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Targeting Effectiveness and Multidimensional Poverty in a Conflict Setting: Evidence from Conditional Cash Transfer Beneficiary Households in Northern Senatorial District, Plateau State, Nigeria

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Abstract
Conditional cash transfer (CCT) programmes can no doubt deliver significant poverty reduction, improve health and education gains; however, their impact is largely shaped by the systems that decide who qualifies to benefit. This study interrogates whether beneficiaries' own assessment of targeting effectiveness is associated with multidimensional poverty among Household Uplifting Programme (HUP-CCT) beneficiaries in the Northern Senatorial District of Plateau State, Nigeria. A convergent parallel mixed-methods design was used, anchored in Social Protection Theory, with Adaptive Social Protection serving as a supplementary lens for interpreting targeting under changing vulnerability. Survey data comprised 353 clean cases for descriptive analysis, of which 212 complete cases were retained for hierarchical multiple regression (Model 1: $R^2 = .098$, $F(4, 207) \approx 5.64$, $p < .001$; Model 2: $R^2 = .114$, $\Delta R^2 = .016$, F-change$(3, 204) = 1.23$, $p = .299$). Nine key informants, purposively selected across five categories of programme actor, supplied complementary qualitative evidence. Beneficiaries rated the targeting process favourably overall ($M = 3.66$), especially on identifying genuinely poor households and on the transparency and fairness of selection. Despite this, targeting effectiveness showed no statistically significant association with the household multidimensional poverty score ($\beta = .019$, $SE = .018$, $t = .273$, $p = .785$) once age, educational attainment, household size and duration of programme participation were controlled for. The qualitative evidence points to registry obsolescence after displacement, constraints on field verification, and the possibility of local influence as plausible, though untested, mechanisms that may weaken the link between initial targeting decisions and later household conditions. The findings suggest that perceived targeting quality and measurable multidimensional poverty outcomes are simply different things. In conflict-affected settings, targeting is better understood as a continuing process of registry maintenance, verification and institutional oversight than as a one-time act of enrolment.

Keywords: targeting effectiveness; conditional cash transfers; multidimensional poverty; social protection; National Social Register; Plateau State; Nigeria

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1. Introduction

Conditional cash transfers (CCTs) combine income support with conditions meant to encourage investment in education, health and human capital. Evidence from low- and middle-income countries shows that cash transfers can improve consumption, education, health and other welfare outcomes, though the size and durability of these effects vary considerably across settings (Bastagli et al., 2019; Fiszbein & Schady, 2009). How much of that potential is realised, however, depends partly on whether a programme can actually identify and reach the households it is meant to serve.

Targeting effectiveness exactly addresses this: the extent to which programme resources reach intended populations while limiting inclusion errors, where relatively better-off households receive benefits, and exclusion errors, where eligible households are left out (Coady et al., 2004). Targeting constitutes a pivotal allocative mechanism rather than a mere administrative formality; its efficacy significantly shapes the distributive equity and impact of social protection programmes.

In Nigeria, this is not a hypothetical concern. The Household Uplifting Programme's underlying National Social Register has expanded rapidly and now covers tens of millions of households nationally (Okorie, 2026), a scale that magnifies the operational challenge of keeping beneficiary information current. Recent Nigerian scholarship documents welfare gains from CCT programmes alongside persistent implementation problems that bear directly on targeting: Ogwola et al. (2024) identify tensions between social-safety-net policy objectives and implementation realities, and El-Yaqub et al. (2025) report gaps between programme design and operational delivery. In Plateau State specifically, Dati (2025) finds poverty-reduction and empowerment potential alongside constraints limiting programme performance, while Maikasuwa et al. (2025), comparing Nigeria and Brazil, show that programme outcomes depend on targeting and institutional coordination as much as on the transfer itself. None of this literature, however, directly tests whether beneficiaries' assessment of targeting quality is statistically associated with an independently measured multidimensional poverty outcome at the household level in a conflict-affected subnational setting; the specific question this study addresses.

Community-based targeting can draw on local knowledge that external administrators simply do not have, while proxy means testing and administrative verification offer additional ways of assessing household eligibility. Evidence reviewed by Coady et al. (2004) nonetheless shows considerable variation in targeting performance across settings, and stresses that implementation conditions are often what separates a targeting method that works from one that does not.

More recent evidence points in the same direction, while making the limitations of targeting more visible. Salifu and Kufoalor (2024), reviewing cash-transfer programmes across Sub-Saharan Africa, find that targeting is often only moderately effective, with undercoverage and leakage linked not only to the choice of targeting mechanism but also to political interference, corruption and operational constraints. The Nigerian evidence is similarly attentive to implementation. Akinola (2017), examining community-based targeting in Nigeria's earlier COPE programme, shows why local participation has to be considered alongside questions of accountability and institutional control. These studies reinforce that targeting is not simply a technical choice of method; its performance depends on how the system is administered and maintained.

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This issue becomes particularly important where poverty is dynamic and household circumstances are shaped by conflict, displacement and livelihood disruption. Adaptive Social Protection emphasises the capacity of social-protection systems to respond to shocks and changing patterns of vulnerability (Bowen et al., 2020). In such environments, beneficiary information can lose its representativeness as households move, livelihoods change and communities become harder to reach. Ibanga and Nanbyet (2026), studying farmer-herder conflict in Langtang South LGA, document exactly this kind of disruption to livelihoods, displacement and access to services elsewhere in Plateau State; a recent picture of the security conditions within which registries like this one have to be maintained.

The Northern Senatorial District of Plateau State provides a suitable platform to examine this problem. The study is guided by Social Protection Theory and uses Adaptive Social Protection as a supplementary framework for understanding how insecurity and shifting vulnerability might affect the continuity of targeting and beneficiary status. Beneficiary households here operate in an environment marked by insecurity, displacement and disrupted access to livelihoods and services. The central finding is, on its face, an unexpected one: although respondents rate the targeting process favourably, targeting effectiveness shows no statistically significant association with the multidimensional poverty score.

That finding constitutes the main puzzle this work addresses. It does not mean targeting is unimportant, or that the programme's targeting architecture has failed. It raises a narrower question instead: why might a targeting process that beneficiaries experience as fair, transparent and effective show no statistically detectable relationship with the multidimensional poverty outcome? The study draws on qualitative evidence to explore this gap, while keeping a clear line between what the data actually show and what remain plausible, untested explanations for it.

1.1 Objective, Research Question and Hypothesis

The study's specific objective is to examine whether beneficiaries' assessment of targeting effectiveness is associated with household multidimensional poverty among CCT beneficiary households in the Northern Senatorial District of Plateau State, net of household demographic characteristics and duration of programme participation. This translates into the following research question: is targeting effectiveness, as assessed by beneficiaries, statistically associated with the household multidimensional poverty score once age, educational attainment, household size and duration of participation are controlled for? The corresponding null hypothesis, phrased in the association-based terms that a cross-sectional design actually supports, is:

H0: Targeting effectiveness has no statistically significant association with household multidimensional poverty scores among CCT beneficiary households in the Northern Senatorial District of Plateau State.

2. Theoretical Framework and Literature Review

2.1 Social Protection Theory and the Place of Targeting

The study is anchored in Social Protection Theory, and in particular its conception of social protection as encompassing protective, preventive, promotive and transformative functions (Devereux & Sabates-Wheeler, 2004). Targeting matters within this framework because it determines who actually gets access to protective and promotive resources. A household wrongly

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excluded loses the chance to benefit from the intervention at all; a household wrongly included, meanwhile, draws down resources that would otherwise reach households with greater need.

That theoretical relationship is not unconditional, though, and this study draws three propositions from it. Proposition 1 (P1): correctly identifying an eligible household is necessary but not sufficient for poverty reduction, since the household still has to receive the transfer, remain able to access it, and have enough access to complementary services and opportunities for the transfer to move the needle on the dimensions of deprivation measured here. Proposition 2 (P2): because targeting accuracy is fixed at one point in time while multidimensional poverty is measured at the time of the survey, the strength of any observed association between the two hinges on how much household circumstances have shifted in between. Proposition 3 (P3), drawn from Adaptive Social Protection below: in conflict-affected settings that intervening shift is likely to be larger, which should weaken the targeting–poverty association relative to more stable settings. The distinction P1 and P2 draw, between eligibility, programme exposure and welfare outcome, turns out to matter a great deal for interpreting the null association reported later in this article.

2.2 Adaptive Social Protection and Changing Vulnerability

Adaptive Social Protection extends the conventional analysis by focusing on resilience to covariate shocks and a system's capacity to adjust as conditions change (Bowen et al., 2020). Its relevance to targeting comes down to how unstable household circumstances can be in shock-affected settings: displacement changes household location and composition, insecurity disrupts livelihoods and access to services, and economic shocks alter household vulnerability outright. A register that accurately reflected circumstances at one point can become steadily less representative as those conditions change; the mechanism P3 above specified.

This framework does not replace Social Protection Theory here; it supplements it, offering a lens for understanding why insecurity and changing vulnerability might weaken the relationship between targeting decisions made at one time and poverty outcomes observed at another.

2.3 Targeting in Practice: Design, Accuracy and Implementation

The targeting literature draws a useful line between the choice of targeting method and the quality of its implementation. Coady et al. (2004) show that no single targeting method performs uniformly well across contexts, and that implementation is often the decisive factor in how well targeting actually works. Community-based approaches can draw on local information, but they are also vulnerable to selection error where accountability and verification mechanisms are weak.

Nigerian evidence adds real weight to this general pattern. Ogwola et al. (2024) document a persistent gap between social-safety-net policy design and what happens on the ground, and El-Yaqub et al. (2025) similarly find that delivery does not always match stated design. Okorie (2026) reports that Nigeria's National Social Register has grown to cover tens of millions of households; a scale that inherently strains the continuous verification accurate targeting depends on. In Plateau State specifically, Oyewopo et al. (2026) find a gendered paradox within the HUP-CCT programme in Angwan Rogo, Jos North: women made up the majority of beneficiaries yet had marginally less favourable financial outcomes, which is itself consistent with the idea that correct initial enrolment does not guarantee uniform downstream welfare results. Okoli (2024, 2025) reports comparable heterogeneity among women beneficiaries nationally. However, none of this

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literature isolates targeting effectiveness as an independent predictor of multidimensional poverty in a regression framework, which is the specific contribution of the present study.

The implication for this study is that a positive beneficiary assessment of the targeting process should not automatically be read as evidence of objectively accurate targeting. The perception of fairness and transparency capture something real and important about programme legitimacy, but they are not the same thing as an independent measure of inclusion and exclusion errors.

There is also evidence that the target itself may be multidimensional. Crespo (2020), using Chilean administrative data, shows that a proxy means test may identify one priority group reasonably well while overlooking another important programme objective, such as the risk of school dropout. This is considered relevant here because a beneficiary can be correctly identified as vulnerable without the targeting system necessarily capturing every dimension of vulnerability that later shapes household welfare. Puorideme (2020) adds a different caution from Ghana, showing that the operation of a proxy means test is mediated by institutional practices and local social relations rather than simply applied as a neutral technical instrument. Collectively, these studies suggest that targeting should be examined not only as a question of who enters a programme, but also as a process shaped by multiple objectives, administrative practice and the social setting in which eligibility decisions are made.

3. Research Methodology and Empirical Specification

3.1 Research Design, Population and Sample

The study used a convergent parallel mixed-methods design, in which quantitative and qualitative evidence were collected and analysed as complementary sources rather than sequential ones (Creswell & Creswell, 2018). The quantitative component covered Barkin Ladi, Bassa, Jos East, Jos North, Jos South and Riyom, the six Local Government Areas (LGAs) making up the Northern Senatorial District of Plateau State.

The target population comprised 42,894 beneficiary households in the active 2023–2025 CCT scale-up register maintained by the Plateau State NSIP Coordinating Office. This register served as the sampling frame because it represented the beneficiary population relevant to the study period. Applying Yamane's (1967) formula at a 5% margin of error ($n = N / [1 + N(e^2)]$) to N = 42,894 yielded approximately 396 households; the sample was rounded to 400 to allow a practical margin for non-response and incomplete questionnaires.

A multistage sampling technique was used to select respondents. First, the six LGAs were stratified according to administrative boundaries. Second, Bowley's proportional allocation procedure distributed the 400-household sample in proportion to each LGA's share of the 42,894-household sampling frame, shown in Table 1. Third, simple random sampling was used within each LGA to select households from the beneficiary list, with one eligible adult respondent selected per sampled household.

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Table 1
Proportional Distribution of Sample Size by Local Government Area

LGA	Beneficiary Households	Share (%)	Allocated Sample
Barkin Ladi	11,266	26.27	105
Bassa	2,509	5.85	23
Jos East	781	1.82	7
Jos North	14,939	34.83	140
Jos South	9,868	23.01	92
Riyom	3,531	8.23	33
Total	42,894	100.00	400

Note. Source: Researcher's computation (2026), based on beneficiary records obtained from the Plateau State Cash Transfer Unit.

Of the 400 questionnaires administered, 376 were retrieved (a 94.0% retrieval rate), and 353 were retained as clean cases for descriptive analysis after screening. Complete-case screening for the hierarchical multiple regression left 212 observations with valid data on every variable in the specified model. The gap between the administered, retrieved, cleaned-descriptive and complete-case regression samples reflects different stages of data processing, not an arbitrary trimming of respondents. Complete-case analysis is a recognised way of handling item-level missingness in multivariable analysis, though it can reduce statistical power and introduce bias where missingness is systematic (Graham, 2009; Little & Rubin, 2019).

Nine key informants were purposively selected across five categories of programme actor: State Social Investment Programme (SIP Focal Person (1), State Conditional Cash Transfer Unit Officer (1) State Operations Coordinating Unit (SOCU) Official (1), Community Facilitators (2), Traditional Rulers (2), and CDA/WDC Chairmen or Women Leaders (2), for their direct institutional or community knowledge of programme targeting and implementation. Selection continued until responses across categories reached thematic saturation, consistent with purposive sampling for information-rich cases (Patton, 2015). Interviews were transcribed, coded and organised thematically; their role was explanatory, meant to illuminate mechanisms through which the quantitative association might plausibly operate rather than to confirm or test it independently. Informed consent was obtained, and identifying information was withheld given that the research was conducted in a conflict-affected setting.

3.2 Operationalisation of Variables

Dependent variable: Household Multidimensional Poverty Score (Y). Household multidimensional poverty was operationalised as a study-specific deprivation score informed by, but not claimed to be a formal replication of, the Alkire–Foster framework (Alkire & Foster, 2011). The score covers three dimensions and eight indicators: education (school attendance and financial

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constraints affecting schooling), health (reasonable-distance access to healthcare and affordability of treatment), and living standards (improved drinking water, improved sanitation, electricity or solar power, and personal mobile-phone ownership). Each indicator was coded 1 for deprivation and 0 for non-deprivation according to the operational criteria specified in the study instrument.

Within each dimension, indicator scores were averaged; the three resulting dimension scores were then combined using equal weights of one-third each: MPI = (Education Score + Health Score + Living-Standards Score) / 3. In practical terms, the two education indicators each carry half the education dimension, the two health indicators each carry half the health dimension, and the four living-standards indicators each carry a quarter of the living-standards dimension (Table 2). The resulting score ranges from 0 to 1, with higher values indicating greater deprivation. This follows the counting logic of the Alkire–Foster approach without computing the formal adjusted headcount ratio (M_0) or applying a separate cross-dimensional poverty cut-off (k). Insecurity was excluded from the poverty score itself, since it was specified separately as a contextual explanatory variable elsewhere in the study. Mobile-phone ownership was retained as a context-specific living-standards indicator because access to communication and digital financial infrastructure bears on household economic and social participation; it is not intended as a stand-in for wealth or income.

Table 2

Operationalisation of the Household Multidimensional Poverty Score

Dimension	Indicator	Deprivation coding	Weight within dimension
Education	School attendance	1 = deprived; 0 = non-deprived	1/2
Education	Financial constraint affecting schooling	1 = deprived; 0 = non-deprived	1/2
Health	Reasonable-distance access to healthcare	1 = deprived; 0 = non-deprived	1/2
Health	Affordability of treatment	1 = deprived; 0 = non-deprived	1/2
Living standards	Improved drinking water	1 = deprived; 0 = non-deprived	1/4
Living standards	Improved sanitation	1 = deprived; 0 = non-deprived	1/4
Living standards	Electricity or solar power	1 = deprived; 0 = non-deprived	1/4
Living standards	Personal mobile phone	1 = deprived; 0 = non-deprived	1/4

Note. Each dimension carries equal overall weight (one-third); indicator weights shown are the share of that one-third contributed by each indicator within its dimension. Source: Study instrument and researcher's computation (2026).

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Independent variable: Targeting Effectiveness (T). Targeting effectiveness was operationalised as a composite perception score built from five items covering respondents' assessments of poverty identification, transparency and fairness of selection, community participation, clarity of eligibility criteria, and the extent to which political influence affected selection. The negatively worded political-influence item was reverse-coded so that higher composite values consistently reflected greater perceived targeting effectiveness.

Control variables: age of household head, educational attainment, household size and duration of programme participation were entered as controls. Two further variables from the wider study: CCT effectiveness (a five-item composite assessment of perceived programme contribution to household welfare) and insecurity (a composite index of disruption to livelihoods, access to services and markets, and displacement) were also included in Model 2 alongside targeting effectiveness, since all three were specified together in the underlying regression model. Their own associations with multidimensional poverty are reported for completeness in Table 3, though they are not the focus of the present analysis.

3.3 Regression Model Specification

Hierarchical Multiple Regression Analysis (HMRA) was used to estimate the independent association between targeting effectiveness and the household multidimensional poverty score. Hierarchical regression allows variables to be entered in theoretically specified blocks, so that the additional explanatory contribution of a later block can be assessed once earlier variables are already accounted for (Field, 2018). Model 1 entered age, educational attainment, household size and duration of programme participation. Model 2 added targeting effectiveness alongside CCT effectiveness and insecurity, the other principal explanatory variables specified in the broader study. The analysis rests on 212 complete observations. The dependent variable was coded so that higher scores represent greater deprivation, meaning a negative coefficient indicates an association with lower deprivation.

4. Empirical Results

4.1 Descriptive Profile of Targeting Perceptions

Respondents rated the targeting mechanism favourably overall, with a composite mean of 3.66 on the five-point scale. The strongest item-level rating concerned the statement that the programme targeted genuinely poor households (M = 4.65, SD = 0.61), followed by perceived transparency and fairness of the selection process (M = 4.43, SD = 0.74). Community leaders' participation in beneficiary identification was also rated favourably (M = 4.03, SD = 0.88). The political-influence item recorded a raw mean of 1.28, indicating strong disagreement that political influence affected selection; the item was reverse-coded when the composite targeting score was constructed.

This descriptive picture points to a high level of beneficiary confidence in the targeting process. That result matters, but it should not be equated with independent evidence of targeting accuracy, which would require a direct assessment of inclusion and exclusion errors or a comparison of beneficiary status against independently established poverty criteria — neither of which this study attempts.

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Table 3
Hierarchical Multiple Regression Predicting Household Multidimensional Poverty (N = 212)

Predictor	B	SE	β	t	P	95% CI for B
Age of household head	−.002	.008	−.026	−.277	.782	[−.018, .014]
Educational attainment	.015	.014	.076	1.074	.284	[−.012, .042]
Household size	.028*	.012	.174*	2.261	.025	[.004, .053]
Duration of programme participation	−.068***	.018	−.306***	−3.740	< .001	[−.103, −.032]
Targeting effectiveness	.005	.018	.019	.273	.785	[−.031, .040]
CCT effectiveness	−.034	.018	−.137	−1.851	.066	[−.071, .002]
Insecurity	.006	.017	.023	.327	.744	[−.029, .040]

Note. Source: Field Survey (2026). Dependent variable = Household Multidimensional Poverty Score; higher scores indicate greater deprivation. Model 1: $R^2 = .098$, Adjusted $R^2 = .081$, $F(4, 207) \approx 5.64$, $p < .001$. Model 2: $R^2 = .114$, Adjusted $R^2 = .084$, $\Delta R^2 = .016$, F-change(3, 204) = 1.23, $p = .299$ (not statistically significant). Variance inflation factors for the full model specification ranged from 1.10 to 2.00, indicating no meaningful multicollinearity. 95% confidence intervals computed as B ± 1.96(SE). *p < .05. ***p < .001.

Targeting effectiveness showed no statistically significant association with the multidimensional poverty score (B = .005, SE = .018, β = .019, t = .273, p = .785). The coefficient sits close to zero and offers no evidence of an independent association within the specified model. Duration of programme participation, by contrast, remained negatively and significantly associated with deprivation (β = −.306, p < .001), and household size was also significant in Model 2 (β = .174, p = .025).

Adding targeting effectiveness, CCT effectiveness and insecurity in Model 2 raised explained variance only modestly, from $R^2 = .098$ in Model 1 to $R^2 = .114$ in Model 2. That ΔR^2 of .016 was tested directly and was not statistically significant, F-change(3, 204) = 1.23, p = .299, confirming that this block of variables did not improve model fit beyond the controls alone.

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Table 4
Test of the Targeting-Effectiveness Hypothesis

Hypothesis	β	P	Decision
H0: Targeting effectiveness has no statistically significant association with multidimensional poverty	.019	.785	Fail to reject H0

Note. Decision rule: reject H0 when $p \leq .05$; fail to reject H0 when $p > .05$. Source: Field Survey (2026).

The evidence does not establish a statistically significant independent association between perceived targeting effectiveness and the multidimensional poverty score, so the null hypothesis is not rejected at the 5% significance level.

The qualitative component was analysed thematically from the nine purposively selected key-informant interviews. Recurring themes clustered around targeting procedures, verification constraints, changing household circumstances and insecurity-related implementation difficulties. These themes are used below to contextualise the quantitative findings and surface plausible mechanisms; they are not offered as statistical tests of the regression relationships. No verbatim interview quotations are presented, since the available material was coded thematically rather than prepared for quotation-level reporting.

5. Qualitative Triangulation and Discussion

5.1 The Targeting–Poverty Puzzle

The central finding is that favourable beneficiary assessments of the targeting process did not translate into a statistically significant association with multidimensional poverty. Respondents rated targeting effectiveness positively overall, particularly on identifying genuinely poor households and on the fairness and transparency of selection. Yet targeting effectiveness produced a near-zero, statistically non-significant coefficient in the regression model ($\beta = .019$, $p = .785$).

This does not mean targeting is unimportant. It challenges a narrower assumption instead: that the quality of the targeting process, as perceived by beneficiaries at the point of selection, necessarily predicts subsequent household poverty outcomes. Targeting exists to reduce exclusion and inclusion errors so that limited social-protection resources reach households with greater need (Coady et al., 2004). But the payoff from accurate initial selection may depend on whether beneficiary status keeps corresponding with household circumstances over time — exactly the point Proposition 2 raises.

The result also fits the broader distinction between how a targeting system is designed and how it performs under changing conditions. Community-based approaches can draw on local knowledge, but how well they work depends on functioning verification, accountability and institutional capacity (Coady et al., 2004).

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5.2 Registry Currency, Displacement and Changing Vulnerability

The qualitative evidence offers one plausible, though untested, explanation for the absence of a significant targeting–poverty association. Key informants reported that household information can go stale quickly following displacement, population movement and changes in household circumstances. A state operations officer described how fast beneficiary records lose currency once households move from their original locations (KII/SSIP/02/2026).

Targeting accuracy is, by its nature, time-dependent. A household correctly identified as poor at registration may later experience changes in location, composition, livelihood or vulnerability. Conversely, households that become vulnerable after registration may remain outside the programme altogether. Getting the targeting decision right at enrolment, in other words, does not guarantee the register stays accurate afterwards.

This reading fits the adaptive perspective on social protection, which stresses the capacity of social-protection systems to respond to shocks and changing patterns of vulnerability (Bowen et al., 2020). The present study did not directly test registry age, updating frequency or changes in beneficiary status over time, so these factors should be treated as plausible explanatory mechanisms rather than demonstrated causes of the null regression finding.

5.3 Verification, Local Influence and Institutional Capacity

A second explanation from the qualitative evidence concerns the conditions under which verification actually happens. A community facilitator described attempts by local actors to influence beneficiary selection, and noted that verification becomes harder once insecurity restricts field supervision (KII/SSIP/05/2026).

The broader targeting literature treats community-based selection as potentially informative but also variable, shaped by implementation conditions and local institutional incentives (Coady et al., 2004). This qualified reading is consistent with Okorie (2026), whose Rivers State evidence identifies documentation, verification and grievance-redress problems even where formal targeting structures exist. It also sits alongside Dati's (2025) Plateau State evidence of CCT poverty-reduction potential, while showing that strong programme performance should not be assumed to follow automatically from perceived targeting quality. In the Plateau State context specifically, insecurity may restrict staff movement, drive up field-operating costs and reduce opportunities for direct verification. The present evidence cannot establish that insecurity caused the null targeting coefficient, but it offers a plausible contextual mechanism, consistent with Proposition 3, through which the targeting–poverty association could be weakened.

The conflict setting gives this issue a wider empirical context. Merttens, Hodey and Doyle (2024), in a Nigeria case study of targeting in protracted crises, show that violence, displacement and uneven exposure to shocks create difficult trade-offs over who should be targeted and which criteria remain appropriate as vulnerability changes. Their analysis is particularly relevant because it treats vulnerability as dynamic rather than assuming that a targeting decision remains equally appropriate over time. Salifu and Kufoalor (2024) likewise find that targeting performance across Sub-Saharan Africa is commonly affected by undercoverage, leakage and implementation constraints. The present study does not reproduce either approach: it examines beneficiary-assessed targeting effectiveness rather than independently calculating inclusion and exclusion

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errors. Their findings nevertheless provide a useful context for interpreting why favourable perceptions of targeting need not correspond neatly with later multidimensional poverty outcomes.

5.4 Interpreting the Null Association

Jointly, the quantitative and qualitative findings suggest the absent targeting association deserves a cautious reading. The survey shows beneficiaries generally regard the targeting process as fair and effective; the regression shows that this favourable assessment does not independently explain variation in multidimensional poverty.

One possible explanation is a temporal gap between initial and sustained targeting accuracy. Respondents' perceptions likely reflect their experience of the selection process itself, while the poverty score captures their multidimensional deprivation at the time of the survey. Where households go through displacement, livelihood disruption and changing vulnerability, the link between initial beneficiary identification and later poverty status can weaken.

The finding also suggests that targeting effectiveness should not be treated as a stand-in for programme impact. Correctly identifying a poor household establishes eligibility for assistance, but it does not guarantee that the household will go on to see enough improvement in education, health or living standards to produce a measurable drop in multidimensional deprivation.

Recent Nigerian evidence supports keeping these concepts analytically separate. Nomor, Asue and Akaakohol (2025), in an evaluation of CCT and NG-CARES in Benue State, report that the programmes were effective in reaching vulnerable groups, particularly women and larger households, while improvements across several poverty-related outcomes remained limited. The study is not directly comparable to the present analysis, but it illustrates an important point: relatively effective identification of vulnerable households does not automatically imply substantial improvement across all dimensions of household welfare. That distinction is consistent with the present finding that beneficiary assessments of targeting effectiveness were positive even though targeting effectiveness itself was not a significant predictor of the study's multidimensional deprivation score.

This matters particularly alongside the finding that duration of programme participation was significantly associated with lower deprivation ($\beta = -.306$, $p < .001$), while targeting effectiveness was not. Because the design is cross-sectional, this should be read as an observed difference in association, not as evidence that longer participation causes poverty reduction, or that targeting definitively has no effect at all. A non-significant coefficient in this specific model and sample means only that an independent association was not statistically detected; it does not rule out a small effect, nor one that a larger sample, a longitudinal design, or an independently verified measure of targeting accuracy might pick up.

The central contribution, then, is not to show that targeting is ineffective, but to show that perceived targeting quality and measurable poverty outcomes are analytically distinct constructs.

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6. Limitations of the Study

The cross-sectional design limits how far temporal or causal relationships can be established. The analysis identifies an association between targeting effectiveness and multidimensional poverty, but it cannot say whether changes in targeting effectiveness produce subsequent changes in household poverty.

The inferential analysis rests on 212 complete cases, against 353 available for descriptive analysis. Complete-case analysis was necessary for the specified hierarchical regression models, but the resulting drop in sample size may introduce bias if cases with missing observations differed systematically from those retained.

The quantitative household sample itself was drawn through probability sampling, with LGA-level stratification with proportional allocation, followed by simple random selection within each LGA, so the reduction to 212 complete cases is a missing-data limitation, not a non-probability sampling one. The qualitative component, by contrast, used purposive sampling of nine key informants chosen for their institutional or community knowledge; this is standard practice for explanatory qualitative inquiry (Patton, 2015), but it means the qualitative findings cannot be treated as statistically representative of all programme stakeholders.

Targeting effectiveness was measured through beneficiaries' own perceptions of the targeting process. Such measures speak to perceived fairness, transparency and credibility, but they are not an independent audit of targeting accuracy. Respondents may also have been shaped by social-desirability considerations, and because targeting effectiveness, CCT effectiveness and household welfare perceptions were all collected from the same respondents through the same instrument, the results carry some degree of common-method bias that cannot be fully separated from genuine association in a single cross-sectional survey.

The multidimensional poverty measure captures deprivation across the study's specified education, health and living-standards indicators. Although the Alkire-Foster framework offers a systematic approach to multidimensional poverty measurement (Alkire & Foster, 2011), this study uses its own operationalised household deprivation score, with equal dimensional weighting assumed by default, rather than computing the formal adjusted headcount ratio M_0 with an explicit deprivation cut-off. Findings involving this measure should be read as applying to the study's own deprivation score, not to a formally validated Alkire-Foster poverty measure.

Finally, the absence of statistical significance is not proof that targeting has no substantive effect on poverty. A coefficient of $\beta = .019$ ($p = .785$) means an independent association was not statistically established in this model and sample; it does not rule out a small effect, or one that a larger sample, a longitudinal design, or a direct, independently verified measure of targeting accuracy might detect.

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7. Policy Implications and Conclusion

7.1 Policy Implications

The recommendations below are evidence-informed, drawn from the study's findings and contextual evidence from key informants; they are not interventions the study has directly evaluated. Targeting should be treated as a continuing administrative function rather than a one-time enrolment exercise. Where displacement and livelihood disruption recur, the relevant social-protection institutions should build in periodic updating of beneficiary records, so registered households keep corresponding with prevailing patterns of vulnerability (Bowen et al., 2020).

Verification and grievance-redress mechanisms need enough institutional independence and operational capacity to actually function. Community participation remains valuable, since local actors hold information administrators simply do not have, but community-based targeting needs effective oversight to keep selection error and implementation risk in check (Coady et al., 2004).

Programme monitoring should separate targeting-process indicators from targeting-outcome indicators. Beneficiary perceptions of fairness and transparency are useful measures of programme legitimacy, but they should not stand in for targeting accuracy or poverty impact. Where feasible, monitoring should examine inclusion and exclusion errors, registry updating, changes in household circumstances and the persistence of beneficiary eligibility, ideally through periodic independent spot-checks rather than beneficiary self-report alone.

Finally, accurately identifying poor households does not, by itself, close gaps in water, sanitation, electricity, healthcare, livelihoods and other dimensions of deprivation. CCTs are likely to have greater poverty-reduction potential when paired with complementary interventions from the relevant sectoral agencies — primary healthcare, basic education, water and sanitation, and agricultural extension services (Fiszbein & Schady, 2009; Bastagli et al., 2019).

7.2 Conclusion

This study asked whether beneficiary-assessed targeting effectiveness was associated with multidimensional poverty among CCT beneficiaries in the Northern Senatorial District of Plateau State. The descriptive evidence shows strong beneficiary confidence in the targeting process; the inferential analysis found no statistically significant independent association between targeting effectiveness and multidimensional poverty ($\beta = .019$, $p = .785$).

That is a failure to detect a statistically significant association in this specific model and sample, not evidence that targeting effectiveness has no relationship whatsoever with household welfare. The distinction matters: the study cannot rule out a smaller effect, an effect running through pathways not captured here, or an effect a larger or longitudinal sample would pick up. What the evidence does establish is that positive perceptions of the targeting process do not automatically translate into measurable differences in multidimensional poverty outcomes as measured here. The qualitative evidence points to registry obsolescence after displacement, constraints on field verification, and the continuing possibility of local influence as plausible mechanisms behind this gap. These mechanisms need further empirical testing before they can be treated as established explanations rather than informed conjecture.

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In conflict-affected environments, the policy challenge is therefore not simply to identify poor households accurately at enrolment, but to keep that identification accurate as circumstances change. A credible targeting process is a necessary condition for effective social protection, but it is not, on its own, sufficient evidence of a measurable poverty effect. Future research should examine the temporal relationship between targeting accuracy, changes in beneficiary circumstances and multidimensional poverty outcomes, ideally through longitudinal designs and independently validated measures of inclusion and exclusion errors.

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