Le Bourget-du-Lac, January 17th 2023

Dear Hydrology and Earth System Sciences Editors,

We would like to thank the *Hydrology and Earth System Sciences* Editors for giving us one more opportunity to revise the manuscript, and the editor for its constructive comments on our manuscript « *Past and future climate change effects on thermal regime and oxygen solubility of four peri-alpines lakes* » by O. Desgué-Itier, L. Soares, O. Anneville, D. Bouffard, V. Chanudet, P-A. Danis, I. Domaizon, J. Guillard, T. Mazure, N. Sharaf, F. Soulignac, V. Tran-Khac, B. Vinçon-Leite, J-P. Jenny. Please find enclosed a revised manuscript that takes into account the concerns raised by the editor.

We carefully considered new comments, and accordingly, we added statements to clarify why only MyLake model was used instead of the other models. As recommended, the abstract has been revised to better highlight research questions and the value of this study. Besides, we did a careful proofreading to correct grammatical and typing errors.

Please find below in blue text our responses to the comments.

Sincerely yours

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Response to the editor' s general comments:

* More generally, the first half of the abstract is relying on a few introduction type sentences, rather than specifying a clear aim. I would ask that you try to make the opening more concise and make clear the research questions (rather than just saying 1D models have shortfalls). The interesting question of this study is buried in a later methodological statement: "the effects of climate change on the thermal regime and oxygen solubility were analyzed …" . But why are they analysed? What are the questions the study answers?
We thank you for pointing this out. We have modified the abstract accordingly, more specifically the first part, to clarify the research questions and to insist on the interests of that study.

* Regarding the model inter-comparison – this is given some weight in the abstract ("MyLake is the best …"), but in the main paper this comparison is in supplementary material. There has been some discussion of this in the review and I'm satisfied, except I do have the position that it is not 1 model that is the "best" but the implementation of one model that was the best … in this case. I say this as the parameter vector optimised for each model seems limited and for example there are parameters and model options that were excluded (e.g., deep mixing in GLM) and so it's hard from this assessment to tell if one model is truly better than other. As developer of GLM I am somewhat biased so I will leave it to the authors to decide how to navigate this, but my suggestion is to simply qualify the language slightly by saying that "of the models implemented for this analysis, MyLake performed…. ".
We have taken into account your interesting remark and modified the text accordingly. Indeed, we explained more precisely that the models' performances were dependent on the implementation and the parameters tested for each model.

* Last line: "These results suggest important degradation in lake thermal and oxygen conditions and a loss of habitats for endemic species." Strictly, the paper does not prove their will be a loss of habitat for a specific species and this is inferred by a change in temp, and in solubility from ~10.75 down to 9 mg/L. The discussion on line 575 talks generally about sensitivity to temperature and this makes the case that some species have physiological cues that may be impacted upon. The paper however does not answer how important the oxygen concentration change is from a habitat point of view so the wording of the abstract is therefore potentially misleading in this regard. I'm not sure from reading the paper (section 4.2) if there are species that are so sensitive to a change in oxygen concentration? Noting that this will still be 100% saturation, which I understand from fish ecophysiologists is more important than the absolute oxygen concentration. Usually species come under pressure at lower oxygen saturation values like <50%. Therefore, whilst I agree you need a significance statement in the abstract along these lines, more precise use of language should be used for this, and I would also suggest that further discussion (with references) about implications of change in oxygen saturation on fish habitat is warranted. For example, see also Magee et al 2018 (CJFAS Vol 75 "Modeling oxythermal stress for cool-water fishes in lakes using a cumulative dosage approach"
As you suggested, we modified the last line of the abstract to be more precise about the potential impact of changes in temperature and oxygen conditions. We no longer refer to a loss of habitats but we are referring to changes in habitat conditions (L36-38). We also added some references in the discussion (L585-586 & L594-596).

Response to specific comments:

* Line 20 lake modelS (plural)
* Line 21 such AN approach (missing word)
* Line 23: Please reword to be more concise – I think there is some discussion about downscaling and not sure what is meant by "serious" - " ... which have several limitations that are barely discussed, such as the need of serious downscaling "
* Line 24 – extenD, not extenT

Following suggestions, the term "serious" has been replaced by "significant" (L25); the identified spelling mistakes were corrected.

Comments made in the PDF have been addressed and are highlighted in blue in the manuscript. The line numbering refers to the new version of the manuscript (c.f. Draft_HESS_version_RC1_RC2_last_version_blue.docx).

In the introduction:
* Line 53 (and line 70): "the study of lakes' thermal regime over decadal to centennial timescales is still limited" . There are modelling papers that validate over long-time periods, (eg: Magee & Wu, HESS, 21, 6253–6274, 2017; Magee and Wu, Hydrol Proc 31 308-323, 2017; Ayala et al 2020 already cited in discussion but relevant here), so I think these statements are overly general and could be more precise.

We thank you for that very good point. We added some details about the limits of existing studies, considering the literature you advised us to refer to (L54).

* Line 65 : papers like Sadeghian et al (Hydrol Sci J, Vol 67, 2022) have compared lake model sensitivity to different weather forcing products

Tkank you for that reference which has been added to the introduction (L66-67).

* In the discussion, I was wondering if Section 4.2 sits better after section 4.3? Maybe I misunderstand the flow of logic but, but its seems 4.1 and 4.3 are about past and future temp (respectively), and 4.2 is about oxygen, so I would suggest the segue to oxygen after discussion of temp may suit better? Not a requirement of the revision but just for the authors to consider, also in light with my comment about oxythermal habitat discussion made above.

We thank you for that very good point. The idea was to first discuss the evolution of the two studied variables and then to interpret and compare the sensitivity of each lake regarding these variables. But as you noticed, it appeared more logical to move 4.3 before 4.2 as it focuses on temperature, as then to discuss about the oxygen conditions.

Overall, I still note quite a few grammatical and small editorial issues (eg missing space here and there, extra letter etc), so request you do a thorough proofread to check for these issues.

We did a complete review of the article to correct the grammatical and typing errors.