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IMPACTS OF INFORMAL HOUSING RELOCATION IN THE ZAATARI SYRIAN REFUGEE CAMP, JORDAN

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Informal shelter extensions and home-based micro-enterprises in protracted refugee camps often operate as critical coping infrastructure, supporting livelihoods, comfort, privacy, and neighborhood-level social support. Relocation policies—implemented for decongestion, infrastructure upgrading, risk reduction, or administrative regularization—may improve physical conditions while simultaneously disrupting access to services and the proximity-based networks that sustain daily life. Yet rigorous causal evidence remains limited because relocation is rarely random, service landscapes evolve, and humanitarian data are sensitive. We present a mixed-methods quasi-experimental framework to estimate the multidimensional impacts of relocation in the Zaatari Syrian Refugee Camp, integrating (i) a stratified panel household survey (two or more waves bracketing relocation), (ii) privacy-protected spatial accessibility indices derived from pre/post locations and a walkable network, and (iii) administrative relocation timing and service-change metadata where accessible. We specify difference-in-differences estimators with event-study diagnostics, robustness stress tests, and sensitivity analyses for social-network disruption metrics. Because empirical microdata in humanitarian settings are typically restricted, we include an end-to-end, fully reproducible synthetic validation package (explicitly labeled as synthetic) that mirrors realistic non-random targeting and staggered relocation timing and produces publication-grade tables and figures. The framework supports policy-relevant recommendations, including service-synchronized and cluster-preserving moves, access-loss minimization using accessibility constraints, and targeted transitional support for vulnerable and livelihood-dependent households.

Keywords: refugee camp; informal housing; relocation; humanitarian planning; Zaatari; spatial accessibility; social networks; livelihoods; wellbeing; displacement; shelter policy; Jordan

INTRODUCTION

Refugee camps are frequently conceived as temporary emergency responses, yet many persist for years and develop durable socio-spatial structures that resemble urban neighbourhoods in both function and form (Dalal, 2022; Patchett, 2022; UNHCR, 2022). As displacement becomes protracted, households and communities must solve day-to-day problems of shelter adequacy, mobility, and income generation under binding constraints on land, materials, and services (Handbook, S., 2011; Sharma, 2022). A growing interdisciplinary literature therefore treats camps not only as planned humanitarian infrastructures but also as lived environments in which residents actively reshape space to sustain dignity, welfare, and agency (Dalal, 2022; Ledwith, 2014; Mouris Hanna, 2021).

Within this perspective, informality is not merely a deviation from plan; it often functions as a pragmatic complement to formal provision. Resident-led shelter extensions, partitions, shading, and insulation can improve thermal comfort, privacy, and household productivity, while home-based commerce and micro-enterprises create locally embedded service and employment ecosystems (Dalal, 2022; Ledwith, 2014; Mouris Hanna, 2021). In Zaatari specifically, the evolution from standardized emergency shelters toward incrementally adapted dwellings has been documented as a central mechanism through which households stabilize living conditions and negotiate space in a high-density setting (Dalal, 2022; Mouris Hanna, 2021). Parallel transformations occur in the camp economy: Zaatari has developed a well-known commercial corridor and extensive micro-retail activity, illustrating how livelihood systems become spatially concentrated and highly dependent on local accessibility and footfall (Turner, 2020; UNHCR, 2022).

Camp authorities nonetheless periodically implement relocation and reconfiguration policies—including plot reallocation, decongestion of corridors, and removal or redesign of informal structures—to enable infrastructure upgrades, reduce hazards, or regularize layouts (Handbook, S., 2011). Prior research on displacement and resettlement emphasizes that such moves can generate welfare losses not only through changes in physical conditions but also through disrupted access to economic opportunities and weakened social capital (Cernea, 2021; Putnam, 2000). These mechanisms are especially salient in camp contexts because routine activities (shopping, schooling, clinic visits, work, and mutual aid) are frequently organized around short-distance walking networks and repeated neighbourly interaction (Dalal, 2022; Ledwith, 2014). Consequently, even modest increases in travel time to key destinations may cumulate into non-trivial opportunity costs, while the dispersal of proximate ties can undermine informal childcare, risk-sharing, and psychosocial support (Cernea, 2021; Putnam, 2000).

Despite strong theoretical reasons to expect multi-domain impacts, credible quantitative evidence on the causal effects of *intra-camp* relocation and informal-housing reconfiguration remains limited. Empirical work on camp urbanization and informality has often been descriptive or qualitative, providing rich accounts of adaptation but offering fewer identification strategies that separate relocation effects from concurrent changes in services, governance, or population composition (Dalal, 2022; Patchett, 2022). At the same time, humanitarian evaluations face distinctive constraints—non-random program placement, mobility and attrition, overlapping interventions, and restricted access to administrative and spatial microdata—which complicate replication and external scrutiny (Gazi, 2020; Pulido, 2021; Sharma, 2022). Methodologically, staggered relocation timing further raises well-known identification problems for conventional two-way fixed-effects difference-in-differences and naive event-study specifications, motivating modern estimators and explicit diagnostic practice (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Sun & Abraham, 2021).

This paper addresses these gaps by developing and documenting a mixed-methods, quasi-experimental assessment of informal housing relocation in Zaatari that (i) quantifies changes in access, livelihood stability, safety perceptions, psychosocial wellbeing, and social-network continuity, and (ii) makes identification assumptions transparent through stress tests, sensitivity analyses, and falsification diagnostics. To reconcile evidentiary needs with data-responsibility requirements, the analysis uses privacy-preserving spatial measurement and provides a fully reproducible synthetic validation package that mirrors the end-to-end pipeline without exposing protected microdata (Gazi, 2020; Pulido, 2021).

Contributions.

1. *Identification under non-random, staggered moves:* We implement a transparent difference-in-differences design tailored to staggered relocation timing, reporting cohort-robust estimands, event-study diagnostics, and decompositions that avoid misleading two-way fixed-effects summaries (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Sun & Abraham, 2021).
2. *Robustness and credibility diagnostics:* We provide pre-trend and placebo tests, permutation-based inference, alternative window specifications, and sensitivity analysis that quantifies how conclusions change under bounded deviations from parallel trends (Angrist & Pischke, 2009; Roth & Rambachan, 2022).
3. *Privacy-preserving spatial accessibility evidence:* We translate protected pre/post locations into network-based walking-time indices to services and markets, reported only in aggregation-safe forms consistent with humanitarian data-responsibility guidance (Gazi, 2020; Pulido, 2021).
4. *Measurement of social disruption mechanisms:* We operationalize proximity-based network disruption using a neighbour-continuity index and complementary survey measures of cohesion and support proximity, with sensitivity to plausible neighbourhood radii (Cernea, 2021; Putnam, 2000).

THEORY OF CHANGE AND TESTABLE PREDICTIONS

Relocation within a protracted camp is a policy-induced reallocation of households over a fixed spatial substrate of services, economic activity, and social ties. Unlike voluntary residential mobility, relocation occurs in an environment with constrained transport options, walking-dominant travel, and highly localized informal markets, so spatial displacement can translate nonlinearly into time, cost, and welfare effects. We formalize exposure using a staggered adoption framework. Let i index households and t index survey months. Define the (possibly cohort-specific) relocation time $T_i \in \mathbb{Z} \cup \{\infty\}$ and treatment status

$$D_{it} = Ind\{t \geq T_i\}, \tag{1}$$

with $D_{it} = 0$ for never-relocated households ($T_i = \infty$). Let L_{it} denote the household's protected (privacy-preserving) location at time t and let Y_{it} denote an outcome in the domains of access, livelihood continuity, safety perceptions, wellbeing, or social cohesion. The theory of change posits that relocation affects Y_{it} primarily through two empirically measurable channels—*spatial access* and *social proximity*—which jointly govern households' effective opportunity sets and informal insurance capacity. These channels are not merely conceptual; they are designed to be operationally actionable: they define the constraints relocation planners can manipulate (destination assignment, timing, and service placement) and the intermediate quantities that can be monitored to prevent avoidable harm.

Channel 1: Spatial access as a constraint on opportunity and livelihood continuity

Relocation changes a household's position relative to essential destinations $s \in \mathscr{S}$ (markets, clinics, schools, distribution points, livelihood hubs). Let $\mathscr{G} = (\mathscr{V}, \mathscr{E})$ denote the walkable network and $\tau : \mathscr{E} \to \mathbb{R}_+$ edge traversal time. Define network travel time from the protected location L_{it} to destination s as

$$A_{it}(s) = \min_{p \in \mathscr{P}(L_{it},s)} \sum_{e \in p} \tau(e), \qquad A_{it}^{(k)} = \min_{s \in \mathscr{S}_k} A_{it}(s), \tag{2}$$

where $\mathscr{S}_k$ indexes a destination category k (e.g., markets or clinics). We focus on access losses $\Delta A_i^{(k)} = A_{i,\text{post}}^{(k)} - A_{i,\text{pre}}^{(k)}$ and corresponding cost burdens. In camp settings where economic activity and services concentrate along specific corridors, $\Delta A_i^{(k)}$ can generate nonlinear impacts because (i) walking time substitutes poorly for paid transport when mobility is constrained, (ii) trip frequency is endogenously reduced when time costs rise, and (iii) micro-enterprises depend on localized demand and supplier access. Consequently, increased $A_{it}^{(k)}$ can propagate to livelihood continuity through multiple mechanisms: reduced customer flow (lower sales volume), higher procurement costs (longer replenishment trips, more paid transport), and time reallocation away from income-generating activity toward mobility and caregiving. Formally, if q_{it} denotes trip frequency and c_{it} denotes monetary travel cost, then for routine destinations we expect q_{it} to be weakly decreasing and c_{it} to be weakly increasing in $A_{it}^{(k)}$, implying that the generalized cost of access $g(A_{it}^{(k)})$ enters livelihood production and household time allocation constraints. This channel motivates primary endpoints such as minutes-to-market, mobility expenditure, and an income or business continuity index.

Channel 2: Social proximity as informal insurance and psychosocial protection

Relocation also changes adjacency relations and therefore the feasibility of proximity-based social support. Let $\mathscr{N}_i^{\text{pre}}(r)$ be the set of households within radius r of i prior to relocation and $\mathscr{N}_i^{\text{post}}(r)$ the analogous set after relocation, constructed using protected locations. We quantify tie preservation with the neighbor continuity index

$$\text{NC}_i(r) = \frac{\left|\mathscr{N}_i^{\text{pre}}(r) \cap \mathscr{N}_i^{\text{post}}(r)\right|}{\left|\mathscr{N}_i^{\text{pre}}(r)\right| + \varepsilon}, \qquad \varepsilon > 0, \tag{3}$$

which measures the share of proximate ties retained, with ε preventing division by zero for sparse neighborhoods. We treat $\text{NC}_i(r)$ as a policy-relevant proxy for the stability of local mutual aid and informal insurance. A reduction in $\text{NC}_i(r)$ lowers the probability that support is available at low time cost, weakening childcare exchanges, informal lending, and rapid assistance in emergencies. These losses plausibly affect perceived cohesion and psychosocial wellbeing through (i) reduced frequency of reciprocal interaction, (ii) diminished perceived control and security, and (iii) increased stress associated with uncertainty and social isolation. Because the relevant spatial scale of social ties is uncertain and may differ across contexts, we pre-specify sensitivity reporting over radii $r \in \{30, 50, 80, 120, 200\}$ m and triangulate $\text{NC}_i(r)$ with survey measures of cohesion, mutual aid frequency, and support proximity.

Moderators, heterogeneity, and policy-relevant risk stratification

Relocation effects are expected to be heterogeneous because both channels interact with baseline constraints and dependence on locality. Let V_i denote a vulnerability indicator, Tenure_i duration in camp, and Liv_i livelihood type (micro-enterprise vs. non-enterprise). First, vulnerability increases reliance on proximate support and reduces substitutability of time and money for access, amplifying both $\Delta A_i^{(k)}$ and $\Delta\text{NC}_i(r)$ consequences. Second, longer tenure increases place-based embeddedness: routines, informal credit, and trust networks are more spatially anchored, increasing potential disruption from tie loss. Third, micro-enterprises depend disproportionately on localized demand and corridor proximity, increasing sensitivity to access loss. These moderators are not ancillary; they define a risk stratification scheme for operational decision-making and motivate ex ante targeting of mitigation resources.

Testable predictions and design-relevant questions

To discipline inference and connect evidence to implementation, we state directional hypotheses that map directly onto measurable channel quantities and downstream outcomes.

- *H1 (Access and economic continuity).* Relocation increases network travel time to routine destinations ($A_{it}^{(k)}$) and associated monthly mobility costs, and reduces short-run livelihood continuity (income index, business continuity, customer access). The magnitude of these effects is larger for micro-enterprise households and for households with high baseline dependence on corridor-based markets.
- *H2 (Network disruption and wellbeing).* Relocation reduces social proximity—lower $\mathrm{NC}_i(r)$ and lower cohesion and mutual-aid measures—and increases psychosocial stress. These effects are larger among vulnerable households and those with longer tenure, consistent with greater reliance on proximate informal insurance.
- *RQ3 (Mechanism-linked heterogeneity).* Which baseline characteristics $(V_i, \mathrm{Tenure}_i, \mathrm{Liv}_i, \text{informality intensity})$ predict adverse versus neutral outcomes, and to what extent are heterogeneous impacts statistically aligned with channel shifts $(\Delta A_i^{(k)}, \Delta \mathrm{NC}_i(r))$?
- *RQ4 (Mitigation as implementable constraints).* Which operational measures—service-synchronized moves, cluster-preserving destination assignment, phased timing, compensation and transitional livelihood support, and service re-siting—reduce $\Delta A_i^{(k)}$ and attenuate network disruption (higher $\mathrm{NC}_i(r)$ trajectories), thereby improving downstream outcomes?

STUDY DESIGN, DATA, AND MEASUREMENT

Design overview and integrated data architecture

The empirical study is a mixed-methods household panel designed to support credible quasi-experimental inference under staggered, non-random relocation. At minimum, two survey waves bracket the relocation window; however, the design is explicitly *multi-period* in spirit, because additional pre- and post-relocation waves substantially strengthen identification diagnostics (lead tests for parallel trends), enable dynamic treatment effect estimation, and reduce reliance on functional-form assumptions about time trends. The unit of analysis is the household, indexed by $i \in \{1, \ldots, m\}$, observed at survey month $t \in \{1, \ldots, T\}$.

The data architecture integrates four complementary components, each mapped to a distinct inferential role:

1. *Household panel survey.* A stratified panel measures outcomes and mediators across access/mobility, livelihood continuity, safety perceptions, psychosocial wellbeing, and social support. The survey provides the primary outcome series Y_{it} and key baseline covariates for adjustment, weighting, and heterogeneity analysis.
2. *Administrative relocation and service metadata.* Administrative records provide relocation timing T_i and origin/destination identifiers (block/district), enabling construction of exposure D_{it} and cohort assignment. Where feasible, service-change logs (facility openings/closures/relocations; infrastructure works) are compiled into a district-by-month ledger used for confounding assessment, restricted-sample analyses, and as controls in robustness specifications.
3. *Spatial layers and walkable network.* Geospatial layers of service/market locations and a walkable network $\mathcal{G} = (\mathcal{V}, \mathcal{E})$ support construction of privacy-preserving accessibility indices from protected household

locations L_{it}. These indices operationalize the access channel as measurable mediators and outcomes and provide policy-relevant targets for relocation design.

4. *Qualitative module (sequential explanatory).* Semi-structured interviews and focus groups are implemented after preliminary quantitative estimation to interrogate mechanisms, validate measurement constructs, and elicit feasible mitigation strategies. The qualitative protocol is purposively informed by quantitative effect patterns (e.g., high access loss, low neighbor continuity) to strengthen causal interpretation and policy relevance.

Sampling frame, field procedures, and statistical power

Households are selected using a stratified sampling frame defined by district/zone and relocation exposure (relocated during the study window vs. not relocated), with random selection implemented within each stratum from the most complete listing available (administrative rosters, enumerator-built listings, or updated household maps). Because vulnerability and livelihood dependence are theory-relevant moderators, we purposively oversample vulnerable households and, where feasible, households operating micro-enterprises so that subgroup estimates (and interactions) are sufficiently precise rather than being driven by small-cell noise. Sampling weights (including oversampling adjustments and any nonresponse corrections) are retained for population-representative estimands; unweighted analyses are reported only when the estimand is explicitly sample-specific, and we report weighted and unweighted results side-by-side when interpretation could change.

Given non-trivial mobility and survey fatigue in humanitarian contexts, panel maintenance is treated as a core design component, with operational protocols specified ex ante. At baseline we collect and verify multiple locator channels (phone, landmarks, secondary contacts) and, where appropriate, record safe-to-use tracking metadata (e.g., GPS at the dwelling with consent, neighborhood identifiers) to support recontact while protecting confidentiality. Field procedures include standardized contact attempts with minimum revisit thresholds, escalation rules for hard-to-find cases, and structured disposition codes that distinguish refusal, temporary absence, out-migration, and safety-related nonresponse. Enumerators are trained on consistent respondent identification, privacy-preserving interview practices, and neutral prompting; data collection is implemented via CAPI with built-in range checks and time stamps, complemented by back-checks and supervisor audits to reduce measurement error. Attrition is monitored by arm and baseline covariates in real time, and we report: (i) wave-by-wave retention/recontact rates and reasons for loss; (ii) differential attrition tests and balance among remainers; and (iii) sensitivity analyses using inverse-probability-of-attrition weights, alternative missingness scenarios, and bounding exercises when attrition is meaningfully differential.

Power calculations are anchored to pre-specified primary endpoints that map directly to theory-of-change channels and downstream welfare impacts: (i) minutes to market (network travel time) $A_{it}^{(\mathrm{mkt})}$; (ii) perceived cohesion; and (iii) psychosocial stress. Let $\widehat{\beta}$ denote the planned DiD estimand (TWFE benchmark with cohort-robust ATT estimators reported in the identification section) with cluster-robust inference at the household level. For a two-period DiD with household clustering, an approximate minimum detectable effect (MDE) is

$$\mathrm{MDE} \approx \left(z_{1-\alpha/2} + z_{1-\beta}\right) \cdot \sqrt{\mathrm{Var}(\widehat{\beta})}, \tag{4}$$

where $\mathrm{Var}(\widehat{\beta})$ is computed under the planned estimator using assumptions on within-household correlation, the realized covariate set, and expected attrition (including design effects induced by clustering and weighting). For multi-period panels, we compute $\mathrm{Var}(\widehat{\beta})$ under the full design matrix (accounting for staggered timing and clustering) and report detectable effects for pooled estimates and for pre-registered subgroup contrasts (including the oversampled vulnerability and micro-enterprise strata), with scenario-based power under conservative and optimistic attrition paths. To preserve inferential credibility, empirical deployment pre-registers primary endpoints, analysis windows, and heterogeneity dimensions, specifies a multiple-testing adjustment within outcome families, and documents any deviations from the pre-analysis plan.

Measurement framework and operational definitions

Table 1: Outcome domains, core constructs, and measurement sources.

Domain	**Constructs and measures (examples)**	**Source**
Spatial access	network time to nearest market/clinic/school; destination-specific access losses; trip frequency; mobility expenditures	GIS + survey
Livelihoods	income index; business continuity; customer access; procurement/supply constraints; time allocation	survey + interviews
Safety	perceived day/night safety; harassment concerns; lighting/security adequacy; safe-route perceptions	survey + interviews
Wellbeing	stress scale; life satisfaction; perceived stability/control; sleep disruption (optional)	survey
Social proximity	neighbor continuity; support proximity; mutual aid frequency; cohesion/trust scale	survey + GIS + interviews

Note. Instruments are culturally adapted, translated/back-translated, and piloted. Spatial outputs are privacy-preserving and reported only in aggregation-safe forms with small-cell suppression.

Let $\tau : \mathscr{E} \to \mathbb{R}_+$ be edge traversal time (minutes). For household i at time t with protected location L_{it} and destination set $\mathscr{S}_k$ for category k (markets, clinics, schools),

$$A_{it}^{(k)}(s) = \min_{p \in \mathscr{P}(L_{it},s)} \sum_{e \in p} \tau(e), \qquad A_{it}^{(k)} = \min_{s \in \mathscr{S}_k} A_{it}^{(k)}(s). \tag{5}$$

We emphasize two derived quantities: (i) *level access* $A_{it}^{(k)}$ as an interpretable outcome in minutes; and (ii) *access loss* $\Delta A_i^{(k)} = A_{i,\text{post}}^{(k)} - A_{i,\text{pre}}^{(k)}$ as a policy-relevant measure for relocation planning and privacy-preserving reporting. Mobility costs are decomposed into a monetary component (reported transport expenditure) and a time component (trip frequency $\times$ travel time). These components are reported separately to avoid embedding normative valuations of time, while enabling downstream policy analysis (e.g., scenario-based costing) in a transparent manner.

Social proximity is operationalized using complementary spatial and survey-based measures. Using protected locations, we compute neighbor continuity for radii $r \in \{30, 50, 80, 120, 200\}$ m:

$$\mathrm{NC}_i(r) = \frac{\left|\mathscr{N}_i^{\text{pre}}(r) \cap \mathscr{N}_i^{\text{post}}(r)\right|}{\left|\mathscr{N}_i^{\text{pre}}(r)\right| + \varepsilon}, \qquad \varepsilon > 0. \tag{6}$$

We pre-specify sensitivity reporting across r because the empirically relevant scale of proximate support varies by context and household type. Survey-based cohesion and mutual aid constructs are treated as latent; we report internal consistency (Cronbach's α and McDonald's ω) and, when item structure permits, factor-analytic checks for dimensionality and measurement invariance across waves and relocation status. Support proximity is measured directly via time/distance to key support persons (kin/trusted neighbours), providing triangulation against $\mathrm{NC}_i(r)$ and mitigating reliance on any single operationalization.

For outcomes such as "income index" or "livelihood continuity," we construct composites from pre-specified item sets using either (i) z-score standardization relative to the pre-relocation control distribution or (ii) a latent factor score when measurement properties justify it. All composite construction rules are fixed ex ante, documented in the reproducibility materials, and accompanied by item-level robustness checks to ensure that effects are not driven by a single component.

The theory of change motivates analysis of channel measures $(A_{it}^{(k)}, \mathrm{NC}_i(r))$ alongside downstream outcomes. We therefore pre-specify mediator-outcome associations (e.g., regressions of wellbeing on access loss and neighbor continuity) as *mechanism-consistent descriptives*. We explicitly avoid causal mediation claims unless stronger assumptions or designs (e.g., randomized mitigation components) are available; instead, we treat these associations as evidence for plausibility of mechanisms and as guides for policy simulation.

Ethics, privacy protection, and data responsibility

Empirical deployment requires institutional ethics approval, informed consent, and enumerator training on sensitive interviewing and referral protocols. Data minimization is applied to identifiers and geospatial information, with household locations protected via aggregation and/or calibrated jittering under a documented disclosure-risk assessment. Public outputs use district- or block-level aggregation, suppress small cells, and avoid identifiable maps. Data are stored in encrypted, access-controlled environments, and analysis is conducted in restricted workspaces consistent with humanitarian data responsibility guidance (Gazi, 2020; Pulido, 2021). Reproducibility materials are structured to separate (i) publicly shareable derived indices and synthetic validation data from (ii) restricted raw spatial and administrative inputs, enabling external verification of code paths without compromising participant safety.

EMPIRICAL STRATEGY AND IDENTIFICATION

Potential outcomes, treatment timing, and target estimands

Let $Y_{it}(d)$ denote the potential outcome for household i at time t under treatment status $d \in \{0,1\}$, where treatment corresponds to having been relocated by t. Relocation occurs at a household-specific time $T_i \in \mathbb{Z} \cup \{\infty\}$ and induces the observed treatment path $D_{it} = \mathbb{1}\{t \geq T_i\}$. We allow (i) staggered adoption, (ii) heterogeneous and dynamic impacts, and (iii) non-random relocation correlated with baseline characteristics and district-level plans.

Our primary causal estimand is the dynamic average treatment effect on the treated (ATT) at event time k:

$$\mathrm{ATT}(k) = \mathbb{E}[Y_{i,T_i+k}(1) - Y_{i,T_i+k}(0) \mid T_i < \infty], \qquad k \in \mathscr{K}, \tag{7}$$

where $\mathscr{K}$ includes pre-treatment leads and post-treatment lags. This estimand is policy-relevant because it characterizes the temporal profile of access losses and social disruption (e.g., short-run shocks with partial recovery versus persistent effects). We additionally report aggregated estimands—e.g., an average post-relocation ATT over a window $k \in \{0,\ldots,K\}$—to support operational planning, while retaining the event-time profile as the primary inferential object.

Identification conditions and diagnosing their plausibility

Identification of $\mathrm{ATT}(k)$ from observational panel data requires assumptions restricting how untreated outcomes would have evolved absent relocation. We employ a conditional parallel trends framework adapted to staggered adoption. Let G_i index relocation cohort (the value of T_i for treated units) and let W_i denote baseline covariates. A sufficient condition for identification is that, for each cohort g and each t prior to treatment, the mean evolution of untreated potential outcomes for cohort g matches that of an appropriate comparison group (not-yet-treated and/or never-treated), conditional on district structure and W_i:

$$\mathbb{E}[Y_{it}(0) - Y_{i,t-1}(0) \mid G_i = g, \mu(i), W_i] = \mathbb{E}[Y_{it}(0) - Y_{i,t-1}(0) \mid C_{it}(g) = 1, \mu(i), W_i], \tag{8}$$

where $\mu(i)$ denotes the household's district (or origin district) and $C_{it}(g)$ denotes membership in the cohort-appropriate control set (not-yet-treated at t and/or never-treated). This assumption is non-trivial in camp settings because relocation may coincide with service upgrades or hazard mitigation works. Accordingly, we treat identification as an empirical claim requiring systematic diagnostics rather than a default premise.

We evaluate plausibility of (8) using: (i) event-study *lead* coefficients (joint tests that pre-treatment effects are statistically indistinguishable from zero), (ii) pre-treatment fit and trend-slope comparisons by cohort and district, (iii) falsification outcomes and placebo timing, and (iv) sensitivity analysis that quantifies how large a deviation from parallel trends would be required to overturn core conclusions (Roth & Rambachan, 2022).

Benchmark TWFE specification and its interpretive limits

For continuity with prior literature and as a descriptive benchmark, we report a two-way fixed effects (TWFE) regression:

$$Y_{it} = \alpha_i + \lambda_t + \beta^{\text{TWFE}} D_{it} + \mathbf{X}_i^{\top}\gamma + \varepsilon_{it}, \tag{9}$$

where α_i are household fixed effects, λ_t are month fixed effects, and $\mathbf{X}_i$ contains pre-treatment covariates (included primarily for precision, given α_i). Standard errors are clustered at the household level, and we additionally report randomization-inference p-values for outcomes where the effective number of independent district-level shocks may be small.

Crucially, when treatment timing varies and effects are heterogeneous, β^{TWFE} is generally a non-convex weighted average of cohort-time effects and can place negative weight on some comparisons, leading to biased sign and magnitude interpretations (Goodman-Bacon, 2021). We therefore do *not* treat (9) as the primary causal estimator; instead, it serves as a benchmark whose divergence from cohort-robust estimates is itself informative about heterogeneity and timing structure.

Cohort-robust estimation and dynamic event studies

Primary causal estimates are obtained using group-time (cohort-time) DiD estimators that remain valid under heterogeneous treatment effects with staggered adoption (Callaway & Sant'Anna, 2021). Let $\widehat{\text{ATT}}_{g,t}$ denote the estimated ATT for cohort g at time t, constructed using a comparison group comprised of not-yet-treated and/or never-treated households. We report:

1. *Cohort-time effects* $\widehat{\text{ATT}}_{g,t}$ and their aggregation to overall $\widehat{\text{ATT}}(k)$ using pre-specified weights (e.g., cohort share weights).
2. *Event-study profiles* $\widehat{\delta}_k$ for $k \in \mathcal{K}$ with reference period $k = -1$ omitted, computed using robust event-study methods that avoid TWFE contamination under heterogeneous effects (Sun & Abraham, 2021).
3. *Inference and uncertainty* via cluster-robust standard errors and, where relevant, wild cluster bootstrap or permutation-based inference within districts to guard against over-rejection when the number of clusters is limited.

Event-time plots display both pre-treatment leads and post-treatment lags with confidence intervals, enabling direct scrutiny of parallel-trends plausibility and dynamic adjustment patterns (shock-and-recovery versus persistence).

Addressing selection into relocation and concurrent service changes

Relocation assignment is typically correlated with planning priorities, infrastructure corridors, and household characteristics. We address this through a layered strategy:

- *Design-stage comparability:* stratified sampling within districts and documentation of relocation rules or operational criteria when available.
- *Covariate adjustment and reweighting:* inverse-probability weighting (IPW) based on baseline covariates and origin-district indicators to improve balance and reduce sensitivity to compositional differences. We report stabilized weights, overlap diagnostics (effective sample size, weight distribution), and post-weighting balance.
- *Service-change controls and restrictions:* incorporation of a district-by-month service-change ledger as controls where measurement is reliable; and restricted-sample analyses excluding districts/time windows with major concurrent facility relocation or infrastructure disruptions.

Robustness, stress tests, and sensitivity analysis

To make identification transparent, we pre-specify and report a robustness suite targeting the dominant threats in camp settings:

1. *Differential trends and functional form:* district-specific linear (and, where justified, piecewise) trends; alternative time-window definitions; and leave-one-district-out analyses to assess influence.
2. *Negative controls and placebo designs:* placebo outcomes plausibly unaffected by relocation within the study horizon (e.g., household size) and placebo treatment dates assigned prior to actual relocation.
3. *Randomization inference:* within-district permutation of T_i (or re-labeling of treated units) to obtain finite-sample p-values robust to few-cluster concerns and to strengthen transparency of statistical significance.
4. *Sensitivity to parallel trends violations:* partially identified intervals under bounded deviations from parallel trends and reporting of the smallest deviation required to alter qualitative conclusions (Roth & Rambachan, 2022).
5. *Partial interference and spillovers:* buffer-based exclusions around relocation corridors and alternative definitions of control units to assess sensitivity to spillover exposure.
6. *Measurement error stress tests:* re-estimation under alternative privacy-protection parameters for spatial measures (e.g., aggregation levels/jitter radii), reporting whether conclusions are stable to plausible perturbations in $A_{it}^{(k)}$ and $\mathrm{NC}_i(r)$.

Together, these components implement an "identification-forward" workflow: the estimand is explicitly defined, the assumptions required for identification are stated and interrogated, and the empirical results are accompanied by diagnostic and sensitivity evidence that allows readers to assess credibility under realistic humanitarian data constraints.

SYNTHETIC VALIDATION: REPRODUCIBLE WORKFLOW WITHOUT SENSITIVE MICRO-DATA

Purpose and validation targets

All quantitative results in this section are *synthetic* and serve a specific scientific purpose: validating the complete workflow (measurement construction, estimation, diagnostics, and reporting) in a form that is fully reproducible without disclosing protected humanitarian microdata. The synthetic data generating process (DGP) induces (i) non-random targeting into relocation, (ii) staggered T_i, and (iii) correlated multi-domain outcomes with realistic effect heterogeneity. Empirical deployment replaces synthetic inputs with approved derived datasets while preserving identical estimation code paths and diagnostic outputs.

Pre-relocation comparability

Table 2: Baseline balance at the pre-relocation wave (synthetic validation).

Variable	Treated mean	Control mean	SMD
Vulnerable (0/1)	0.339	0.323	0.035
Tenure in camp (years)	2.431	2.417	0.008
Household size	6.079	5.893	0.083
Informal intensity (std)	0.774	0.646	0.120

Note. SMD denotes standardized mean difference. Balance checks are descriptive; empirical deployment reports overlap diagnostics and, when used, the impact of weighting on balance.

Primary estimates across outcome domains

Table 3: Primary two-way fixed effects DiD estimates (synthetic validation).

Outcome	ATE	SE	p-value	N
Minutes to market	6.875	0.134	0.000	8640
Monthly travel cost (JOD)	4.104	0.063	0.000	8640
Income index (std)	-0.338	0.015	0.000	8640
Cohesion (1–7)	-0.484	0.023	0.000	8640
Stress (0–16)	1.687	0.057	0.000	8640
Household size (placebo)	0.004	0.002	0.060	8640

Note. Household and month fixed effects; standard errors clustered by household. The placebo outcome illustrates falsification logic in the validation pipeline.

The synthetic estimates exhibit the expected sign pattern consistent with the theory of change: access worsens (higher A_{it}), monetary mobility costs rise, livelihood outcomes decline, and social/wellbeing indicators deteriorate. In empirical deployment, we additionally report cohort-robust ATT estimates and event-time profiles, treating TWFE as a benchmark rather than a definitive causal estimator in the presence of heterogeneous effects (Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Sun & Abraham, 2021).

Dynamic effects and parallel-trends diagnostics

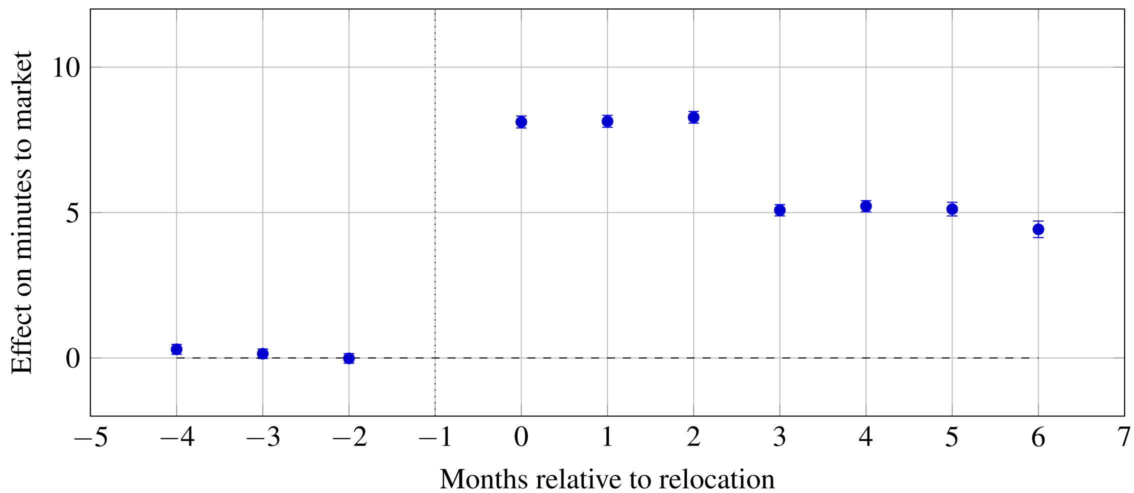

Figure 1: Event-study estimates for access: minutes to market (synthetic validation). The dotted line marks the omitted reference period ($k = -1$).

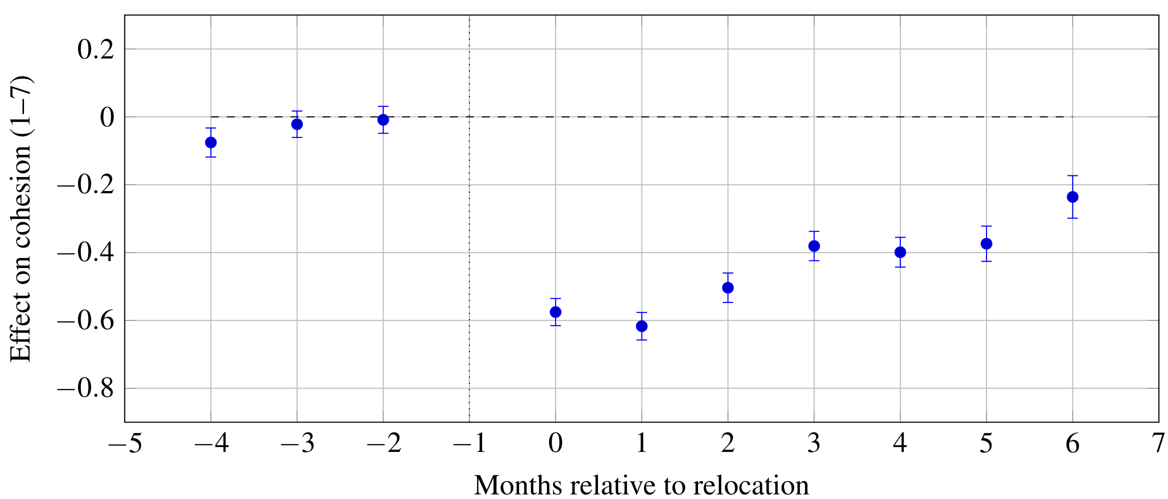

Figure 2: Event-study estimates for social proximity: cohesion (synthetic validation). The dotted line marks the omitted reference period ($k = -1$).

Diagnostics. In the synthetic validation, a joint Wald test of pre-treatment leads ($k = -4, -3, -2$) for market access does not reject parallel trends ($p = 0.203$), while a within-district permutation test ($B = 200$) yields strong evidence against a null of no effect for market access ($p = 0.005$). Empirical deployment reports the same diagnostics for each primary endpoint, with multiplicity control for families of outcomes when appropriate.

Heterogeneity consistent with channel dependence

Table 4: Heterogeneous effects by vulnerability and livelihood dependence (synthetic validation).

Outcome	Group	ATE	SE	*p*-value
Minutes to market	Vulnerable=1	8.289	0.210	0.000
Minutes to market	Vulnerable=0	6.153	0.153	0.000
Minutes to market	Microenterprise	9.318	0.196	0.000
Minutes to market	Non-microenterprise	5.679	0.130	0.000
Income index (std)	Vulnerable=1	-0.406	0.026	0.000
Income index (std)	Vulnerable=0	-0.303	0.019	0.000
Income index (std)	Microenterprise	-0.487	0.026	0.000
Income index (std)	Non-microenterprise	-0.263	0.018	0.000
Cohesion (1–7)	Vulnerable=1	-0.645	0.040	0.000
Cohesion (1–7)	Vulnerable=0	-0.401	0.028	0.000
Cohesion (1–7)	Microenterprise	-0.520	0.045	0.000
Cohesion (1–7)	Non-microenterprise	-0.475	0.027	0.000
Stress (0–16)	Vulnerable=1	2.210	0.086	0.000
Stress (0–16)	Vulnerable=0	1.419	0.067	0.000
Stress (0–16)	Microenterprise	1.663	0.104	0.000
Stress (0–16)	Non-microenterprise	1.696	0.068	0.000

Note. Subgroup estimates illustrate heterogeneity reporting in the reproducible pipeline. Empirical deployment pre-registers subgroup definitions and applies multiplicity adjustments when expanding subgroup searches.

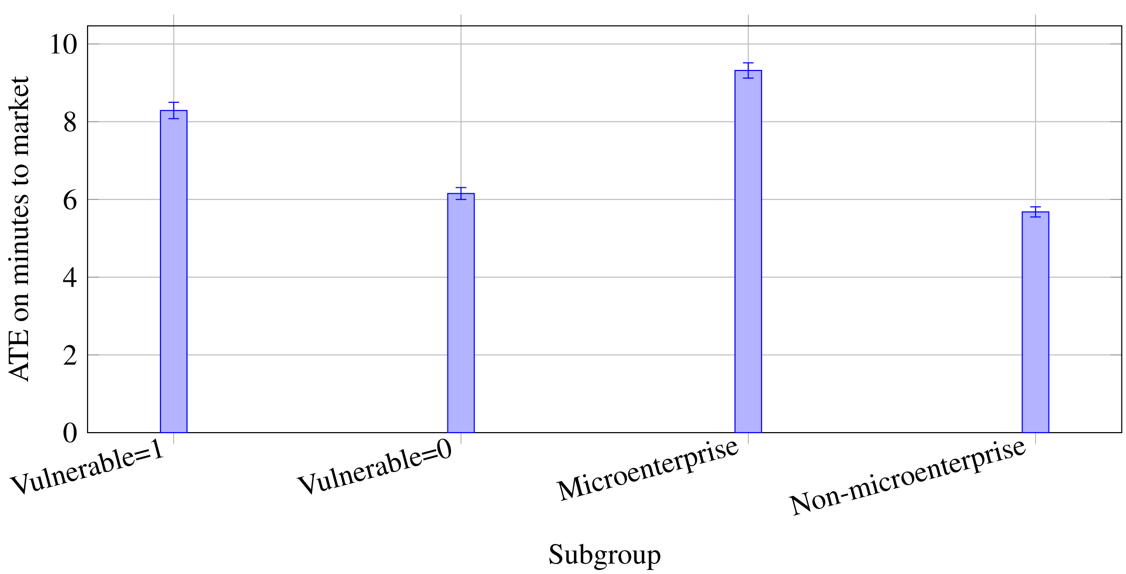

Figure 3: Heterogeneous relocation effects on market access by vulnerability and livelihood dependence (synthetic validation).

Robustness and stress tests

Table 5: Robustness and stress tests for key outcomes (synthetic validation).

Outcome	Specification	ATE	SE	*p*-value
Minutes to market	TWFE (unit+time FE)	6.875	0.134	0.000
Cohesion (1–7)	TWFE (unit+time FE)	-0.484	0.023	0.000
Stress (0–16)	TWFE (unit+time FE)	1.687	0.057	0.000
Minutes to market	TWFE + district trends	6.880	0.133	0.000
Cohesion (1–7)	TWFE + district trends	-0.483	0.023	0.000
Stress (0–16)	TWFE + district trends	1.696	0.057	0.000
Minutes to market	TWFE, districts<6	6.463	0.195	0.000
Cohesion (1–7)	TWFE, districts<6	-0.465	0.034	0.000
Stress (0–16)	TWFE, districts<6	1.668	0.080	0.000
Minutes to market	IPW-TWFE	6.756	0.133	0.000
Cohesion (1–7)	IPW-TWFE	-0.473	0.023	0.000
Stress (0–16)	IPW-TWFE	1.668	0.056	0.000

Note. "District trends" adds district-specific linear trends. IPW-TWFE uses stabilized inverse-probability weights estimated from pre-relocation covariates; empirical deployment reports overlap and weight diagnostics.

Sensitivity of network disruption to neighborhood definition

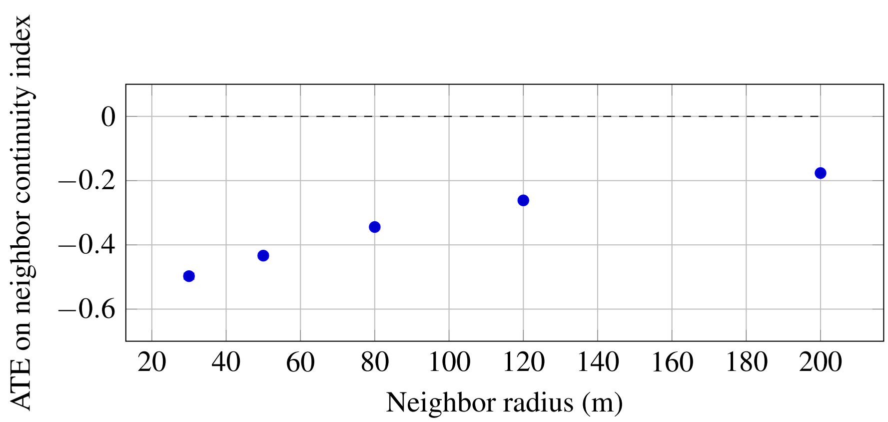

Figure 4: Sensitivity of social-network disruption estimates to the radius defining neighborhood ties (synthetic validation).

Table 6: Sensitivity of neighbor-continuity effects to the radius defining "neighbors" (synthetic validation).

Radius (m)	ATE	SE	*N*
30	-0.497	0.009	8640
50	-0.433	0.007	8640
80	-0.344	0.006	8640
120	-0.262	0.004	8640
200	-0.176	0.003	8640

Note. Neighbor continuity is an index in $[0, 1]$. More negative effects indicate larger loss of proximate ties after relocation.

Privacy-preserving spatial reporting template

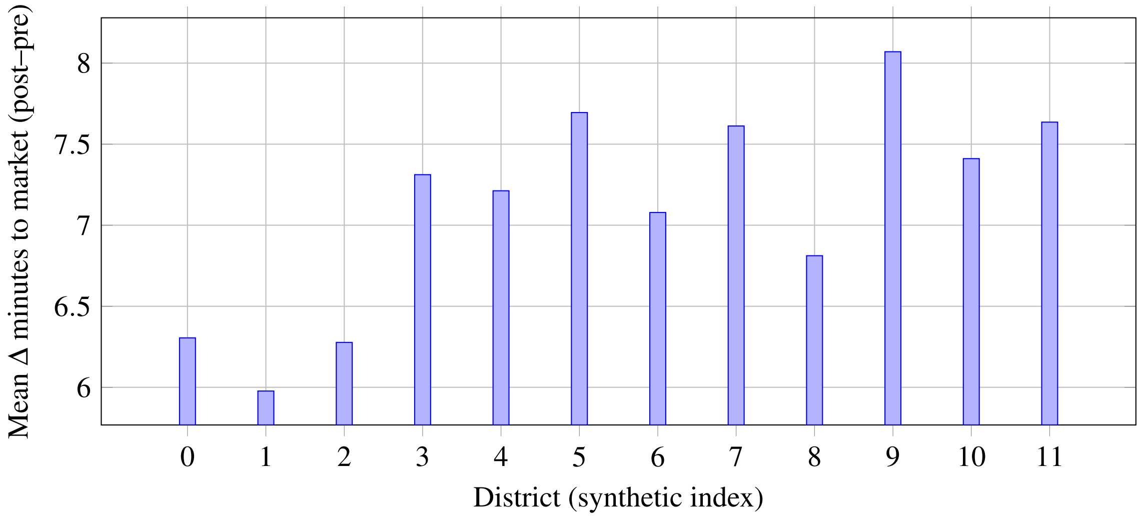

Figure 5: District-level average access change among relocated households (synthetic validation). Values are aggregated to district level to mimic privacy-preserving reporting.

MECHANISMS, MIXED-METHODS INTEGRATION, AND POLICY DESIGN

The research design treats quantitative estimation and qualitative inquiry as integrated components of a single inferential pipeline aimed at both explanation and actionability. Quantitative models establish *whether*, *how much*, and *for whom* outcomes change, distinguishing mean effects from distributional impacts across baseline vulnerability, livelihood dependence, and relocation exposure, and identifying which channels move alongside headline outcomes. However, reduced-form patterns often admit multiple plausible mechanisms (e.g., access loss versus livelihood reconfiguration, network disruption versus procedural stress), and these mechanisms imply different mitigation levers. We therefore implement the qualitative module as a sequential explanatory design that is explicitly keyed to the quantitative results: after estimating wave-specific and pooled effect patterns, we purposively sample households for follow-up interviews and focus groups using pre-specified "contrast case" criteria (e.g., high ΔA_i with stable income, low ΔA_i with large stress increases, low $\mathrm{NC}_i(r)$ with rapid recovery in cohesion, or persistent livelihood loss despite modest access changes). Sampling is balanced across treatment arms, districts/zones, and baseline risk strata to avoid narratively over-representing any single pathway. Interview protocols are organized around a priori mechanism families—service access frictions and travel-time shocks; livelihood displacement and client/supplier loss; social-network fragmentation and mutual aid; perceived procedural fairness and uncertainty; and adaptation costs and coping strategies—with standardized probes that map directly to measured channel variables. Qualitative themes are then triangulated against channel measures (mobility costs, service utilization, network/contact proxies, enterprise activity) and administrative service-change logs to assess concordance, surface competing explanations, and identify operational constraints (information, timing, safety, capacity) that govern whether and how mitigation can be implemented. Integration is operationalized through an evidence-to-design matrix that links estimated effect heterogeneity to mechanism adjudication and to concrete policy levers with measurable implementation indicators.

A “minimum-harm” relocation toolkit as testable design constraints

We translate the integrated evidence into a “minimum-harm” relocation toolkit defined as implementable constraints on planning and execution that can be empirically evaluated using the same outcome and channel measures used for estimation. Each element is specified as (i) an operational rule, (ii) a compliance/process indicator, and (iii) an outcome-monitoring target with pre-registered thresholds that can trigger corrective action:

1. *Service-synchronized moves:* relocate only when destination areas satisfy minimum service parity for core services (e.g., water, sanitation, primary health access, schooling, and safe transport), defined ex ante and verified with a standardized checklist; where parity is infeasible, deploy time-bound bridging services and monitor ΔA_i at high frequency during the transition window to detect acute access losses and enable rapid response.
2. *Access-loss minimization:* incorporate explicit accessibility constraints into destination assignment to minimize expected ΔA_i for essential services and livelihood hubs, with prioritization rules for micro-enterprises and vulnerable households; evaluate both average and tail access losses, and track the share of placements that violate the pre-specified accessibility constraints.
3. *Cluster-preserving allocation:* relocate pre-existing social clusters together (e.g., extended-family groups, savings groups, childcare/mutual-aid ties) to preserve functional support networks; operationalize clusters using baseline network mapping where available and evaluate using $\mathrm{NC}_i(r)$, cohesion trajectories, and qualitative confirmation that preserved ties remain active after relocation.
4. *Targeted transition support:* provide time-limited, exposure-indexed assistance (transport vouchers, transitional market space, re-siting support, and administrative facilitation) for households with high predicted livelihood exposure; evaluate using livelihood continuity indicators (enterprise operation, client retention, hours worked) and mobility-cost outcomes over both short-run disruption and medium-run recovery horizons.
5. *Transparent process and grievance handling:* publish criteria, timelines, and assignment logic in accessible formats and implement a grievance channel with documented service standards (acknowledgment, resolution timelines, escalation); evaluate perceived fairness and stress as monitored outcomes and track grievance volume, resolution time, and substantiation rates as operational performance metrics and early-warning indicators.

LIMITATIONS AND EMPIRICAL IMPLEMENTATION ROADMAP

The synthetic validation demonstrates computational feasibility and reproducibility, but it cannot replace empirical inference; field deployment must therefore confront operational threats that are structurally common in camp settings. First, *concurrent interventions* (e.g., shifting service provision, cash programs, infrastructure repairs) can confound relocation effects, motivating continuous district-by-month service availability logs, restricted samples around stable service periods, and explicit controls for measured co-interventions. Second, *partial interference* is plausible because relocations can change congestion, prices, or social ties for nearby non-relocated households; we address this through spatial buffer and exposure analyses and, where feasible, cluster-level designs that define treatment at an appropriate neighborhood unit and enable inference under interference-aware assumptions. Third, *attrition and mobility* may bias panel estimates in high-movement contexts, so we report wave-by-wave retention, test for differential attrition by arm and baseline covariates, and implement inverse-probability weighting and bounding exercises to assess robustness when loss is meaningfully differential. Fourth, *measurement and reporting constraints* require privacy-preserving dissemination and careful disclosure control; we therefore publish only aggregation-safe outputs (e.g., district/block summaries with small-cell suppression), avoid identifiable maps, and align release practices with humanitarian data responsibility guidance (Gazi, 2020; Pulido, 2021). Finally, to preserve inferential credibility, we pre-register primary endpoints, analysis windows, and heterogeneity dimensions, and we label any post hoc specifications and exploratory analyses as explicitly exploratory.

CONCLUSION

Relocation in protracted displacement settings is not merely logistical; it reshapes spatial access and social proximity—the key infrastructures through which households sustain livelihoods, safety, and psychosocial stability. This paper offers a technically explicit, ethically implementable framework for estimating relocation impacts in Zaatari using privacy-preserving spatial measurement, cohort-robust difference-in-differences estimands, and a diagnostic suite tailored to staggered adoption, selection, and concurrent change. By tying mitigation design to measurable channel targets (A_{it} and $\mathrm{NC}_i(r)$), the approach enables evidence-informed relocation planning that reduces avoidable harm while remaining operationally feasible.

DATA AVAILABILITY

This single-file manuscript embeds all synthetic tables and figures. For empirical deployment, share only de-identified derived indices and aggregation-safe outputs; raw household locations and administrative records should remain restricted due to re-identification risks.

ETHICS STATEMENT

Empirical deployment requires ethics approval and adherence to humanitarian data responsibility principles. Informed consent is obtained from all participants, and dissemination avoids re-identification risk.

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