.45***
(se)	(0.03)	(0.06)	(0.07)	(0.06)	(0.06)
Control mean	0.69	0.66	0.68	0.71	0.72
Control SD	0.46	0.48	0.47	0.46	0.45
N	1099	309	222	294	274
Outcome: Community is dissatisfied					
coef.	-0.22***	-0.19***	-0.21***	-0.23***	-0.24***
(se)	(0.03)	(0.06)	(0.07)	(0.07)	(0.06)
Control mean	0.62	0.56	0.61	0.6	0.7
Control SD	0.49	0.5	0.49	0.49	0.46
N	1099	309	222	294	274
Outcome: Selection is unfair					
coef.	-0.21***	-0.17***	-0.3***	-0.17***	-0.19***
(se)	(0.03)	(0.06)	(0.07)	(0.06)	(0.07)
Control mean	0.5	0.39	0.59	0.42	0.62
Control SD	0.5	0.49	0.49	0.5	0.49
N	1099	309	222	294	274
Outcome: Selection is inaccurate					
coef.	-0.16***	-0.12**	-0.15**	-0.17***	-0.22***
(se)	(0.03)	(0.06)	(0.07)	(0.06)	(0.07)
Control mean	0.46	0.36	0.55	0.41	0.53
Control SD	0.5	0.48	0.5	0.49	0.5
N	1099	309	222	294	274
Outcome: Filed grievance redress claim					
coef.	-0.31***	-0.38***	-0.27***	-0.35***	-0.22***
(se)	(0)	(0)	(0.01)	(0)	(0)
Control mean	0.42	0.43	0.42	0.43	0.39
Control SD	0.49	0.5	0.49	0.5	0.49
N	176597	49481	32548	44328	50240

Note: Table contains estimates of τ from Equation (6) in the text. **Reading:** Households marginal to the monetary poverty targeting arm have 37 percentage-point lower rate of reporting being dissatisfied with the program when they receive a higher transfer due to being assigned to the monetary poverty targeting arm.