

The Editorial Board of HESS

Hydrology and Earth System Sciences

February 15, 2026

Dear editor

Thanks you very much for your letter dated 13 February, 2026 and the comments from referees. Attached please you find our revised manuscript (Ms. Ref. No.: egusphere-2025-4284). We are able to accommodate all comments and suggestions from the referees, and also updated and re-formatted the references. Specifically, we carefully checked and updated the data, figures and language presented in the manuscript. On separated pages, we have a point-by-point response to reviewers' comments. In addition, for your easy review, we also supplied edit tracked main text. I hope you will find the manuscript publishable in the Journal.

Thank you very much for your consideration.

Sincerely Yours,

Zhikang Wang & co-authors

Guizhou Minzu University (GMU)

Dear Zhikang Wang,

We are pleased to inform you that the editor report for the following HESS manuscript is now available:

egusphere-2025-4284

Title: Extreme drought–accelerated dissolved carbon metabolism triggers pulsed CO₂ outgassing in karst lakes

Author(s): Maofei Ni et al.

MS type: Research article

Iteration: Minor revision

The editor has decided that **minor revisions** are necessary before the manuscript can be accepted. We kindly ask you to revise your manuscript accordingly and to upload the revised files, a point-by-point reply to the comments, and a marked-up manuscript version showing the changes made no later than 23 Feb 2026.

Please note that all referee and editor reports, the author's response, as well as the different manuscript versions of the peer-review completion (post-discussion review of revised submission) will be published if your paper will be accepted for final publication in HESS.

In case any questions arise, please do not hesitate to contact me. Thank you very much for your cooperation.

Kind regards,

The editorial support team
Copernicus Publications
editorial@copernicus.org

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COMMENTS FROM THE EDITOR AND/OR REVIEWERS
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Referee #1

Overall, the manuscript is very well prepared, presents a clear structure, and effectively integrates biogeochemical and microbiological approaches to investigate dissolved carbon dynamics and CO₂ emissions in karst lakes. The study addresses a highly relevant and timely topic, particularly in the context of climate change and the increasing frequency of extreme events such as droughts. **I particularly appreciate that the study addresses processes that remain poorly documented in tropical and subtropical regions, where our understanding of aquatic carbon cycling and greenhouse gas emissions is still limited. The results significantly improve our**

understanding of the role of karst lakes in the broader carbon cycle. In general, the manuscript is very solid, and I only have minor suggestions regarding figure presentation. Specifically, separating the map from the boxplots in Figure 1 and harmonizing the boxplot style between Figures 1 and 2 (especially the way statistical significance brackets are displayed) would improve visual clarity and consistency. These are minor points and do not affect the overall conclusions of the study.

Response: *We thank you for your positive comments. Based on your valuable suggestions, we separated the map and the boxplots in Figure 1 and harmonized the boxplot style between Figures 1 and 2, especially the display of statistical significance brackets. Please see the new version of “figures” section for these revisions.*

Referee #2

This preprint reports a 2-year survey of three karst lakes (Guizhou, China) spanning an extreme drought and subsequent wetter periods, linking dissolved carbon (DOC/DIC) dynamics and aquatic microbiome shifts to CO₂ outgassing. Using carbonate-system calculations (CO₂SYS) and a thin boundary-layer gas exchange model, they show drought conditions coincide with much higher pCO₂ and CO₂ efflux than wet periods, alongside DOC and community changes consistent with stronger heterotrophy during drought and more photosynthetic signatures during wet conditions. I find the manuscript interesting and the methodology sound.

Response: *We thank you for your constructive comments. We have thoroughly revised the manuscript in response to your suggestions, which have helped improve the clarity of data interpretation as well as the overall structure and language.*

General comments:

1. I find the hypothesis rather shallow and suggest its revision. As the study is focused on drought and wetting events, I would expect the main hypothesis to address this.

Response: *Based on your suggestion, we removed the overly shallow/general hypothesis and updated the main hypothesis in the Introduction section to focus on event-driven wetting and drought. The revised hypothesis now explicitly links these events to shifts in microbial metabolic strategies, dissolved carbon (DIC-DOC) turnover, and CO₂ dynamic as follows:*

“Because hydrologic wet-dry shifts can restructure aquatic habitats and substrate availability for microorganisms, we thus hypothesized that wet and drought conditions will shift microbial metabolic strategies, thereby modulating dissolved C turnover and CO₂ dynamics in karst lakes.”

The following text was added in the section of Discussion to address our hypothesis:

“Overall, consistent with our hypothesis, wet-dry seasonality shifted microbial metabolic strategies and altered temporally structured turnover between DIC and DOC, thereby regulating CO₂ uptake and evasion in the karst lakes.”

2. The “extreme drought pulse” conclusion is based on capturing one major drought episode; it’s compelling, but the manuscript should be careful about extrapolating

frequency/magnitude of pulsed emissions without additional years/events or external validation.

Response: *Thank you for this helpful comment. Based on your comment, we realized that drought-driven responses of aquatic CO₂ fluxes can vary across spatial and temporal scales (Leng et al., 2025; Zhao et al., 2025), and therefore estimating the frequency / magnitude of pulsed emissions requires additional years/events or external validation. Therefore, we now explicitly clarify that the “extreme drought pulse” conclusion is based on a single major drought episode observed during the study period. Accordingly, we refrain from extrapolating the frequency/ magnitude of pulsed emissions beyond this event. The relevant information in the sections of Abstract, Results, Discussion and Conclusion was thoroughly revised as follows:*

Abstract section:

“We therefore propose that during the observed extreme drought, pulses of CO₂ emissions from the study karst lakes occur after wet conditions end, as a consequence of rapid remineralization of newly produced bioavailable organic C.”

Results section:

“We show that the extreme drought increased areal CO₂ efflux sharply (111 ± 104 mmol m⁻² d⁻¹), relative to initial- (6 ± 22 mmol m⁻² d⁻¹) and post-wet periods (10 ± 26 mmol m⁻² d⁻¹) in our observed episode ($p < 0.001$).”

Discussion section:

“In this study, we propose that the observed extreme-drought event yielded large CO₂ emissions from the karst lakes, increasing >10 times on average relative to wet conditions (Fig. 1d).”

Conclusion section:

“These specific microbial strategies indicate that in our observed episode, pulsed CO₂ outgassing from karst lakes may occur after periods of high photosynthetic activity, with the magnitude being significantly amplified during extreme droughts.”

3. I think the authors should at least discuss the limitations of surface water sampling.

Response: *Based on your comment, we added text discussing the spatial and temporal limitations of our surface water sampling in the “Data Quality” section as follows:*

“Water collection and analysis followed the standard procedures, as proposed by the American Public Health Association (1985). However, we acknowledge that (1) temporally, discrete surface sampling may omit short-lived C dynamics driven by diel variability and episodic event (e.g., flooding and thermal fluctuations); and (2) spatially, surface measurements at limited stations may not fully represent horizontal heterogeneity (e.g., differences in macrophytes and local human activities). Therefore, we interpret our results as evidence for surface water patterns during the observed period and avoid extrapolating the frequency and magnitude of CO₂ pulses without higher-frequency and multi-site observations.”

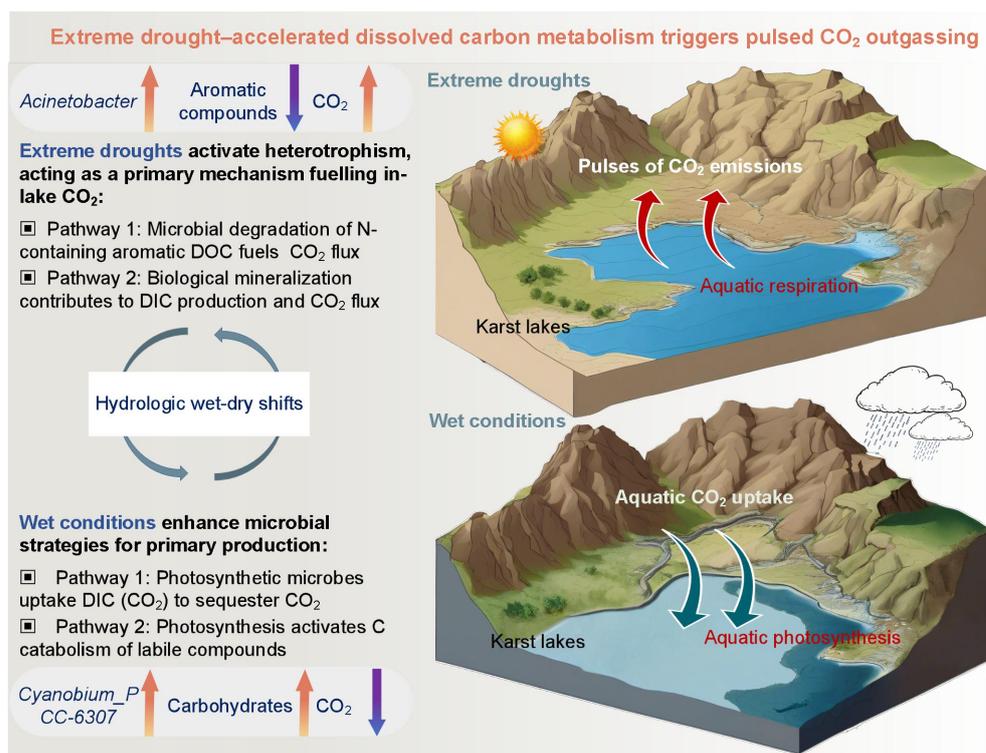
4. The English throughout the manuscript needs some improvement. Many sentences have mistakes that harm the readers understanding and ultimate flow of the text.

Response: *We have carefully revised the English throughout the manuscript to improve clarity and readability, corrected grammatical errors and awkward phrasing, and improved the overall flow. The English was polished by a native speaker throughout the manuscript. All changes have been incorporated in the revised manuscript.*

Specific comments:

5. I think that the graphical abstract should be simplified a little, particularly the left side. What do the arrow colors and formatting represent? What are the numbers? It is all so small and packed together that the differences between conditions are unclear.

Response: *We realized that the previous Graphical Abstract was too crowded, especially on the left-hand side (structural equation model, SEM), and that the arrow colors (red: positive effect; blue: negative effect), formatting, and the numbers (path coefficients) were difficult to interpret at the reduced size. To improve readability, we simplified the Graphical Abstract by removing the left-hand SEM panel and replacing it with clear visual symbols and brief text that convey the same key message. The updated Graphical Abstract is shown below.*



6. Furthermore, there are some grammar mistakes. Examples of unclear sentences:

Response: *Thank you for your careful review. We revised the sentences highlighted in your examples and carefully checked the similar mistakes to improve clarity and readability throughout the manuscript.*

Line 64 – “heterotrophic respiration of DOC”? Perhaps rephrase?

Response: *Changed “heterotrophic respiration of DOC” to “heterotrophic*

respiration fuelled by DOC”. The similar phrasing was also revised throughout the manuscript.

Lines 80-83 – “related to aquatic biology” seems very general and vague...

Response: *We clarified the general and vague term “aquatic biology” by specifying it as “microbially mediated carbon fixation and mineralization”. The sentence has been revised as follows:*

“These critical processes are tightly linked to microbially mediated C fixation and mineralization, but little is known about how lake microorganisms drive dissolved C turnover and ultimately modulate CO₂ emissions in karst lakes.”

Lines 84-85 – “Theoretically, microbiome is anticipated to govern internal cycling between DIC and DOC, and this process, in turn, may affect specific CO₂ pathways”. This sentence is very unclear. Is this the lake aquatic microbiome? What is internal C cycling? And which specific pathways are the authors referring to?

Response: *Thank you for helping improve the writing and organization of the paper. We now specify that “microbiome” refers to the lake aquatic microbial community, define “internal carbon cycling” as in-lake transformation between DIC and DOC, and clarify that the “CO₂ pathways” refer to CO₂ production and consumption associated with microbial carbon fixation and mineralization. The sentence was revised as follows:*

“Theoretically, aquatic microbial community in karst lakes is expected to regulate DIC-DOC transformation, whereby microbially mediated C fixation and mineralization shift the balance between CO₂ production and consumption”.

References

- Leng, P., Rode, M. and Koschorreck, M. 2025. Summer drought enhances diurnal amplitude of CO₂ in two German rivers of different size. *Water Research* 271, 122870.
- Zhao, L., Zhao, X., Fan, X., Zhang, J., Gan, G., Tan, Z., Yao, J., Wan, R., Xu, L. and Yang, G. 2025. Moderate Hydrological Droughts Maximized CO₂ Sink in China's Largest Floodplain Lake. *Environ Sci Technol* 59(33), 17606-17616.