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Farmer-participatory action research for improving chickpea yield and farm income under rainfall risk in Banda District, Uttar Pradesh: A statistical protocol with demonstration analysis

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Abstract

Rainfall uncertainty in the Bundelkhand region makes chickpea production a problem of both agronomy and decision-making. This paper develops a farmer-participatory action-research protocol for Banda district, Uttar Pradesh, in which a locally feasible chickpea management package is evaluated not only for yield response but also for farm income, practice adoption, equity across irrigation conditions, and yield risk. The proposed design includes 160 chickpea-growing farmers from eight villages, allocated within villages to an immediate-intervention group or a wait-list comparison group after matching on baseline yield and irrigation status. The primary confirmatory outcome is endline chickpea yield. The treatment effect is estimated by Analysis of Covariance (ANCOVA) adjusted for baseline yield and pre-specified farm characteristics, with village fixed effects and HC₃ heteroskedasticity-robust standard errors. Net return and practice adoption are secondary outcomes; treatment-by-irrigation interaction and yield dispersion are examined as effect-modification and resilience questions. Because no primary farmer-level dataset was supplied for the present manuscript, numerical results are based on a reproducible calibrated synthetic dataset and are reported only to demonstrate the intended analysis. In this demonstration, the adjusted yield effect was 2.00 q ha⁻¹ (95% CI 1.64 to 2.37), the adjusted increase in net return was INR 8,580 ha⁻¹ (95% CI 6,480 to 10,680), and high adoption of recommended practices was more frequent in the intervention group. No meaningful treatment-by-irrigation interaction was detected, while evidence for reduced yield dispersion was inconclusive. The framework shows how action research can combine farmer knowledge, a credible comparison group, explicit statistical hypotheses, economic evaluation, and iterative learning to support locally defensible agricultural decisions.

Keywords: Action research, banda, bundelkhand, chickpea, farmer-participatory research, ANCOVA, farm income, rainfall risk

1. Introduction

Agricultural recommendations are usually presented as technical choices, yet farmers experience them as decisions made under uncertainty. A chickpea grower deciding whether to purchase improved seed, sow in lines, conserve residual soil moisture, or reserve scarce water for one protective irrigation is simultaneously managing biological risk, cash-flow constraints, labour availability and the possibility of a poor season. For applied agricultural research, the relevant question is therefore not simply whether a package can increase mean yield. A stronger question is whether the package produces a gain that is statistically credible, economically worthwhile, feasible for farmers to implement, and sufficiently robust across contrasting resource conditions.

This problem is particularly relevant in Banda district of the Bundelkhand region of Uttar Pradesh. Recent local studies describe substantial climate vulnerability in rainfed farming systems and document a large pulse economy in which chickpea occupies an important place. Irrigation is incomplete, so resilience cannot be defined only as access to water. Seed choice, sowing method, soil-moisture management, crop monitoring and timely protective irrigation where available are more realistic components of a locally adaptable response. Existing KVK and pulse-seed interventions in Banda further show that farmer participation and access to improved seed can alter both productivity and management practice.

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Modern crop statistics increasingly treats variation as information rather than nuisance. Mixed models, genotype-by-environment analysis, robust methods and predictive tools have shown that performance must be interpreted in relation to environment, uncertainty and economic consequence. The same logic can be brought to farmer-level action research. Instead of asking only whether one variety or treatment has a higher mean, the study can ask who benefits, whether the gain survives the cost calculation, whether farmers actually adopt the recommended practices, and whether remaining variation points to a specific failure mechanism that should be addressed in the next cycle.

The present paper proposes such an approach for chickpea in Banda. It has two aims: first, to specify a realistic farmer-participatory field design with explicit hypotheses and a pre-defined statistical analysis plan; and second, to demonstrate how the results would be interpreted using a calibrated dataset without presenting simulated values as observed evidence. The framework is intended for conversion into a prospective field study through collaboration with KVK, district extension personnel and participating farmers.

2. Materials and Methods

2.1 Study Area

Banda district lies in the Bundelkhand region of Uttar Pradesh and is characterized by marked rainfall variability, substantial rainfed cultivation and uneven irrigation access. The proposed study is situated in the Rabi chickpea system because chickpea is locally important, compatible with the district resource base, and suitable for testing a moisture-conscious management strategy. Eight villages should be selected from four blocks representing variation in irrigation access, soil conditions and market connectivity. Tindwari, Badokhar Khurd, Baberu and Naraini provide plausible block-level strata, but the final village list should be fixed only after rapid rural appraisal and consultation with KVK and district extension staff.

2.2 Study Design and Participants

The study uses a farmer-participatory, matched wait-list design embedded within one Plan-Act-Observe-Reflect action-research cycle. The farmer-field is the unit of inference. Within each village, eligible farmers are paired on irrigation status and baseline chickpea performance, with landholding used as an additional matching variable where required. One farmer from each pair is allocated to receive the intervention package during the study season and the paired farmer continues usual practice for that season before receiving the package after endline measurement. This design provides a contemporaneous counterfactual while remaining acceptable in an extension setting because the comparison group is not permanently denied the intervention.

A total sample of 160 farmers is proposed: 80 intervention and 80 wait-list comparison farmers, approximately 20 per village. Eligibility requires planned chickpea cultivation during the study season, a study plot with verifiable baseline management information, willingness to maintain a simple

input and labour record, and informed consent. Smallholders, rainfed farmers and resource-constrained households should not be excluded because these groups are central to the policy relevance of the research.

The sample size is intentionally larger than the minimum required for a simple two-group comparison. If a difference of 2.0 q ha⁻¹ is considered a plausible target effect and the within-group standard deviation is approximately 2.8 q ha⁻¹, the standardized effect is about 0.71. A conventional calculation would require roughly 32 farmers per arm for 80% power at the 5% level under independent sampling. Increasing the target to 80 per arm provides allowance for attrition, village-level stratification, baseline adjustment, interaction analysis and the heterogeneity typical of farm data.

2.3 Intervention Package

The intervention is a locally adaptable chickpea resilience package rather than a fixed high-cost technology. Components are selected on the basis of local feasibility and are recorded separately so that implementation can be examined rather than assumed. The core package consists of:

- Locally validated improved chickpea seed, subject to current KVK or certified-seed availability;
- Seed quality assurance and recommended seed treatment;
- Line sowing at locally recommended spacing;
- Soil-test-informed nutrient management where soil testing is available, otherwise a clearly documented local recommendation;
- In-situ soil-moisture conservation appropriate to field slope, soil and available implements;
- One need-based protective irrigation when a usable water source exists and a pre-defined crop stress condition is reached; and
- Participatory crop walks and short advisories for pest scouting, rainfall response and timing of management operations.

The estimand is the effect of being offered and supported to implement the package. The primary analysis therefore follows the intention-to-treat principle. A farmer remains in the assigned intervention group even if one or more components are not adopted. Per-protocol analyses may be reported only as exploratory because farmers who comply fully may differ systematically from those who do not.

2.4 Action-Research Process

The action-research cycle is deliberately simple. Farmers first identify the most important production constraints and define which recommended practices are realistically adoptable. The intervention is then implemented, field outcomes and process indicators are recorded, and the results are discussed with farmers after harvest. The reflection stage is not treated as an informal add-on: unexplained low performance, non-adoption and unexpected costs become explicit questions for the next cycle.

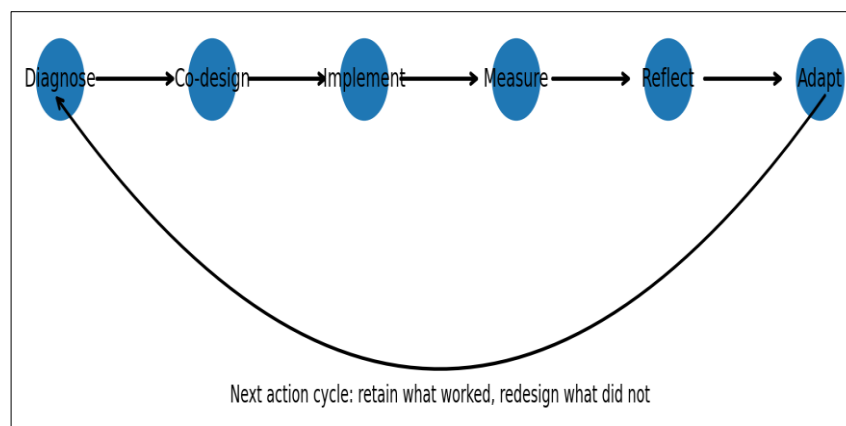


Fig 1: Farmer-participatory action-research cycle used in the proposed study

2.5 Outcomes and Measurement

The primary outcome is chickpea grain yield at harvest, measured by standardized plot harvest or crop-cut methodology and converted to q ha^{-1} . Secondary outcomes include net return per hectare, adoption of the seven intervention components, the probability of a low-yield outcome, and farmer-reported implementation constraints.

Net return is defined as gross value of grain and by-product minus variable costs, with the treatment of family labour stated explicitly and examined in sensitivity analysis. Practice adoption is summarized as a 0-7 index; high adoption is pre-defined as implementation of at least five components.

Table 1: Primary and secondary study outcomes, operational definitions, and measurement protocols

Outcome	Operational definition	Measurement rule
Yield (primary)	Chickpea grain yield (q ha^{-1})	Standardized harvest/crop-cut; method fixed before fieldwork.
Net return	INR ha^{-1}	Gross crop value minus pre-specified variable and labour costs.
Practice adoption	0-7 index; high adoption ≥ 5	One point for each package component implemented to protocol.
Low-yield risk	Pre-specified yield threshold	Threshold fixed from local agronomic/historical evidence before outcome analysis.
Irrigation access	Usable irrigation for study plot	Baseline status plus number/timing of protective irrigations.
Process fidelity	Delivered, received, implemented	Separates intervention delivery failure from farmer non-adoption.

2.6 Statistical hypotheses

The hypotheses are specified before endline analysis. The primary hypothesis concerns yield and is confirmatory. Secondary hypotheses concern economic return and adoption. Effect modification by irrigation and reduction in yield dispersion are important research questions, but they are interpreted more cautiously because interaction and variance tests generally require larger samples and, for resilience, repeated seasons.

Primary hypothesis (H_1). After adjustment for baseline yield and pre-specified covariates, assignment to the chickpea resilience package will increase mean endline yield relative to the wait-list comparison group. Formally, $H_0: \beta Y = 0$ versus the directional research hypothesis $H_1: \beta Y > 0$. To avoid relying on a one-sided significance claim, inference

will be reported with a two-sided 95% confidence interval and two-sided p-value. A result will be considered agronomically meaningful only when the estimated gain is at least 1.5 q ha^{-1} and the 95% confidence interval excludes zero.

The same separation between statistical evidence and practical importance is used for secondary outcomes. For net return, a positive effect must remain after the additional cost of the package is included. For adoption, both the adjusted odds ratio and the absolute difference in adoption probability are reported. For irrigation, the hypothesis concerns effect modification rather than a main effect. For yield variability, evidence of reduced dispersion is treated as exploratory unless reproduced across seasons.

Table 2: Study hypotheses, rationales, and analytical approaches

Hypothesis	Null hypothesis	Research hypothesis	Rationale	Main analysis
H_1 : Yield (primary)	$\beta Y = 0$	$\beta Y > 0$	A useful package should increase productivity after baseline differences are controlled.	ANCOVA; 95% CI; HC3 SE
H_2 : Net return	$\beta N = 0$	$\beta N > 0$	Yield gain is not sufficient if added costs absorb the benefit.	Baseline-adjusted linear model / DiD
H_3 : Adoption	$OR = 1$	$OR > 1$	A scalable intervention should change farmer practice, not only supply inputs.	Logistic regression + risk difference
H_4 : Irrigation interaction	$\beta Y \times I = 0$	$\beta Y \times I \neq 0$	Benefit may differ according to water access; this is an equity and targeting question.	Treatment $\times$ irrigation interaction
H_5 : Yield dispersion	$\sigma^2 T = \sigma^2 C$	$\sigma^2 T < \sigma^2 C$	A resilience package may reduce variability as well as raise the mean.	Brown-Forsythe; CV; robust sensitivity

2.7 Statistical Analysis

The primary estimand is the average effect of treatment assignment on endline yield. Because baseline yield is expected to explain a substantial part of between-farm variation, the primary model is an ANCOVA rather than an unadjusted comparison of change scores:

$$Y_1 = \beta_0 + \beta_1 T + \beta_2 Y_0 + \beta_3 I + \beta_4 L + \beta_5 A + \text{village effects} + \varepsilon$$

Where Y_1 is endline yield, T is treatment assignment, Y_0 is baseline yield, I is irrigation status, L is livestock ownership and A is operational landholding. The coefficient β_1 estimates the adjusted treatment effect. Village fixed effects account for the stratified design. HC₃ heteroskedasticity-consistent standard errors are used because the variance of farm outcomes is unlikely to be constant across holdings.

Difference-in-differences is also reported as an intuitive descriptive estimate: [mean change in the intervention group] minus [mean change in the comparison group]. Net return is analysed with an analogous baseline-adjusted model. The adoption index is analysed as a continuous score and as the pre-specified high-adoption binary endpoint. Logistic regression provides an adjusted odds ratio, but absolute risks and risk differences are also reported because they are easier to interpret for extension planning.

Effect modification by irrigation is tested by adding a treatment-by-irrigation interaction. A non-significant interaction is not interpreted as proof of identical effects; it indicates that meaningful heterogeneity was not detected with the available precision. Yield variability is examined using group-specific standard deviations, coefficients of variation and the Brown-Forsythe test. Extreme outcomes are investigated rather than automatically deleted. Huber M-estimation or median regression is used as a sensitivity analysis if influential observations materially affect the primary estimate.

All estimates are presented with 95% confidence intervals and in farmer-interpretable units. Missing outcomes are reported by group and reason. The primary analysis is intention-to-treat. If missingness is more than trivial and the missing-at-random assumption is defensible, multiple

imputation using baseline yield, village, landholding, irrigation and treatment assignment is preferred to complete-case analysis. For multiple confirmatory secondary tests, Holm adjustment is recommended; exploratory analyses are identified as such rather than interpreted through an arbitrary significance threshold.

2.8 Ethics and Data Quality

The empirical study should obtain the institutional approval appropriate for agricultural household research and informed consent from participating farmers. Study identifiers should replace farmer names in the analytic dataset. Permission is required for photographs or named case histories. Input support must be documented so that economic analysis distinguishes project-subsidized cost from farmer-replicable cost. Outcome definitions, exclusion rules, treatment assignment and the primary analysis model should be frozen before endline outcomes are examined.

2.9 Nature of the Demonstration Data

The numerical results that follow are not field observations. They are generated from a reproducible synthetic dataset calibrated to plausible Banda conditions and to the scale of effects reported in local pulse demonstrations. The purpose is to show exactly how the pre-specified hypotheses, models and interpretation would operate once field data are available. The demonstration dataset must not be cited as evidence of programme effectiveness in Banda.

3. Results

3.1 Baseline Characteristics

The calibrated dataset contains 80 intervention and 80 comparison farmers. Baseline mean yield was 11.56 q ha⁻¹ in the intervention group and 11.33 q ha⁻¹ in the comparison group. Irrigation prevalence was the same in both groups (51.2%), while landholding and age were also similar. Livestock ownership was somewhat lower in the intervention group and was therefore retained as a pre-specified adjustment variable. These baseline values are used to demonstrate model behaviour and should not be interpreted as observed village statistics.

Table 3: Baseline characteristics in the calibrated demonstration dataset

Characteristic	Comparison (n=80)	Intervention (n=80)
Age (years)	44.8	45.7
Landholding (ha)	1.26	1.27
Irrigated farms (%)	51.2	51.2
Livestock-owning farms (%)	70.0	58.8
Baseline yield (q ha ⁻¹)	11.33	11.56
Baseline net return (INR ha ⁻¹)	35,367	36,388
Baseline practice index (0-7)	2.99	3.12

3.2 Effect on chickpea yield

Mean yield changed from 11.33 to 11.48 q ha⁻¹ in the comparison group and from 11.56 to 13.73 q ha⁻¹ in the intervention group. The unadjusted difference-in-differences was 2.01 q ha⁻¹. In the pre-specified ANCOVA, the adjusted treatment effect was 2.00 q ha⁻¹ (95% CI 1.64 to 2.37; $p < 0.001$). The estimate exceeded the pre-defined 1.5 q ha⁻¹ threshold for agronomic relevance, and the confidence

interval excluded zero. Thus, the demonstration analysis is consistent with H1.

The interpretation should remain on the original scale. An estimated gain of about two quintals per hectare is more useful for farmers and extension personnel than a statement based only on statistical significance. The confidence interval also shows the plausible range of the gain and therefore supports planning under uncertainty.

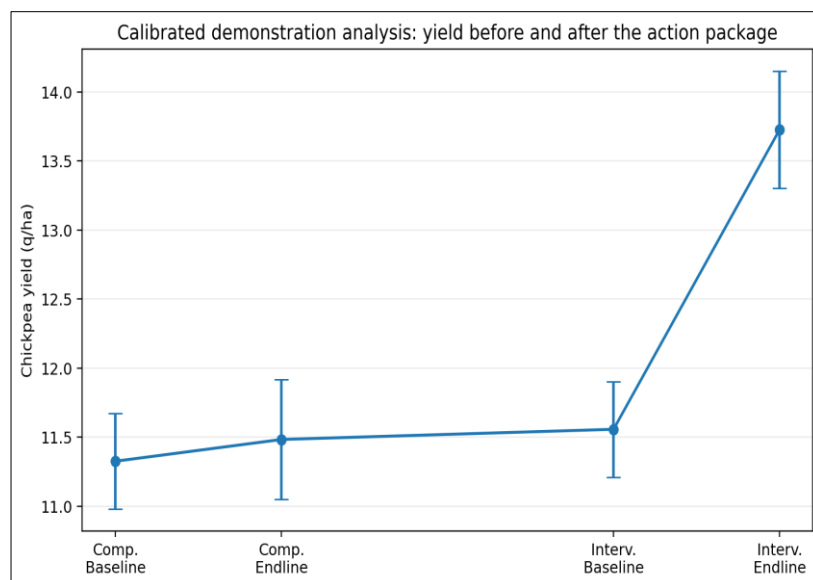


Fig 2: Mean chickpea yield at baseline and endline in the calibrated demonstration dataset

3.3 Effect on Net Return

Average net return increased by INR 3,195 ha⁻¹ in the comparison group and INR 11,840 ha⁻¹ in the intervention group. The simple difference-in-differences was INR 8,645 ha⁻¹. After adjustment for baseline net return and farm covariates, the estimated intervention effect was INR 8,580 ha⁻¹ (95% CI 6,480 to 10,680; $p < 0.001$), supporting H₂ in the demonstration analysis. Using the calibrated yield effect and the assumed crop value used in the synthetic dataset, the incremental gross value of yield was approximately INR 11,319 ha⁻¹ against an incremental package cost of about INR 2,600 ha⁻¹. The corresponding gross-benefit-to-added-cost ratio was approximately 4.35:1. This ratio is an analytical illustration rather than a price forecast; empirical analysis should use observed farm-gate prices and farmer-replicable costs.

3.4 Practice Adoption

The mean practice index increased from 2.99 to 3.83 in the comparison group and from 3.12 to 5.01 in the intervention group. Baseline-adjusted analysis estimated an additional 1.05 practices (95% CI 0.73 to 1.37; $p < 0.001$). High adoption, defined a priori as at least five of seven practices, occurred in 63.7% of intervention farms and 37.5% of comparison farms. The adjusted odds ratio was 6.67 (95% CI 2.46 to 18.07; $p < 0.001$). The absolute difference of about 26 percentage points is more directly useful for programme planning and indicates support for H₃.

3.5 Effect by Irrigation Status

The estimated treatment-by-irrigation interaction was 0.03 q ha⁻¹ (95% CI -0.68 to 0.73; $p = 0.941$). Thus, the demonstration analysis does not support H₄. The appropriate conclusion is not that irrigation status can never modify the intervention effect, but that no meaningful differential effect was detected in this one-season, moderate-sized dataset. A multi-season study would be a stronger test, especially under a severe dry-spell year.

3.6 Yield Variability and Low-Yield Risk

Endline yield standard deviation was 1.98 q ha⁻¹ in the comparison group and 1.94 q ha⁻¹ in the intervention group. Relative variability was lower in the intervention group because of the higher mean (CV 14.1% versus 17.2%), but the Brown-Forsythe test for dispersion of yield change was not significant ($p = 0.628$). H₅ is therefore not supported in the demonstration analysis. This distinction is important: the package may raise average yield without yet showing that it reduces farmer-to-farmer variability.

Using an illustrative low-yield threshold of 10 q ha⁻¹, 5.0% of intervention farms and 21.2% of comparison farms fell below the threshold. Because such a result depends on the threshold chosen, an empirical study should define the stress threshold before examining endline data, preferably using agronomic or historical yield evidence.

Table 4: Main results from the calibrated demonstration analysis

Outcome	Adjusted estimate	95% CI	p-value	Interpretation
Yield (q ha ⁻¹)	2.00	1.64 to 2.37	<0.001	Supports H ₁ ; exceeds practical threshold
Net return (INR ha ⁻¹)	8,580	6,480 to 10,680	<0.001	Supports H ₂
Practice index (0-7)	1.05	0.73 to 1.37	<0.001	Supports H ₃
High adoption (adjusted OR)	6.67	2.46 to 18.07	<0.001	Supports H ₃
Treatment × irrigation (q ha ⁻¹)	0.03	-0.68 to 0.73	0.941	H ₄ not supported
Yield-change dispersion	Brown-Forsythe F = 0.24	-	0.628	H ₅ not supported

4. Discussion

4.1 Main Findings

The proposed framework produces a result pattern that is more informative than a conventional demonstration-versus-check comparison. In the calibrated analysis, the

intervention increases adjusted mean yield by about 2 q ha⁻¹ and the gain remains economically positive after added costs are included. Practice adoption also improves, indicating that the intervention is represented not only by supplied inputs but by measurable changes in crop management. At

the same time, the analysis does not manufacture evidence where it is weak: the irrigation interaction is close to zero and the variance test does not support a reduction in yield dispersion. This combination of positive and null findings is precisely what a credible action-research study should be able to report.

The primary yield hypothesis is deliberately framed around an adjusted treatment effect rather than an unadjusted endline mean. Baseline productivity, irrigation and landholding are strong sources of farm heterogeneity, and ignoring them can either inflate residual noise or create misleading comparisons. ANCOVA preserves the randomized comparison while improving precision. Reporting a practically meaningful threshold in addition to the p-value further prevents a large sample from turning a trivial difference into a policy recommendation.

4.2 Meaning for Farmers

From a farmer's perspective, the intervention is not a statistical treatment. It is a set of decisions that require cash, labour, timing and confidence that the additional effort will be rewarded. An improved seed lot may cost more than saved seed; line sowing may require a seed drill or additional labour; and one protective irrigation may compete with other household or farm uses of scarce water. For this reason, yield and net return must be interpreted together. A package that raises yield but leaves little additional income after cost is unlikely to persist after project support ends.

The adoption outcome is equally important. If the observed gain occurs only while the project provides inputs and close supervision, the intervention may be a successful demonstration but a weak scaling strategy. If farmers retain the practices they consider most valuable, the research has identified a plausible behavioural pathway for sustained benefit. Post-harvest reflection meetings should therefore discuss not only which fields yielded most, but which components were retained, which were dropped, what they cost, and why performance differed between farmers.

4.3 Learning from Unexplained Variation

Residual variation should not be treated only as statistical error. In action research it is also a source of the next research question. Intervention farms that perform below expectation should be examined for sowing delay, shallow soil, late seed delivery, pest pressure, grazing, missed irrigation opportunity or poor implementation of a specific component. The next cycle can then simplify, replace or target the weak part of the package rather than repeating the same intervention unchanged.

A multi-season extension of the study is therefore essential if the objective is climate resilience rather than one-season productivity. With repeated Rabi seasons, farmer and village can be represented as random effects and treatment-by-weather interaction can be tested directly. Such a model would distinguish persistent farmer differences from year-specific shocks and would provide a stronger test of whether the intervention buffers adverse rainfall conditions.

$$\text{Yield} = \beta_0 + \beta_1 \text{Treatment} + \beta_2 \text{Year} + \beta_3 \text{Weather} + \beta_4 (\text{Treatment} \times \text{Weather}) + \text{farmer effect} + \text{village effect} + \varepsilon$$

4.4 Implications for Extension in Banda

For KVK and district extension systems, the proposed design offers a practical shift from counting demonstrations to estimating effects. Routine reporting can include the adjusted yield gain, uncertainty interval, additional net return, adoption persistence and cost per additional quintal. These quantities are more useful for deciding whether to scale a package than beneficiary counts alone. The wait-list structure also fits extension practice because comparison farmers can receive the package after the study season.

The irrigation analysis has an important equity interpretation. Because irrigation coverage is incomplete, a resilience strategy that is beneficial only for already-irrigated farms may widen rather than reduce vulnerability. The package is therefore intentionally built around several non-irrigation components. If future field data show strong treatment-by-irrigation interaction, the correct response is to target the current package and develop a separate rainfed version rather than average the interaction away.

The district's existing experience with pulse seed systems creates a plausible delivery platform. Rather than establishing a short-lived parallel project, the action-research protocol can be nested within existing seed and KVK networks. This increases the chance that the research cycle becomes part of extension learning rather than a one-time academic exercise.

4.5 Strengths and Limitations

The principal strength of the framework is the integration of a credible comparison group, pre-specified hypotheses, farmer participation, economic evaluation and robustness checks within one operational study. The design distinguishes the effect of assignment from the level of compliance, reports uncertainty rather than only significance, and treats heterogeneity and non-adoption as substantive findings. The practical-significance rule for the primary outcome further separates a statistically detectable effect from an effect worth scaling. The major limitation of the present manuscript is that its numerical results are synthetic. They demonstrate the analysis but cannot establish effectiveness in Banda. A one-season study also cannot establish climate resilience across contrasting rainfall years. Farmer-level allocation within villages may produce information spillovers, which would tend to reduce the observed contrast. The intervention is multi-component, so the first cycle estimates the bundle effect rather than the isolated contribution of each component. Net return is sensitive to valuation of family labour and by-products, and self-reported adoption may be affected by social desirability. These issues should be addressed through field verification, pre-specified cost rules, sensitivity analysis and, where possible, repeated-season follow-up.

5. Decision Rules for the Next Action Cycle

Action research should end with a decision, not only a significance test. Before implementation, the research team and participating farmers should agree on simple rules that determine whether the package is scaled, modified, targeted or stopped. The thresholds below are examples and should be finalized prospectively.

Table 5: Example decision rules for the next action-research cycle

Evidence pattern	Interpretation	Action for next cycle
Adjusted yield gain ≥ 1.5 q ha ⁻¹ ; lower 95% CI >0; net return positive	Productivity gain is statistically and practically credible	Retain core package and expand to the next village cohort.
Yield gain positive but net return small or negative	Agronomic response is not economically sustainable	Reduce high-cost components and re-test the lower-cost bundle.
Strong treatment $\times$ irrigation or soil interaction	Benefit is context-specific	Target the package and develop an alternative for excluded farms.
Average effect positive but adoption poor or residual variance high	Delivery or implementation pathway is weak	Use farmer interviews and crop-walk records to redesign the weak component.
No meaningful yield effect and no economic gain	Mechanism is not supported	Do not scale; return to diagnosis and redesign the intervention.

6. Conclusion

A useful agricultural action-research study must connect the farmer's problem to a testable causal question. In Banda, rainfall uncertainty, incomplete irrigation and the importance of chickpea provide a realistic setting for such work. The proposed protocol treats yield as the primary outcome but does not stop there: farm income, practice adoption, heterogeneity and risk are incorporated because these determine whether a statistically effective intervention is also a practical one. The calibrated analysis illustrates the intended standard of inference. It supports a meaningful yield and net-return effect and stronger adoption, while providing no evidence of irrigation-specific benefit or reduced yield dispersion in one season. The value of this result pattern is methodological: positive findings are retained where the evidence is strong, and unsupported claims are not added for narrative completeness. Once the protocol is implemented with observed farmer-level data over repeated seasons, it can provide a defensible basis for local extension decisions and for a genuine empirical action-research paper.

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Appendix A: Minimum field data set

Variable	Definition	Type / unit
Farmer_id	Anonymous study identifier	Text
Village	Village code/name	Categorical
Treatment	Immediate package offer vs wait-list	Binary
Baseline_yield	Previous verified chickpea yield or baseline plot yield	q ha ⁻¹
Endline_yield	Standardized harvest/crop-cut yield	q ha ⁻¹
Land_ha	Operational landholding	ha
Irrigated	Usable irrigation for study plot	0/1
Livestock	Livestock-owning household	0/1
Sowing_date	Actual sowing date	Date
Practice_index	Number of package components implemented	0-7
Variable_cost	Farmer-replicable variable cost	INR ha ⁻¹
Net_return	Gross crop value minus pre-specified cost	INR ha ⁻¹
Rainfall_exposure	Seasonal rainfall/dry-spell indicators	mm / derived
Process_fidelity	Delivered, received, implemented	Categorical
Loss_event	Pest, grazing, waterlogging, hail or other shock	Categorical