ISSN(O): 2320-5407 | ISSN(P): 3107-4928

ISSN NO. 2320-5407

International Journal of Advanced Research

Publisher's Name: Jana Publication and Research LLP

www.journalijar.com

REVIEWER'S REPORT

Manuscript No.: IJAR-58796

Title: ANALYSE DE LA DIVERSITÃ‰ ET DE LA STRUCTURE DEMOGRAPHIQUE DES PEUPLEMENTS DE ANNONA SENEGALENSIS AU CENTRE-SUD DU NIGER,

Recommendation:
Accept as it is .....
Accept after minor revision.....
Accept after major revision ...YES...
Do not accept (*Reasons below*) .....

Rating	Excel.	Good	Fair	Poor
Originality		□		
Techn. Quality			□	
Clarity			□	
Significance		□		

Reviewer's ID: JPR-094

Detailed Reviewer's Report

REVIEWER'S REPORT

Overall recommendation: MAJOR REVISION

The manuscript addresses an important ecological and conservation issue concerning *Annona senegalensis* in the semi-arid agroecosystems of Niger. The field dataset appears potentially valuable because it covers **142 plots across 14 villages in Maradi and Zinder** and combines floristic diversity, regeneration, importance values and demographic structure. However, several **substantial methodological, statistical, numerical, interpretative and presentation problems** need to be corrected before the manuscript can be considered for publication.

1. STRENGTHS OF THE MANUSCRIPT

1.1 Scientific relevance

The study focuses on *Annona senegalensis*, an important multipurpose woody species in the Sudano-Sahelian zone. Its regeneration and conservation are relevant to biodiversity, agroforestry, food security and rural livelihoods.

1.2 Good geographical coverage

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The study includes **14 villages, six in Maradi and eight in Zinder**, with 142 plots. This provides a useful regional dataset and permits comparison between two ecologically and anthropogenically contrasting regions.

1.3 Multiple ecological indicators

The authors use several complementary approaches:

Shannon diversity index

Pielou evenness

Sørensen similarity

Importance Value Index (IVI)

Diameter-class structure

Height-class structure

Weibull distribution

Regeneration rate

This multidisciplinary approach is one of the strongest aspects of the manuscript.

1.4 Important regeneration findings

The reported regeneration of *A. senegalensis* is particularly interesting:

Zinder: 88 young individuals/ha; regeneration rate 79.3%

Maradi: 24 young individuals/ha; regeneration rate 47.1%

The contrast between low adult importance and relatively high recruitment could provide an interesting ecological interpretation.

1.5 Practical conservation significance

The recommendation to protect natural regeneration, control anthropogenic pressures and promote assisted natural regeneration is relevant to sustainable management in the Nigerien Sahel.

1.6 Literature coverage

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The manuscript cites several relevant studies dealing with Sahelian vegetation, regeneration, agroforestry, phytogeography and diversity.

2. MAJOR WEAKNESSES / CRITICAL ISSUES

2.1 Major inconsistency in the Shannon index calculation

This is one of the **most important methodological problems**.

The manuscript states:

$$H = -\Sigma\ Pi \log_2 Pi$$

and gives:

Maradi: H = 2.33; Hmax = 3.14

Zinder: H = 2.67; Hmax = 3.53

Global: H = 2.71; Hmax = 3.58

However, if **$\log_2$** is used, Hmax should be:

$Hmax = \log_2(S)$

For 26 species:

$\log_2(26) \approx 4.70$, not 3.14.

For 38 species:

$\log_2(38) \approx 5.25$, not 3.53.

The reported Hmax values appear much closer to values obtained using **natural logarithm (ln)**.

Required correction

The authors must explicitly state whether they used:

$\log_2$, or

natural logarithm (ln).

The Shannon index and Hmax must be recalculated consistently throughout the manuscript.

2.2 Statistical significance of Shannon diversity is incorrectly presented

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The manuscript states:

"P-value < 0.001" for H, Hmax and E

and concludes that the differences between Maradi and Zinder are statistically significant.

This requires substantial clarification.

A Shannon index is a **descriptive diversity measure**. Simply comparing H with Hmax and reporting P < 0.001 does not demonstrate that diversity differs significantly between Maradi and Zinder.

Required correction

The authors should specify:

What statistical test was used?

What were the sample units?

Was the test based on individual plots?

Were Shannon indices calculated separately for each plot?

Was a bootstrap, permutation test, Hutcheson's t-test, or another appropriate method used?

The statement that **P < 0.001 proves ecological differences between the two regions is currently inadequately supported**.

2.3 Contradiction concerning the number of species

The manuscript reports:

26 species in Maradi

38 species in Zinder

34 species for the biological and phytogeographical spectrum.

This may be possible if 34 represents the **pooled number of species used for a particular classification**, but it is not adequately explained.

If 26 + 38 = 64 observations of species occurrence, there must be substantial overlap between regions. Since Sørensen similarity is 0.75, the number of shared species can indeed be approximately 24, giving a pooled richness of approximately 40 species—not necessarily 34.

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Required correction

Provide a clear table showing:

species occurring only in Maradi;

species occurring only in Zinder;

species common to both regions;

total pooled species richness.

The origin of the **34 species used in Tables 1 and 2 must be explained**.

2.4 Regeneration definition is inconsistent

The manuscript uses several different thresholds.

Earlier it states:

individuals with diameter <5 cm were considered regeneration.

But later the regeneration rate is defined using:

Ø <4 cm for shrubs, or 10 cm for trees.

These definitions are not equivalent.

Required correction

The authors must establish **one operational definition of regeneration** and apply it consistently.

They should explain:

diameter threshold;

whether the threshold differs between trees and shrubs;

why the threshold was selected;

whether the same threshold was used for *A. senegalensis* and the whole woody community.

This issue directly affects the reported regeneration percentages.

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2.5 Serious numerical inconsistency in regeneration results

Table 4 reports:

A. senegalensis

Zinder:

88 young / 111 total × 100 = **79.3%**

Maradi:

24 young / 51 total × 100 = **47.1%**

These calculations are correct.

However, for the total woody population:

Zinder:

74 / 97 × 100 = **76.3%**

Maradi:

93 / 131 × 100 = **71.0%**

The figure appears to report approximately **69.1%** for Maradi and **73.2%** globally.

The global value also requires checking because:

83.5 juveniles/ha + 30.5 adults/ha = 114 individuals/ha

and:

83.5/114 × 100 ≈ **73.2%**, not 69.1%.

Required correction

Recalculate all regeneration percentages and ensure that:

Table 4;

Figure 4;

Abstract;

Results;

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Discussion;

Conclusion

contain exactly the same values.

3. IMPORTANT ISSUE WITH WEIBULL INTERPRETATION

The Weibull analysis requires substantial revision.

The manuscript reports:

Maradi: *A. senegalensis* c = 3.16

Zinder: *A. senegalensis* c = 2.78

and interprets these values as indicating:

"a strong representation of small-diameter individuals."

This interpretation is not sufficiently justified.

A Weibull **shape parameter alone should not automatically be translated into a high abundance of young trees**. The interpretation depends on the complete fitted distribution, including the location and scale parameters and the observed frequency distribution.

Furthermore, the manuscript contains contradictory statements such as:

"$1 < c < 3.6$ is characteristic of populations with predominance of young or small-diameter individuals"

while later stating:

"$c > 3.66$ is characteristic of older individuals."

This classification requires a precise reference and should be applied consistently.

Required correction

Provide a complete Weibull table containing:

location parameter **a**;

scale parameter **b**;

shape parameter **c**;

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goodness-of-fit statistic;

P-value;

observed vs expected frequencies.

The authors should also provide the statistical basis for the selected interpretation of c.

4. The Weibull equation is incorrectly formatted

The equation around lines 204–218 is corrupted:

$f(x) = c\ b\ (3-a/b)c-1\ exp...$

It is difficult to determine the intended mathematical expression.

Required correction

Rewrite the equation professionally and define every parameter.

The authors should also check whether the Weibull equation is mathematically correct for the parameterization used in their software.

5. Methodological concern: sampling design is insufficiently described

The manuscript states:

"An échantillonnage systématique suivant des transects orientés sud-nord et est-ouest…"

but the sampling procedure is not sufficiently reproducible.

Important information is missing:

How many plots were established in each village?

How were the first plots selected?

Were plots located in agricultural fields, fallows, forest areas, grazing lands or mixed landscapes?

Were environmental strata considered?

Were plots independent?

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How was pseudoreplication avoided?

What was the distance between transects?

Were the 300-m intervals maintained everywhere?

Recommendation

Add a detailed sampling design table containing:

Region	Villages	Plots	Area sampled	Habitat types	Mean plots/village
Maradi	6	—	—	—	—
Zinder	8	—	—	—	—
Total	14	142	35.5 ha	—	—

6. Environmental variables were collected but not analysed

The authors state that they recorded:

geographical coordinates;

Braun-Blanquet cover;

geomorphology;

soil characteristics.

However, these variables are almost completely absent from the analysis.

This is a missed opportunity because they could explain why *A. senegalensis* performs differently between Maradi and Zinder.

Recommendation

Either:

A. Analyse these environmental variables, or

B. Remove them from the methodological description if they were not used.

A stronger manuscript would examine relationships between regeneration and:

rainfall;

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soil properties;

land-use type;

disturbance;

grazing pressure;

agricultural intensity.

7. Causal conclusions are stronger than the data support

The authors repeatedly state that Zinder is:

"better conserved and ecologically more stable"

and suggest that Maradi is more affected by anthropogenic pressure.

However, the study does not appear to have directly measured:

harvesting intensity;

grazing intensity;

fire frequency;

agricultural expansion;

wood collection;

protection status;

farmer management practices.

Therefore, these explanations remain **hypotheses rather than demonstrated causes**.

Required correction

Use cautious language such as:

> "The higher diversity observed in Zinder may be associated with differences in environmental conditions and anthropogenic pressure; however, these factors were not directly quantified in the present study."

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8. High regeneration does not necessarily prove long-term population stability

The manuscript concludes that *A. senegalensis* has:

> "a strong capacity for renewal"

and describes the population as:

> "écologiquement résiliente."

High numbers of seedlings/young individuals do indicate recruitment potential, but **recruitment alone does not establish successful population persistence**.

The authors have not demonstrated:

survival of seedlings;

transition from juvenile to adult;

mortality rates;

growth rates;

recruitment into reproductive classes;

seed production;

long-term population trends.

Required correction

Replace strong claims of resilience with:

> "The observed regeneration indicates a potentially favourable recruitment capacity."

This is scientifically more defensible.

9. The IVI results need better interpretation

The IVI of *A. senegalensis* is:

4.08% in Maradi

6.20% in Zinder.

The manuscript correctly notes that the species is not dominant.

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However, IVI is presented as a percentage even though the manuscript defines IVI as a value ranging from 0–300.

The authors should clarify whether they calculated:

IVI = relative density + relative frequency + relative dominance

or a normalized IVI.

Required correction

Report the three components separately:

relative density;

relative frequency;

relative dominance/basal area.

Then provide the calculated IVI.

10. Taxonomic nomenclature needs updating

The manuscript uses:

Mimosaceae

Caesalpiniaceae

Combretaceae

The first two are traditionally used family classifications but are not consistent with modern APG-based taxonomy.

For example, Mimosaceae and Caesalpiniaceae are generally treated as subfamilies within **Fabaceae** in modern classifications.

Recommendation

Use an accepted modern taxonomic system and explain the classification source.

11. Introduction is too long and somewhat repetitive

The Introduction contains several general statements about:

Sahel degradation;

climate variability;

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woody vegetation;

assisted natural regeneration;

anthropogenic pressure.

However, the specific scientific gap concerning *A. senegalensis* is not presented sharply enough.

Recommended structure

The Introduction should progress as:

Importance of woody vegetation in Niger.

Ecological and socioeconomic importance of *A. senegalensis*.

Existing knowledge about the species.

Knowledge gap in Maradi and Zinder.

Specific research questions/hypotheses.

Objectives.

12. Objectives/research questions should be explicitly stated

The manuscript would benefit from a clear statement such as:

> "The objective of this study was to compare the floristic diversity, importance, regeneration and demographic structure of *A. senegalensis* populations in Maradi and Zinder."

Specific hypotheses could include:

Floristic diversity differs between Maradi and Zinder.

A. senegalensis has different regeneration rates between regions.

Diameter structure indicates different demographic patterns between the two regions.

13. Discussion is overly repetitive

Several conclusions are repeated multiple times:

Zinder has higher diversity.

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A. senegalensis has high regeneration.

anthropogenic pressure may affect Maradi.

small-diameter individuals indicate regeneration.

The Discussion should instead critically compare the findings with previous studies and explain **why the current findings agree or disagree with previous research**.

14. Figures and tables require improvement

Several figures are referenced but their actual content is not available in the supplied manuscript text.

In particular:

Figure 1: study area

Figures 2–3: family distribution

Figures 5–8: demographic distributions

The figures should include:

readable axis labels;

units;

sample size;

confidence intervals where appropriate;

observed vs fitted Weibull distributions;

legends;

statistical information.

15. References require careful verification

There are several bibliographic inconsistencies and incomplete references.

Examples include:

inconsistent journal titles;

inconsistent author formatting;

missing DOI information where appropriate;

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duplicated or incomplete references;

references cited in text that may not appear consistently in the bibliography;

questionable formatting such as "J.Anim.PlantSci." and "Journal of Applied Biosciences210".

Recommendation

The entire reference list should be checked against the original publications and formatted according to the target journal's style.

16. Language and formatting problems

There are numerous problems with

spacing;

italicization of scientific names;

missing spaces between words;

French/English mixing;

capitalization;

punctuation;

decimal notation;

mathematical formatting.

Examples:

"Annonasenegalensis"

should consistently appear as:

Annona senegalensis

Similarly, expressions such as:

"A. senegalensisest"

must be corrected to:

"*A. senegalensis* est"

The English Abstract also requires substantial language editing by a fluent scientific English speaker.

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17. KEY SCIENTIFIC POINTS FOR THE AUTHORS TO ADDRESS

The following points should be treated as **essential revisions**:

Major points

Recalculate Shannon diversity and Hmax using a consistent logarithm.

Explain exactly how the reported **P < 0.001** values were obtained.

Resolve the discrepancy between **26 + 38 species and the 34 species used for spectra**.

Correct the **regeneration-rate definitions and calculations**.

Correct the discrepancies between Table 4, Figure 4, Abstract and Conclusion.

Reanalyse and clarify the **Weibull distribution and interpretation of c**.

Provide complete Weibull parameters and goodness-of-fit statistics.

Clarify the sampling design and replication.

Avoid claiming that Zinder is "better conserved" unless anthropogenic pressure was actually measured.

Avoid interpreting high regeneration as definitive evidence of long-term population stability.

Clarify IVI calculation and report its components.

Update taxonomic nomenclature.

Improve the statistical analysis and provide appropriate tests

Improve figures and tables.

Thoroughly verify and standardize references.

18. SIGNIFICANCE OF THE STUDY

The study has **good potential significance** because it provides field-based information on a culturally and economically important woody species in a region where vegetation dynamics and natural regeneration are important conservation concerns.

The comparison between Maradi and Zinder is potentially valuable because it demonstrates substantial differences in:

woody species richness;

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diversity;

regeneration;

demographic structure;

abundance of *A. senegalensis*.

If the methodological and statistical problems are corrected, the work could provide useful information for:

conservation planning;

assisted natural regeneration;

agroforestry management;

restoration of degraded lands;

sustainable harvesting of *A. senegalensis*;

community-based forest management.

However, the current manuscript **overstates some ecological interpretations relative to the evidence actually presented**.

19. RECOMMENDATION TO THE EDITOR

MAJOR REVISION

Recommendation: Major Revision, not rejection.

Justification

The manuscript contains a potentially valuable field dataset and addresses an important topic concerning the diversity, regeneration and demographic structure of *Annona senegalensis* in the centre-south of Niger. The comparison of Maradi and Zinder and the combination of floristic, demographic and regeneration analyses provide scientific and practical value.

However, the manuscript in its current form has **important numerical and methodological inconsistencies**, particularly concerning the Shannon/Hmax calculations, statistical significance, regeneration definitions and percentages, species totals, Weibull analysis and interpretation of demographic structure. Several conclusions concerning ecological stability, anthropogenic pressure and resilience are also stronger than what can be directly supported by the presented data.

Therefore, I recommend Major Revision. The authors should first correct the numerical and statistical inconsistencies, clarify the sampling and analytical procedures, re-evaluate the Weibull interpretation, moderate unsupported causal statements, and undertake comprehensive language and reference editing.

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After these corrections, the manuscript could become a useful contribution to the literature on Sahelian woody vegetation and *Annona senegalensis* conservation.

Suggested confidential comment to the editor

> **The manuscript has a potentially publishable dataset and a relevant conservation topic, but substantial revision is necessary. I do not recommend rejection because the field sampling is relatively extensive and the results may be useful. However, the inconsistencies in diversity calculations, regeneration definitions/results and Weibull interpretation must be resolved before the scientific validity of the conclusions can be assessed. I therefore recommend Major Revision.**

Yes. **Major Revision is justified** because the manuscript has several important problems in **methodology, statistical analysis, internal consistency, interpretation, terminology, and presentation**. The study has potentially useful field data, but the authors need to correct these issues before the results can be considered reliable.

Below is a **line-by-line reviewer justification**, with the **issue and reason** linked to the manuscript line numbers.

Reviewer Recommendation: MAJOR REVISION

Overall justification

The manuscript addresses an ecologically relevant topic and contains substantial field data from Maradi and Zinder. However, there are significant methodological and analytical inconsistencies that affect the validity of the reported diversity indices, regeneration rates, Weibull interpretation, statistical significance, and ecological conclusions. Several definitions used in the Methods are inconsistent with the actual field measurements, while some conclusions appear stronger than what the presented data support. The manuscript also requires substantial correction of botanical nomenclature, equations, references, figures/tables, statistical procedures, and English/French scientific presentation.

LINE-BY-LINE MAJOR REVISION COMMENTS

Line(s)	Issue identified	Reason / Required revision
1–2	Title says **"2 peuplements de Annona senegalensis"**	The manuscript actually compares *A. senegalensis* populations/stands within two regions and associated woody vegetation. The terminology "peuplements" should be clearly defined. Consider revising the title to indicate **Maradi and Zinder populations/stands**.
5–30	Abstract contains several unsupported conclusions	The abstract states "potentiel de renouvellement important" and particularly strong renewal at Zinder, but no statistical comparison or confidence intervals for regeneration are adequately presented. Conclusions should be restricted to the evidence provided.
10–13	Sampling description insufficient	The abstract gives 142 plots but does not explain how many plots were located in each village/region or whether

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Line(s)	Issue identified	Reason / Required revision
		sampling was random, systematic, or stratified. This information is essential for reproducibility.
14–20	Diversity results are difficult to reconcile with the reported species numbers	Maradi has 26 species and Zinder 38 species, yet the Shannon/Hmax values need verification because the manuscript uses inconsistent logarithm conventions.
16–18	Biological spectrum based on **34 species**, whereas total richness is 26 + 38	Explain why only 34 species were used for the biological/phytogeographical spectrum. Are these 34 common species, pooled species, or only species with available information? A complete species list should be supplied.
19–20	Shannon and Pielou values require recalculation	The manuscript defines Shannon using **$\log_2$**, but the reported Hmax values (3.14 for 26 species and 3.53 for 38 species) do not correspond to $\log_2$. If natural logarithm is used, $\ln(26) \approx 3.26$ and $\ln(38) \approx 3.64$. The authors must specify the logarithm and recalculate H, Hmax and Pielou consistently.
21–22	Regeneration terminology is inconsistent	The abstract reports 24 and 88 young individuals/ha and regeneration rates of 47.1% and 79.3%. These percentages correspond approximately to 24/(27+24) and 88/(23+88), respectively, but the methodology later defines regeneration differently. The calculation must be explicitly demonstrated.
23–25	Weibull interpretation requires correction	The manuscript states that c = 3.16 and 2.78 indicate a "strong representation of low-diameter individuals." However, Weibull shape parameters around these values generally indicate a unimodal distribution rather than automatically demonstrating abundant young individuals. The actual diameter-frequency histograms must support this interpretation.
32–54	English Abstract requires substantial language editing	There are numerous spacing and grammar problems: "woodyspecies," "This studyassessed," "wereused," etc. The abstract should be professionally edited by a fluent scientific English speaker.
57–64	Introduction lacks precise research gap	The authors state that data are limited, but do not clearly identify what previous studies have already established specifically for *A. senegalensis* in Niger and what exact knowledge gap this study fills.
65–72	"Reverdissement du Sahel" discussion is too general	The link between regional greening and the specific *A. senegalensis* populations studied is not sufficiently established. The authors should distinguish remote-sensing trends from species-level demographic observations.
73–79	Anthropogenic causes are asserted without measurements	Expansion of agriculture, wood-energy exploitation and overgrazing are proposed as causes, but the study apparently did not quantify these pressures. These statements should either be supported by measured variables or presented as hypotheses supported by literature.
80–85	Claims concerning decline of several species need evidence	The manuscript mentions regression of *Balanites aegyptiaca*, *Faidherbia albida* and *Ziziphus mauritiana*, but no historical

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		data from the present sampling sites are provided. Avoid implying that the current study demonstrated decline.
86–92	RNA discussion is disconnected from the actual methodology	Regeneration Natural Assisted is emphasized strongly, but RNA practices do not appear to have been measured systematically in the study. The authors should explain whether RNA was an explanatory variable or merely background context.
93–104	Claims of local disappearance need evidence	The statement that *A. senegalensis* has disappeared locally in northern Zinder and Maradi requires specific references or field evidence. Otherwise, it should be softened.
100–104	"Espèce d'importance socio-économique majeure" needs supporting quantitative evidence	If socio-economic importance is a central justification, the manuscript should provide appropriate ethnobotanical references or explain the basis for this classification.
105–110	Objectives/hypotheses are insufficiently explicit	The introduction should clearly state the research questions/hypotheses, e.g. whether diversity, density, regeneration and demographic structure differ between Maradi and Zinder.
113–118	Study-site description is repetitive	Lines 113–118 repeat essentially the same information twice. Remove duplication and provide a concise, complete description.
119–126	Environmental description insufficient	Exact coordinates, elevation, soil characteristics, annual rainfall range, temperature and vegetation type should be provided for each study region/site.
120–122	Rainfall statement is incomplete	"300–600 mm/an)" has a missing parenthesis and is too broad for comparison between sites. Actual rainfall data for the sampling period and long-term averages should be provided.
123–125	Population/ethnic information appears unnecessary	Ethnic composition is mentioned but is not used analytically. Either explain its relevance to vegetation management or remove it.
131–133	Sampling design requires clarification	"Transects orientés sud-nord et est-ouest" and systematic plots every 300 m are insufficiently described. Explain how the initial transect was established, how starting points were selected, and how villages were selected.
131–133	Potential pseudoreplication	142 plots are nested within only 14 villages. Treating all 142 plots as independent observations without accounting for village-level clustering may inflate statistical significance. A mixed-effects or hierarchical approach should be considered.
132–133	Allocation of plots is missing	State how many plots were established in each village and each region. Unequal sampling effort could affect direct comparison of diversity and density.
134–139	Soil and environmental variables were collected but not analyzed	The authors say geomorphology and soil characteristics were recorded, but no results are presented. Either analyze these variables in relation to species distribution/regeneration or remove the claim from Methods.

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Line(s)	Issue identified	Reason / Required revision
136–140	Diameter measurement protocol is inconsistent	Trees are measured at 1.30 m while shrubs are measured at 20 cm. This is acceptable only if clearly justified and consistently applied. The authors must define how multi-stemmed individuals were treated.
139–142	Regeneration threshold is **<5 cm**, but later the regeneration formula uses **<4 cm for shrubs and <10 cm for trees**	This is a major methodological inconsistency. The same regeneration criterion must be used throughout the study, or separate analyses must be clearly justified.
141–142	Five regeneration subplots per 2,500-m² plot	Explain why the four corners and center were selected, whether subplot locations were standardized, and whether edge effects were controlled.
144–158	Biological classification needs major clarification	The manuscript states that Raunkiaer's classification is based on the position of renewal buds but then defines categories solely by plant height. This is conceptually problematic. The authors must provide the exact classification used by Saadou and explain how it was operationalized.
150–152	Possible incorrect biological terminology	The definitions "nanophanérophytes = 2–8 m" and "microphanérophytes = 8–30 m" require verification against the cited classification. The conventional Raunkiaer terminology should not be altered without a clear reference to the Niger-adapted classification.
153–158	Phytogeographical classification needs source verification	Explain how each species was assigned to SZ, SS, Saharo-Sindian, Afro-tropical or pantropical categories and provide a species-by-species supplementary table.
159–176	Shannon equation is incorrectly formatted	The equation is displayed as "H = − Pi log2 pi" with formatting problems. More importantly, the logarithm used must be consistent with Hmax and Pielou.
164–168	Incorrect/generalized statement about Shannon values	Saying that Shannon values are "frequently between 1 and 5" is not an appropriate methodological definition. Shannon entropy has no universal fixed upper limit; its maximum depends on species richness and logarithm base.
166–168	Hmax must be recalculated	Hmax = log(S), but the base must match the Shannon calculation. This is a significant numerical issue affecting the reported Pielou values.
177–182	Pielou interpretation is broadly acceptable but depends on corrected H/Hmax	Once Shannon/Hmax are corrected, Pielou values should be recalculated.
183–187	Sørensen analysis incomplete	Only one Sørensen value (0.75) is reported. Provide the actual species lists and number of shared species so the calculation can be independently verified.
188–198	IVI methodology needs clarification	The formula IVI = relative density + relative dominance + relative frequency is appropriate, but the authors should state whether IVI is reported on a 0–300 scale or converted to a percentage.
189–	IVI values are presented as	If IVI is calculated as three components each ranging 0–100,

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198	percentages	the resulting value ranges from 0–300 and is not technically a percentage unless explicitly normalized. Calling 4.08% and 6.20% "IVI" is therefore potentially misleading.
200–223	Weibull methodology is poorly presented	The Weibull equation is corrupted/incomplete. The parameters **a, b and c** need to be clearly defined and the exact probability-density equation correctly typeset.
202–204	Choice of 18 diameter classes needs justification	Explain the minimum and maximum diameter and why 18 classes of 5 cm were selected. A histogram should clearly show the observed and fitted distributions.
208–218	Weibull interpretation contains contradictory/incomplete text	Line 216 contains missing mathematical information ("1 3,6…"). The classification of c values is incomplete and should be rewritten based on a cited methodological source.
213–219	Interpretation of c is potentially incorrect	A value of c > 1 does not simply mean a "young population." The shape parameter describes distribution form. Age/diameter structure must be interpreted using the actual frequency distribution and ecological context.
220–221	Statistical goodness-of-fit is inadequately described	"Analyse log linéaire" is not enough. Specify the statistical model, null hypothesis, test statistic, degrees of freedom, goodness-of-fit criterion and significance level.
222–223	Population versus *A. senegalensis* analysis unclear	Clarify whether Weibull models were fitted separately for each region and each taxonomic group.
224–229	Regeneration formula is inconsistent with field definition	This is one of the most serious methodological problems. Field regeneration is defined as <5 cm, whereas the formula later uses <4 cm for shrubs and <10 cm for trees. Correct throughout.
227–229	"Equilibrium" definition is oversimplified	A regeneration percentage >50% cannot by itself establish that a population is sustainable or in equilibrium. Recruitment, mortality, growth and transition between size classes should be considered.
231	Results and Discussion heading structure should be improved	Separate Results and Discussion more clearly. Avoid interpretation in Results.
235–241	Family nomenclature requires updating	"Mimosaceae" and "Caesalpiniaceae" are historically used classifications and are now generally treated within Fabaceae. If the older classification is intentionally used, this should be explicitly stated and consistently applied.
236–241	Species totals need verification	Provide a complete species inventory as supplementary material. This would allow verification of family totals, richness and diversity calculations.
251–271	"34 species" remains unexplained	The manuscript reports 26 species in Maradi and 38 in Zinder but analyzes biological/phytogeographical spectra using 34 species. This must be explained.
263–267	Biological spectrum percentages should be independently verified	19 + 12 + 3 = 34 and the percentages sum correctly, but the manuscript must explain why 34 species were selected.
268–271	Phytogeographical spectrum similarly requires species-level	Provide the identity of the 34 taxa assigned to each category and cite the authoritative flora/database used.

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	verification	
273–285	Major statistical problem with diversity comparison	The manuscript gives $P < 0.001$ for H, Hmax and E, but does not describe the statistical test used. Shannon indices are not normally tested simply by comparing the numerical values. The statistical method must be specified and justified.
275–281	"Global H = 2.71" requires clarification	Explain how the pooled/global diversity was calculated. A pooled Shannon index cannot be casually compared with site-level values without describing the pooling procedure.
281–284	P-values for Hmax are questionable	Hmax is mathematically determined by species richness, so treating it as an independently measured variable and reporting a P-value requires justification.
283–284	"Difference statistically significant" is inadequately supported	The manuscript must state the exact statistical test, test statistic, degrees of freedom and assumptions.
285	Sørensen value of 0.75 requires verification	With 26 and 38 species, a Sørensen coefficient of 0.75 implies approximately 24 shared species. This should be confirmed against the actual species inventory.
287–294	IVI of *A. senegalensis* requires component values	Report relative density, relative frequency and relative dominance separately for *A. senegalensis*. Without these, the IVI cannot be independently evaluated.
293–294	Conclusion about "not dominant" is reasonable but should be quantified	The authors should explain whether IVI was statistically compared or simply ranked descriptively.
296–302	Major inconsistency in regeneration interpretation	The text states that total woody regeneration is "significantly more important at Maradi than Zinder ($P = 0.002$)," but no statistical test or contingency table is presented.
297–300	Density data need uncertainty estimates	Report mean ± SD/SE or confidence intervals and sample size for density estimates. A single density value is insufficient for ecological comparison.
301–302	$P = 0.002$ is unexplained	Identify the statistical test used and its assumptions.
303–306	Table 4 needs clearer structure	Separate adult, juvenile and total densities, provide units, sample size, variability and statistical comparisons.
307–315	Figure 4 is not sufficient for statistical inference	A bar chart of regeneration percentages should include error bars and sample size. If significance is claimed, statistical results should be presented.
318–325	Weibull interpretation again needs verification	The statement that the diameter distribution is "en cloche" and simultaneously indicates young individuals should be supported by the actual frequency distribution.
326–330	Mathematical interpretation is unclear	"$1.50 \leq c \leq 3.66$" is presented as a category without a complete reference or justification. The manuscript should cite the exact classification used for interpreting c.
327–330	"Asymétrique négative ou asymétrique gauche" requires statistical support	Do not infer skewness solely from the Weibull c parameter without explaining the relationship used. Report observed skewness or the fitted distribution characteristics if this

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		interpretation is retained.
328–330	Height distribution is inadequately described	The authors report a range "2 < c < 4.28" but do not provide individual c values for Maradi, Zinder and each population. Provide the complete results.
355 onward	Discussion frequently overinterprets observational data	Several causal explanations are proposed—anthropogenic pressure, rainfall, RNA, soil quality and climate—without these variables being directly analyzed. Conclusions should distinguish measured findings from hypotheses.
356–363	Literature comparison needs stronger specificity	The discussion should compare actual densities, richness, regeneration and structural parameters with previous studies, rather than only stating that the pattern is "similar."
364–372	Phytogeographical interpretation is plausible but needs better evidence	Explain whether the observed SZ dominance differs significantly from other regional studies and whether sampling location explains the pattern.
373–377	"Bonne adaptation" is too strong	Classification as a phytogeographical type does not by itself demonstrate adaptation or ecological fitness. This statement should be moderated.
378–401	Statistical significance is overinterpreted	$P < 0.001$ does not demonstrate that ecological differences are "real" without a valid statistical design. Statistical significance also does not establish ecological stability or conservation status.
385–387	Causes of higher diversity in Zinder are speculative	"Less anthropogenic pressure," "better conditions," and "greater resilience" were not measured. Present these as possible explanations rather than conclusions.
391–398	High Pielou is equated with stability/resilience	Evenness alone cannot demonstrate ecological stability, resilience or lower disturbance. This interpretation should be substantially reduced or supported by appropriate ecological measurements.
399–407	P-values are not adequately documented	The manuscript must provide the statistical procedure behind $P < 0.001$ and $P = 0.002$.
402–420	Anthropogenic pressure/RNA claims need direct evidence	If these factors explain differences between Maradi and Zinder, they should be measured—for example, grazing intensity, wood harvesting, land-use type, RNA status or management practices. Otherwise they belong in the limitations/future research section.
424–434	Low IVI + high regeneration does not prove long-term resilience	High recruitment is encouraging but does not establish long-term population persistence. Mortality and recruitment into adult classes were not followed longitudinally.
435–441	Statements about fire and grazing tolerance require appropriate evidence	These ecological traits should be referenced specifically for *A. senegalensis* rather than inferred from general species descriptions.
442–446	"Ecologically resilient" is an overstatement	The study is essentially observational/cross-sectional. Demonstrating resilience requires evidence of response/recovery following disturbance or temporal monitoring.

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Line(s)	Issue identified	Reason / Required revision
447–454	Major inconsistency in Weibull ranges	Earlier lines report diameter c values of 1.66 and 3.16 at Maradi and 1.50 and 2.78 at Zinder, but the discussion later states a range of 1.50–3.16 and then mentions 2.45–4.28 for height. These values need to be tabulated clearly.
451–454	Height c > 3.66 interpreted as "stable and well developed"	A Weibull shape parameter alone does not demonstrate ecological stability. Rephrase this as a description of distribution concentration.
455–458	"Future of natural formation is assured" is too strong	A high proportion of small individuals does not guarantee recruitment to reproductive adults. Longitudinal monitoring is needed.
460–481	Conclusion exceeds evidence in several places	The conclusion should distinguish observed facts from interpretations. In particular, "dynamic de renouvellement encourageante" is reasonable, but long-term conservation cannot be inferred from regeneration alone.
473–476	Recruitment bottleneck is acknowledged but not measured	The manuscript correctly notes that transition from juvenile to adult is important, but this should be presented as a limitation because the study did not follow cohorts through time.
477–480	Management recommendations are not directly evaluated	Recommendations for RNA and protection are reasonable, but should be framed as management implications rather than experimentally demonstrated interventions.
483–616	Reference list requires extensive verification	Several references have incomplete bibliographic information, inconsistent formatting, possible journal/title inconsistencies, and inconsistent author names. Every citation must be checked against the original publication.
487–490	Reference formatting incomplete	Author names and journal formatting need standardization.
503–505	Potentially problematic reference entry	Two journal references appear merged into a single bibliographic entry ("African Journal of Ecology… Journal of Applied Biosciences…"). Verify and correct.
512–513	Braun-Blanquet reference should be verified	Check original publication details and whether the cited source actually supports the precise scale used.
515–518	Brandt et al. citation needs verification	Ensure title, journal, volume and pages exactly match the cited paper.
523–524	Meteorological reference not cited in text appropriately	If meteorological data were used, specify source, station, period and data-processing method.
530–531	FAO reference appears unrelated to the stated regeneration methodology	The cited FAO "State of World's Soil Resources" reference should not automatically support a vegetation regeneration formula. Use the original source for the formula.
542–544	Important reference potentially relevant to the study is not adequately integrated	If this 2023 *A. senegalensis* conservation study exists, the authors should clearly explain how the current study differs from it.
549–555	Thesis references need consistent formatting	Standardize institutional names, year, title and pagination.
569–	Reference incomplete	"Dynamique des écosystèmes sahéliens" lacks sufficient

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Line(s)	Issue identified	Reason / Required revision
571		bibliographic details.
603–604	Recent reference should be checked carefully	Verify article title, journal, volume and article number.
609–611	Duplicate/inconsistent Tropenbos citation	Lines 609–611 appear to duplicate the same report. Remove duplication.

PARTICULARLY SERIOUS SCIENTIFIC ISSUES

I would emphasize the following **10 issues as the main reasons for Major Revision**:

1. Shannon index calculation is internally inconsistent

This is a **major numerical issue**.

The manuscript states:

$$H = -\Sigma\ Pi \log_2 Pi$$

but gives:

Maradi: **Hmax = 3.14** for 26 species

Zinder: **Hmax = 3.53** for 38 species

Global: **Hmax = 3.58**

These values do **not correspond to $\log_2(S)$**.

For example:

$\log_2(26) \approx$ **4.70**

$\ln(26) \approx$ **3.26**

$\log_2(38) \approx$ **5.25**

$\ln(38) \approx$ **3.64**

Therefore, the authors need to **recalculate H, Hmax and Pielou's E using one consistent logarithm base**.

2. Regeneration definition is contradictory

The field methodology says:

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individuals with diameter <5 cm were considered regeneration.

But line 225–226 states:

Ø <4 cm for shrubs, or <10 cm for trees

This is a **major methodological inconsistency**.

The authors must decide which threshold was actually used and recalculate the regeneration results if necessary.

3. Biological-type classification requires correction/verification

The manuscript says:

nanophanérophytes = 2–8 m
microphanérophytes = 8–30 m

This requires verification against the **exact Saadou (1990) adaptation** being used.

Raunkiaer's classification is fundamentally based on **position of renewal buds**, not simply plant height. If a Niger-specific modification based on height was used, the authors must explicitly cite and describe that modification.

4. Statistical significance is inadequately supported

The manuscript repeatedly reports:

$P < 0.001$

and

$P = 0.002$

but does not adequately identify:

statistical test;

null hypothesis;

test statistic;

degrees of freedom;

assumptions;

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sample size;

whether multiple comparisons were performed.

This is particularly important because **142 plots are nested within 14 villages**, so independence of observations needs consideration.

5. IVI is presented incorrectly as a percentage

The manuscript defines:

IVI = Dr + STR + Fr

Each component ranges from 0–100, meaning traditional IVI ranges from **0–300**.

Therefore, an IVI of 4.08 should generally be reported as an **IVI value of 4.08**, not automatically as **4.08%**, unless the authors explicitly normalize the index.

6. Weibull interpretation is insufficiently justified

The manuscript equates c values such as:

1.66

3.16

1.50

2.78

with a strong presence of young individuals.

This is too simplistic. The authors should provide:

observed diameter distribution;

fitted Weibull distribution;

parameters **a, b and c**;

goodness-of-fit statistics;

statistical significance;

justification for the ecological interpretation of c.

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7. Cross-sectional regeneration cannot establish long-term resilience

The manuscript repeatedly uses terms such as:

"population écologiquement résiliente"

and

"maintien à long terme"

But the study appears to be based on observations collected during **2023–2024**, rather than longitudinal demographic monitoring.

High juvenile density indicates **recruitment potential**, not necessarily demonstrated long-term persistence.

8. Causal explanations are not measured

The manuscript attributes differences to:

anthropogenic pressure;

agricultural expansion;

grazing;

wood-energy collection;

rainfall;

soil conditions;

RNA;

climate variability.

However, these variables do not appear to have been quantitatively incorporated into the statistical analysis.

Therefore, the discussion should distinguish between:

Observed result → possible explanation → evidence supporting explanation.

9. "34 species" requires explanation

The manuscript reports

Maradi = **26 species**

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Zinder = **38 species**

biological/phytogeographical spectrum = **34 species**

Why exactly **34**?

The authors must provide a complete species list and explain whether the 34 species represent:

pooled species;

species common to both regions;

species with available phytogeographical information;

or another subset.

This is essential for reproducibility.

10. Species inventory should be provided

A **supplementary table** should contain at least:

Species Family Maradi Zinder Biological type Phytogeographical type

This would allow verification of:

species richness

family richness;

Shannon index;

Pielou;

Sørensen;

biological spectrum;

phytogeographical spectrum.

Suggested Reviewer Comment to the Editor

You can use the following wording in the editorial recommendation:

Recommendation: Major Revision

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The manuscript addresses an important ecological and conservation topic and presents potentially valuable field data on the diversity and demographic structure of *Annona senegalensis* in Maradi and Zinder, Niger. However, substantial methodological and analytical problems currently limit the reliability and reproducibility of the findings. In particular, there are inconsistencies in the calculation and presentation of Shannon diversity and Hmax, contradictory definitions of regeneration thresholds, insufficient description of the statistical analyses, questionable interpretation of Weibull parameters, ambiguity concerning the 34 species used for biological and phytogeographical spectra, and inappropriate overinterpretation of statistical significance and regeneration as evidence of ecological resilience. The IVI presentation also requires clarification, while the botanical nomenclature, figures, equations, sampling design, and reference list require substantial correction. Several causal explanations concerning anthropogenic pressure, climate, soil conditions and assisted natural regeneration are not directly supported by measured variables. Therefore, although the study has potential scientific value, extensive methodological clarification, recalculation of indices, statistical reanalysis, correction of internal inconsistencies, and substantial revision of the Discussion and Conclusions are required before the manuscript can be considered for publication.

Final assessment

Recommendation: MAJOR REVISION — not rejection.

The **field dataset appears potentially publishable**, but I would **not recommend acceptance in the present form**, primarily because the numerical/statistical inconsistencies—especially **Shannon/Hmax/Pielou, regeneration definitions, statistical P-values, and Weibull interpretation**—need to be resolved before the ecological conclusions can be trusted.