

Food-Assistance Modality, Geographic Patterning, and Child Nutrition in Rohingya Camps: A Reanalysis of Public Replication Data

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Research Article

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Food-Assistance Modality, Geographic Patterning, and Child Nutrition in Rohingya Camps: A Reanalysis of Public Replication Data

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Abstract

Background: Public replication data can support independent verification while remaining insufficient for causal or mechanism-based claims. We reassessed food-assistance modality and child anthropometry in Rohingya camps, focusing on geographic patterning and the limits of the released extract.

Methods: We reanalysed 523 child records. HAZ was the main continuous outcome; stunting, WHZ, and wasting were secondary. Alternative conditioning sets assessed specification sensitivity; none was treated as causally sufficient. Household- and camp-clustered inference and a restricted wild cluster score procedure characterised uncertainty. The audit assessed whether variables required for additional secondary claims were available.

Results: Modality was strongly patterned by log clinic distance (SMD 1.280), registered-camp residence (SMD 1.111), and refugee-arrival group (SMD 1.107); only 17 of 31 camps contained both modalities. The crude HAZ coefficient was 0.280 (95% CI 0.015 to 0.545); the combined estimate was 0.209 (household-clustered 95% CI -0.147 to 0.565 ; camp-clustered 95% CI -0.190 to 0.607 ; restricted score $p = 0.352$). Stunting estimates remained favourable in direction but uncertain. WHZ and wasting estimates were imprecise and compatible with the null. The released variables were insufficient to reproduce the source study's complete adjusted analysis or support multidimensional healthcare-access measurement.

Conclusions: The extract supports descriptive reassessment, not a causal estimate of programme effectiveness. Geographic and institutional patterning limits interpretation. Credible secondary evaluation requires fuller design, allocation, and linkage information.

Keywords: food assistance; child nutrition; Rohingya; reproducibility; programme evaluation; geographic patterning; secondary data analysis; humanitarian monitoring

1. Black RE, Victora CG, Walker SP, et al. Maternal and child undernutrition and overweight in low-income and middle-income countries. *Lancet*. 2013;382(9890):427–451. doi:10.1016/S0140-6736(13)60937-X.

Child undernutrition in humanitarian settings reflects more than food quantity. Infection, poverty, housing conditions, displacement history, infrastructure, and access to timely and appropriate health services can jointly shape growth. Food assistance is therefore delivered within a structural environment that may influence both programme assignment and nutritional outcomes. Comparisons between electronic vouchers and in-kind transfers are especially vulnerable to programme-placement bias when modalities are concentrated in different camps or refugee groups [1–5].

Healthcare access is multidimensional: geographic proximity alone cannot establish service availability, affordability, information access, respectful treatment, medicine supply, provider competence, or clinical quality [6,7,32]. This distinction is used only to determine which secondary claims the released variables can support, not to judge whether the source investigators should have designed the extract as a health-system survey.

Documentation reviewed alongside the replication file included an Emergency Health and Nutrition Survey Round 3 questionnaire containing child morbidity and care-seeking questions. The released 22-variable child-level file does not contain corresponding row-level care-seeking or treatment-barrier responses, and no authenticated merge file was available. The questionnaire therefore informs the measurement audit only and is not treated as evidence that every item was observed for every analysed child [10].

In protracted displacement settings, food-assistance monitoring must distinguish programme differences from changes in settlement geography, markets, service access, and administrative

conditions. Sustainability is treated as a practical monitoring implication, not as an outcome measured in the 2018 cross-sectional data.

The study had two linked objectives. The empirical objective was to examine how the association between e-voucher versus food-transfer receipt and HAZ changed across alternative descriptive conditioning sets; stunting, WHZ, and wasting were secondary outcomes. The audit objective was to distinguish results directly reproducible from the released file from constructs and analyses requiring unavailable variables or design information. No specification was designated as a sufficient causal adjustment set, and the analysis was not designed to estimate mediation or a causal programme effect.

The distinction between chronic and acute undernutrition is analytically important. Linear growth failure accumulates through sustained nutritional deprivation, recurrent infection, inflammation, and adverse caregiving or environmental conditions, whereas wasting more often reflects recent or severe energy imbalance and illness. HAZ and WHZ therefore capture related but non-interchangeable processes. Analysing both continuous z scores and their binary clinical classifications avoids relying on a single representation of nutritional status and reduces the information loss created by dichotomisation [1,2,9,11].

Evidence on cash and voucher assistance is heterogeneous. Systematic reviews suggest small average improvements in HAZ and stunting from cash transfers, alongside more consistent gains in dietary diversity and household food security, but little average effect on wasting. Effects vary with transfer value, conditionality, market functioning, inflation, service availability, household preferences, and programme implementation [3,4,12–15,31]. Humanitarian guidance consequently treats cash and vouchers as instruments that may influence several underlying determinants of nutrition rather than as stand-alone nutrition interventions [14,15].

In Cox's Bazar, this contextual dependence is especially salient. Repeated cross-sectional surveys have documented substantial but changing burdens of acute malnutrition and anaemia, while more recent evidence continues to show persistent nutritional vulnerability, seasonal variation, and declining purchasing power of food assistance [16–18]. These studies caution against interpreting one cross-sectional modality comparison as a timeless estimate. Programme effects may differ across periods of market disruption, inflation, monsoon exposure, camp consolidation, and changes in service provision.

Structural barriers can enter the food–nutrition relationship at several points. Limited household resources can constrain complementary foods, transport, and caregiving time. Damaged shelter, unsafe water, inadequate sanitation, and crowding can increase exposure to enteric pathogens. Administrative status and camp location can shape eligibility, information, freedom of movement, and proximity to services. Health-system barriers can delay treatment or reduce the adequacy of care. Environmental enteric dysfunction provides one plausible biological pathway linking repeated faecal–oral exposure and intestinal inflammation to impaired linear growth, although biomarker evidence is heterogeneous and the mechanism cannot be inferred from anthropometric and distance data alone [19–22].

Associations between assistance modality and nutrition were examined for sensitivity across alternative conditioning and covariance structures without treating any model as causally definitive. Secondary mechanism-based claims were considered supportable only where the released file contained direct measures of the proposed mechanism. This distinction separates analysis of observed structural indicators from assessment of variables unavailable in the public extract.

Reproducible computation concerns whether the same data and procedures regenerate reported results, while broader reproducibility terminology distinguishes methods, results, and inferential agreement. FAIR principles further emphasise reusable data, metadata, and provenance [33–35]. This study applies those established principles to one humanitarian replication package rather than proposing a universally validated framework.

This study contributes an independently verified reanalysis of food-assistance modality and child anthropometry in a protracted displacement setting. It quantifies geographic and institutional patterning, evaluates the sensitivity of conditional estimates to alternative conditioning and covariance structures, and documents which analyses can and cannot be supported by the released extract. The analytical target is the secondary-use capacity of the public file, not the programme’s true causal effect or the validity of the complete source study. For sustainability scholarship, the case shows how incomplete allocation, geographic, and linkage information can restrict the capacity of humanitarian monitoring systems to support transparent adaptation over time.

2. Prendergast AJ, Humphrey JH. The stunting syndrome in developing countries. Paediatr Int Child Health. 2014;34(4):250–265. doi:10.1179/2046905514Y.0000000158.

2.1 Measured structural context

Structural context was defined as the observable socioeconomic, household, displacement, institutional, and geographic conditions that could affect programme placement, child nutrition, or both. Socioeconomic resources comprised asset wealth and formal education. Household vulnerability comprised damaged housing and household-head structure. Displacement and institutional position comprised refugee-arrival group and registered-camp residence. Geographic context comprised recorded distances to primary healthcare and food distribution. These domains were modelled separately and jointly. They were not combined into a single score because the indicators represent different causal and institutional processes rather than interchangeable manifestations of one reflective construct.

2.2 Secondary-use and construct coverage

The construct-coverage assessment asked whether the released variables could support additional secondary claims about multidimensional healthcare access. It does not evaluate the original purpose or quality of the deposit. Clinic distance represents only part of physical accessibility; the file does not directly measure availability, affordability, information access, acceptability, or clinical quality. No healthcare-access score was constructed or validated.

Covariates were grouped by observable domain rather than assumed causal order. Because temporal roles were uncertain, the specifications assessed sensitivity and were not used to identify mediation or a sufficient confounding-control set.

Table 1 summarises numerical reproducibility, design completeness, allocation information, geographic linkage, and construct coverage. The healthcare-access row illustrates that conceptual relevance does not establish that a construct is measured in the analytical file.

Table 1. Reproducibility and secondary-use audit of the released extract

Audit domain	Required information	Released-file status	Analytical consequence
Numerical reproduction	Sample, modality, and anthropometric counts	Supported	Counts and binary outcomes regenerated from released data
Original complete analysis	Weights, design variables,	Not exactly reproducible	Required elements are

Audit domain	Required information	Released-file status	Analytical consequence
	and full control set		absent from the reduced extract
Programme allocation	Assignment rules, timing, value, and mixed receipt	Not represented	Causal modality comparison is unsupported
Geographic linkage	Camp and distance indicators	Partially supported	Overlap and geographic patterning can be assessed
Healthcare-access mechanisms	Direct multidimensional healthcare-access measures	Unavailable	Mechanism-based healthcare-access claims are unsupported

Note. Healthcare-access coverage is assessed only to determine the supportability of subsequent secondary claims, not to evaluate the original purpose of the data release.

3. Doocy S, Tappis H. Cash-based approaches in humanitarian emergencies: a systematic review. *Campbell Syst Rev.* 2017;13(1):1–200. doi:10.4073/csr.2017.17.

3.1 Design, setting, and data source

This cross-sectional, reproducibility-oriented secondary analysis used the public replication file accompanying Hoddinott and colleagues’ study of food transfers, electronic food vouchers, and child nutritional status among Rohingya children in Bangladesh [5]. The underlying survey was conducted in Cox’s Bazar during October and November 2018. The replication file was deposited in Harvard Dataverse under DOI 10.7910/DVN/5BAN6C [10]. Reporting was organised with reference to STROBE guidance for cross-sectional studies [8]. This reanalysis does not evaluate the validity of the complete source analysis because the public extract lacks several original controls and survey-design elements; it evaluates the reproducibility and secondary-use capacity of the released materials.

3.2 Data verification and participants

The file was audited before analysis. It contained 523 child records, 22 original variables, and 507 unique households. Sixteen households contributed two children, and no household contributed more than two. Household-child identifier combinations were unique. Stunting and wasting were independently reconstructed from HAZ and WHZ and matched the supplied indicators exactly. All 523 children had valid HAZ and stunting data. Seven lacked valid WHZ

and wasting data, leaving 516 for acute-nutrition analyses. Because the released file determined the available sample, no a priori sample-size calculation was undertaken for this reanalysis.

3.3 Outcomes and exposure

Continuous outcomes were HAZ and WHZ. Binary outcomes were stunting, defined as HAZ <-2 , and wasting, defined as WHZ <-2 , following the WHO Child Growth Standards [9]. The exposure was food-assistance modality: food/general food distribution (reference) versus cash/e-voucher. The released file did not contain weight-for-age z scores or an underweight indicator; those outcomes were therefore not analysed. The observed age range was 7–23 months, although the source study targeted children aged 6–23 months [5].

3.4 Covariates and structural domains

Demographic variables were child age, child sex, maternal age, and maternal formal education. Socioeconomic resources were asset wealth and household-head formal education. Household vulnerability was represented by female-headed household status and damaged housing. Displacement and institutional position were represented by refugee-arrival group and registered-camp status. Geographic context comprised distances to a primary health clinic and food-distribution point, converted from metres to kilometres. The terms describe available indicators and do not imply complete measurement of each structural construct.

3.5 Statistical analysis

Characteristics were summarised overall and by assistance modality. Model 1 was crude; Model 2 conditioned on child age, child sex, maternal age, and maternal education; Models 3A–3D added socioeconomic, household, institutional/displacement, or geographic domains; and Model 4 combined all documented variables. These models were alternative descriptive conditioning sets, not a causal sequence. Changes in coefficients cannot distinguish confounding control from over-adjustment, proxy adjustment, collider bias, or loss of independent exposure variation. OLS coefficients were reported for HAZ and WHZ, and odds ratios for stunting and wasting.

The authoritative analysis was executed in Python 3.12 using pandas, NumPy, SciPy, patsy, and statsmodels. Household-clustered HC1-type covariance was used for the main descriptive models. Because modality was strongly patterned by camp, M4 coefficients were additionally evaluated using camp-clustered covariance with t inference based on 30 degrees of freedom and a restricted wild cluster score test with 99,999 Rademacher draws, seed 20260906, and the null

modality coefficient imposed. Raw- and log-distance M4 specifications, common-support diagnostics, mixed-modality-camp analyses, household-cluster bootstrap distance contrasts, small-camp exclusions, and logistic stability diagnostics were treated as sensitivity analyses. Complete algorithms and outputs are provided in the supplement and reproducibility package.

Potential bias was considered at the stages of selection, measurement, confounding, and model specification. The analytic sample was predetermined by the released file, so selection into the original survey could not be reconstructed. Anthropometric indicators were already processed in the replication file, but binary outcomes were independently regenerated from the continuous z scores. Exposure misclassification could arise if the binary modality variable simplified households receiving multiple forms of assistance. Unmeasured confounding remained likely because diet, morbidity, water and sanitation, household size, and detailed programme-allocation rules were unavailable.

Continuous outcomes were retained because thresholding z scores can reduce statistical power and obscure changes within clinical categories. Binary outcomes were included because stunting and wasting remain policy-relevant classifications. Continuous and binary outcomes were interpreted jointly, with emphasis on effect direction, magnitude, and uncertainty rather than threshold-based significance. Because the outcomes are correlated and multiple structural specifications were examined, secondary p values were interpreted cautiously and no multiplicity-adjusted confirmatory claim was made.

The choice of household-clustered standard errors reflected the observed data structure rather than a claim about the original multistage sampling design. This covariance estimator permits residual correlation among children within the same household, but it does not correct unequal selection probabilities or clustering at unreleased block or enumeration-area levels. Survey-weighted population estimates were therefore not attempted. This limitation separates conditional associations in the released analytical sample from population-representative estimates.

Covariate overlap was assessed descriptively and through correlations among modality, registration, refugee group, and distance. Propensity-score weighting was not promoted to the primary analysis because weighting cannot create information where exposure groups have little comparable support, and balance on measured covariates does not eliminate unmeasured

confounding [26–28]. Transparent regression sequences were preferred because they show how the modality coefficient changes as conceptually defined blocks enter the model.

Model diagnostics were interpreted as screening tools. Variance inflation factors assessed linear dependence, while Cook’s distance and leverage identified observations warranting sensitivity attention. Flexible distance modelling was used because categorising a heavily skewed continuous exposure at sample-dependent cut points could manufacture apparent gradients. Camp fixed effects were treated as sensitivity analyses because sparse cells can generate unstable or non-estimable logistic coefficients, especially for wasting with only 82 events [29,30].

3.6 Missing data and analytical denominators

HAZ, stunting, exposure, and all verified covariates were complete. Seven children lacked WHZ and wasting values: 2/362 food-transfer observations and 5/161 e-voucher observations. Acute-nutrition models therefore used 516 complete outcomes; chronic-nutrition models used all 523 records. No outcome imputation was performed because auxiliary morbidity and measurement variables were unavailable.

3.7 Ethics and reproducibility

The analysis used de-identified public data. The original study reported approval from the International Food Policy Research Institute institutional review board and informed consent [5]. The authoritative Python workflow, software manifest, input and output checksums, complete machine-readable outputs, and supplementary tables accompany this manuscript. No independently verified R implementation is claimed.

4. Gentilini U. Revisiting the “cash versus food” debate: new evidence for an old puzzle?

World Bank Res Obs. 2016;31(1):135–167. doi:10.1093/wbro/lkv012.

4.1 Sample and structural patterning

The sample comprised 362 children in food-transfer households and 161 in e-voucher households. Mean age was 15.52 months (SD 4.70), stunting prevalence was 33.5%, and wasting prevalence was 15.9% among valid records. Exact balance diagnostics showed large modality differences for log clinic distance (SMD = 1.280), registered-camp residence (SMD = 1.111), older refugee-arrival group (SMD = 1.107), and log food-distribution-point distance (SMD = 0.746). The probability that a randomly selected e-voucher observation had greater clinic

distance than a food-transfer observation was 0.792 (rank-biserial correlation = 0.584). Only 17 of 31 observed camps contained both modalities. These patterns indicate restricted comparability and strong geographic and institutional patterning of assistance modality (Table 2; Supplementary Tables S1–S2).

SMDs were calculated directly from the authenticated row-level file. Distance SMDs use $\log(1 + \text{metres})$.

Table 2. Structural balance by assistance modality

Characteristic	Food transfer/GFD	E-voucher	Absolute SMD
Age, months	15.497	15.559	0.013
Maternal age, years	28.343	31.193	0.300
Wealth index	−0.380	−0.055	0.417
Log clinic distance	6.682	8.771	1.280
Log food-distribution-point distance	6.324	6.956	0.746
Female-coded child records	54.1%	47.8%	0.127
Maternal formal education	21.3%	21.1%	0.004
Female-headed household	14.1%	24.8%	0.274
Household-head formal education	28.7%	29.2%	0.010
Damaged housing	66.9%	64.0%	0.060
Registered-camp residence	6.9%	50.9%	1.111
Older refugee-arrival group	15.5%	62.7%	1.107

Note. SMDs were calculated directly from the authenticated row-level file; distance SMDs use $\log(1 + \text{metres})$.

4.2 Height-for-age and stunting

In the crude OLS model, e-voucher receipt was associated with a 0.280-SD higher HAZ (95% CI 0.015 to 0.545; $p = 0.038$). The coefficient was 0.292 after demographic conditioning, 0.215 after adding displacement and institutional position, and 0.209 in the combined specification (95% CI -0.147 to 0.565 ; $p = 0.251$). The direction remained favourable while precision deteriorated. Because the causal roles of the added variables cannot be established, these changes are interpreted as specification dependence rather than evidence that structural variables explained, mediated, or disproved a programme association. Complete model estimates and coefficient matrices are reported in Supplementary Tables S3–S4.

Stunting affected 131/362 children (36.2%) in food-transfer households and 44/161 children (27.3%) in e-voucher households. The crude OR was 0.663 (95% CI 0.443 to 0.992; $p = 0.046$). The estimate remained inverse after conditioning on the demographic, socioeconomic-resource, and household-vulnerability sets. The OR was 0.782 after adding the displacement and institutional set and 0.766 in the combined specification (95% CI 0.454 to 1.292; $p = 0.317$). These changes do not determine whether the added variables were confounders, mediators, colliders, or proxies for camp context.

4.3 Weight-for-height and wasting

The WHZ estimates were close to the null and imprecise in both the crude specification ($\beta = -0.043$, 95% CI -0.219 to 0.134 ; $p = 0.636$) and the combined structural-context specification ($\beta = -0.047$, 95% CI -0.274 to 0.180 ; $p = 0.686$). Among children with valid outcomes, wasting affected 58/360 (16.1%) in food-transfer households and 24/156 (15.4%) in e-voucher households. The crude wasting OR was 0.947 (95% CI 0.567 to 1.581; $p = 0.834$), and the combined-specification OR was 0.971 (95% CI 0.477 to 1.977; $p = 0.935$). These intervals were compatible with small harmful and protective associations.

Table 3A. Selected household-clustered modality–outcome estimates

Outcome	Model	Effect	95% CI	p value
HAZ	M1	0.280	0.015 to 0.545	0.038
HAZ	M2	0.292	0.028 to 0.555	0.030
HAZ	M3C	0.215	-0.105 to 0.536	0.188
HAZ	M4	0.209	-0.147 to 0.565	0.251
Stunting	M1	0.663	0.443 to 0.992	0.046
Stunting	M2	0.635	0.418 to 0.964	0.033

Outcome	Model	Effect	95% CI	p value
Stunting	M3C	0.782	0.479 to 1.278	0.327
Stunting	M4	0.766	0.454 to 1.292	0.317
WHZ	M1	−0.043	−0.219 to 0.134	0.636
WHZ	M2	−0.059	−0.240 to 0.121	0.517
WHZ	M3C	−0.098	−0.314 to 0.119	0.377
WHZ	M4	−0.047	−0.274 to 0.180	0.686
Wasting	M1	0.947	0.567 to 1.581	0.834
Wasting	M2	0.929	0.551 to 1.564	0.781
Wasting	M3C	0.991	0.511 to 1.924	0.979
Wasting	M4	0.971	0.477 to 1.977	0.935

Table 3B. Camp-sensitive inference for the combined specification (M4)

Outcome	M4 effect	Camp-clustered 95% CI	Camp-clustered p value	Restricted score p value
HAZ	0.209	−0.190 to 0.607	0.293	0.352
Stunting	0.766	0.460 to 1.274	0.293	0.309
WHZ	−0.047	−0.306 to 0.213	0.715	0.695
Wasting	0.971	0.429 to 2.198	0.942	0.942

Note. HAZ is the main continuous outcome; stunting, WHZ, and wasting are secondary outcomes. Effects are OLS coefficients for HAZ and WHZ and odds ratios for stunting and wasting. Models are descriptive conditioning sets, not causal adjustment stages. Table 3A reports household-clustered inference. Table 3B reports M4 camp-clustered confidence intervals and p values using 30 degrees of freedom, plus restricted score p values based on 99,999 Rademacher draws.

4.4 Geographic and camp sensitivity analyses

Camp-level inference further characterised uncertainty arising from the geographic concentration of assistance modality. Under raw-distance M4, HAZ was 0.209 (camp-clustered 95% CI −0.190 to 0.607; $t(30)$ $p = 0.293$; restricted wild cluster score $p = 0.352$), stunting OR was 0.766 (95% CI 0.460 to 1.274; $t(30)$ $p = 0.293$; score $p = 0.309$), WHZ was −0.047 (95% CI −0.306 to 0.213; $t(30)$ $p = 0.715$; score $p = 0.695$), and wasting OR was 0.971 (95% CI 0.429 to 2.198; $t(30)$ $p = 0.942$; score $p = 0.942$). Raw- and log-distance estimates were similar. These procedures quantify uncertainty but do not create within-camp exposure variation where little or none exists. Additional support, mixed-camp, distance-bootstrap, small-camp, and model-stability results are reported in Supplementary Tables S5–S15.

4.5 Diagnostic findings

All non-intercept variance inflation factors were below 3.0, indicating no severe linear multicollinearity in the combined structural-context design matrix. Nonetheless, registered-camp status correlated strongly with refugee-arrival group ($r = 0.723$) and clinic distance ($r = 0.657$), while modality correlated with clinic distance ($r = 0.560$). These diagnostics address linear dependence but do not resolve restricted covariate overlap, uncertain causal roles, or limited within-camp exposure variation. Influence diagnostics identified a minority of observations above conventional Cook's-distance or leverage screening thresholds, but maximum Cook's distances were small; sensitivity across camp specifications was therefore more important than any single influential record.

5. Hoddinott J, Dorosh P, Filipski M, Rosenbach G, Tiburcio E. Food transfers, electronic food vouchers and child nutritional status among Rohingya children living in Bangladesh. PLoS One. 2020;15(4):e0230457. doi:10.1371/journal.pone.0230457.

5.1 Principal interpretation

The central result is not a definitive benefit or absence of benefit from e-vouchers. Favourable HAZ and stunting estimates persisted in direction, but their magnitude and precision depended on conditioning and geographic structure. The released extract therefore does not support a precise or causally interpretable modality estimate. Its strongest empirical contribution is showing that an apparently household-level programme comparison was embedded in markedly different camp, administrative, and distance contexts.

The domain decomposition is more informative than an unsupported composite index. Socioeconomic-resource and household-vulnerability blocks did not materially attenuate the crude chronic-nutrition associations. The largest change occurred after adjustment for refugee-arrival group and registered-camp status, with additional imprecision under geographic adjustment. These variables may encode duration of settlement, programme allocation, livelihood opportunity, service networks, and other unmeasured camp conditions. Because they were measured contemporaneously and may proxy several processes, their inclusion cannot isolate a causal pathway.

5.2 Structural context and measurement limitations

Structural context remains relevant, but empirical claims must match the released measurements. The child-level file represents selected socioeconomic, institutional, housing, and geographic conditions. Clinic distance provides only partial information about physical accessibility, while availability, affordability, acceptability, and quality are not directly measured. Questionnaire items that were not present in the released child-level analytical file were not treated as observed data.

Exploratory distance specifications produced no consistent pattern across WHZ and wasting and are reported in the supplement without confirmatory interpretation. Recorded distance may not represent realised travel burden, affordability, service availability, or quality.

The divergence between minimally and combined structural-context estimates should be read as model dependence, not as proof that structural variables “explain away” a nutritional benefit. Registration and displacement history are associated with modality and may also encode differences in duration of settlement, housing, service networks, livelihood opportunities, and administrative inclusion. Clinic distance is correlated with those variables and may represent location more strongly than realised travel burden. Conditioning on these markers can reduce confounding, but it can also introduce instability when they are highly structured and incompletely measured.

The results should not be interpreted as a comparative validation of the complete source study. Hoddinott and colleagues analysed information not contained in the reduced public extract [5]. This analysis verifies what can be regenerated from the release and describes conditional estimates obtainable from it; it neither confirms nor invalidates the programme effect estimated from the complete source data.

A broader structural interpretation remains justified at the level of research design. Humanitarian nutrition programmes depend on functioning markets, safe infrastructure, service supply, affordability, information, and household agency. Cash or vouchers can ease economic constraints and improve food choice, but cannot by themselves guarantee accessible clinical care, safe water, or adequate sanitation [14,15]. Conversely, service improvements cannot substitute for sufficient food resources. The appropriate policy proposition is complementarity, which

requires direct evaluation rather than inference from the attenuation of one observational coefficient.

The audit provides a case-specific assessment of the public extract's secondary-use boundaries. The deposit is sufficient to verify selected counts and models but insufficient for new mechanism-based analyses requiring unreleased modules, merge keys, allocation variables, or survey-design metadata. Repositories should distinguish raw modules, constructed analytical files, restricted variables, merge keys, and design variables so secondary users can judge whether a proposed construct is estimable.

5.3 Implications

For humanitarian programme monitoring, the results support clearer documentation of allocation criteria, transfer value and frequency, market conditions, camp context, service access, sampling design, and stable linkage identifiers. These recommendations concern transparent and adaptive monitoring; sustainability itself was not measured as an outcome.

A stronger prospective evaluation would enrol children before or at modality assignment and repeatedly measure anthropometry, dietary intake, morbidity, care seeking, expenditure, market conditions, and service supply. Direct household- and facility-level healthcare-access indicators should be specified before testing access mechanisms. Household measures could include travel time, transport cost, treatment foregone because of cost or distance, language and information barriers, and reported respectful care. Facility measures could include staffing, opening hours, medicine availability, waiting time, referral capacity, and adherence to clinical standards.

For programme evaluation, qualitative evidence is also essential. Identical distances may represent different burdens for households with varying mobility, safety, caregiving responsibilities, or knowledge of services. Interviews and participatory mapping could establish which barriers are salient and whether administrative categories reflect lived accessibility. Such work should precede scale construction rather than use statistical factor extraction as a substitute for content validation [23–25].

5.4 Strengths and limitations

Strengths include direct verification of the released file; independent reconstruction of stunting and wasting; analysis of continuous and binary anthropometric outcomes; household-clustered

inference; explicit structural-domain decomposition; flexible geographic specifications; camp sensitivity analyses; and locked reproducibility outputs. The analysis also draws a transparent boundary between concepts documented in questionnaires and variables available for row-level empirical analysis.

Limitations are substantial. The cross-sectional design cannot establish temporality or causality. The reduced public file lacks illness, treatment seeking, water, sanitation, diet, household size, detailed allocation rules, survey weights, and multidimensional service-access measures. Residual confounding and measurement error are likely. Assistance modality showed limited overlap across institutional, geographic, and camp strata. The camp-indicator wasting model was not estimable because of sparse data. Clinic distance may function chiefly as a programme-geography marker rather than realised access. The analysis is therefore a transparent reassessment of the released file, not a complete evaluation of the source survey or a validated multidimensional healthcare-access assessment.

6. Office of the United Nations High Commissioner for Human Rights; World Health

Organization. The Right to Health: Fact Sheet No. 31. Geneva: United Nations; 2008.

Food-assistance modality in the released sample was strongly patterned by camp, administrative position, and distance. Favourable crude HAZ and stunting estimates remained positive or inverse across specifications, but their precision and interpretation were limited by restricted overlap, uncertain covariate roles, and incomplete design information. WHZ and wasting estimates were imprecise and compatible with the null. The extract supports independent descriptive reassessment, not a causal programme comparison. Its practical contribution is to show which findings can be reproduced and which data are required for credible secondary evaluation and adaptive humanitarian monitoring.

Declarations

Ethics approval and consent to participate: This secondary analysis used de-identified data available in a public repository and involved no direct participant contact. The source study reported IFPRI ethical approval and informed consent [5].

Consent for publication: Not applicable.

Data and code availability: The source child-level dataset is publicly available from Harvard Dataverse under DOI 10.7910/DVN/5BAN6C [10]. The complete reproducibility package for this reanalysis is available in Zenodo at <https://doi.org/10.5281/zenodo.22653254> [36]. The deposit contains the authoritative Python workflow, source-data checksum, software-version manifest, locked machine-readable outputs, output checksums, and complete supplementary workbook. The source data are not redistributed in the Zenodo deposit and remain subject to the original Harvard Dataverse terms. No new primary data were collected.

Competing interests: The author declares no competing interests.

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Author contributions: Nerob Ahmed: Conceptualization, Methodology, Formal analysis, Software, Validation, Data curation, Investigation, Visualization, Writing, original draft, and Writing, review and editing.

Prior dissemination: An earlier version was disseminated as a preprint and conference abstract under a related title. The present manuscript is a substantive correction and reanalysis. Unsupported claims involving a multidimensional healthcare-access index, underweight, survey weighting, multiple imputation, and mechanism-based interpretation were removed. A correction notice and the complete reproducibility materials are publicly available in the Zenodo record cited in Reference 36.

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