Dear Authors,

First of all, I appreciate your great important work that significantly contribute to the lake investigation field. Studying the long-term climate-induced changes in Lake Balaton's thermal structure is definitely giving the future researchers and policy makers a good applicable/scalable approach that can be used for further development. The comments below are just for improving the current work and making it interesting for other readers as well.

My opinion is: Accepted for publication after addressing the comments attached.

We thank you for your valuable comments and constructive review. We address each of the comments below.

Abstract:

Although the general information about the workflow has been mentioned, but still, the reader does not know what kind of modeling will be seen in the text. The text is clear and well explained in some aspects but there is a lack of modeling and some quantitative key results if there's any.

We agree that the workflow's modeling aspects and the quantitative findings are not sufficiently represented in the abstract. While the workflow itself represents the main novelty and contribution of the paper and therefore received greater emphasis in the original version, we acknowledge that the description of the modeling approach and the inclusion of key quantitative results are currently insufficient. Therefore, we will revise the abstract to better highlight the applied modeling methods and include the most important quantitative outcomes of the study.

Introduction :

Line 32: Not just in "extreme cases", it might be possible for some shallow lakes, the oxygen depletion happens depending on the stratification strength, the bathymetry of the lake, the lake surface water temperature etc.

(Line 33): In general, when the algal blooms increases then the chance of oxygen depletion increases and may lead to water quality problems.

Line 34-35: The conclusion seems overclaimed: Climate change might generally increase thermal stability not "Even" in polymictic. The word "even" gives the a meaning of separation from the rest of the types of lakes. We should not conclude this evenness now for the climate change effect which is generally affecting all aspects of the water bodies.

The clarifications and comments regarding these statements are well taken, and the manuscript will be revised accordingly.

Line 51: The typos in terms of writing: Either "They are more sensitive" OR "they sensitively reacting to climate change …."

Line 50-60: While the tonality of the text before line 49,50 turned into guidance, policies, upper principals, then in these lines, we see again some introductory information that takes the reader back to the basics of the issue.

Lines 65-75 are also suffering from this problem. These lines along with the previous lines are not coherent and consistent with each other although they have been written very carefully and clearly.

We agree that lines 50-60 interrupt the consistency and logical flow of the introduction, as the text returns to the basic characteristics of polymictic lakes after discussing broader guidance and policies, while the following section (lines 65-75) focuses on the limitations of previous studies. In the revised manuscript, these sections will be restructured to improve the coherence and overall structure of the introduction.

Line 80: Still, we have not seen the other previous literature problems (stating the research gaps and questions that have not been covered through the previous works that can enforce/push us to continue why this is an important research that should be done. Also the model is still unclear to me. Until I go to the methodology and look at the details then I realize you are supposed to use GOTM with a new workflow that has been designed by you. If the novelty is the workflow, we have not seen in the introduction stated out clearly explicitly. If the novelty is the threshold you define or you design, it is not clear at least to me. The novelty is the time period that is being predicted, I don't see the novelty statement selling this point.

We somewhat don't agree with this comment. Lines 63-77 summarize the findings and limitations of previous studies, indicating that polymictic lakes remain comparatively underexplored and that sub-daily processes are generally not considered, despite their importance in polymictic environments. In this context, lines 77-81 introduce the main novelty of the study: the proposed workflow for simulating sub-daily processes in such environments using coarser temporal-resolution data derived from climate model projections. The GOTM model is not an essential component of the proposed framework, as it can be replaced by other hydrodynamic models. For this reason, the workflow is presented in a model-independent manner, with emphasis on the methodology rather than a specific modeling tool. We believe the study's novelty is sufficiently emphasized; nevertheless, the revised manuscript will further clarify and more explicitly present its novel aspects and objectives.

Line 85-end: I don't grasp the key points of these final paragraph. In the meantime that it tries to play the final paragraph role, it still shows like abstract, introductory, results discussion etc. I believe, the final paragraph is like punching the readers to proceed with the text.

The last part of the introduction (lines 85-end) aims to outline the study's focus and to emphasize its relevance and implications for future research. However, we acknowledge that this section may be perceived as too similar to an abstract and does not fully serve its intended function as a concluding transition to the main text. Accordingly, it will be revised to serve as a more effective concluding paragraph.

In general, the introduction parts have been written good but they are not well structured and don't follow each other however the writers might have an opposite opinion. For example, I surely know the importance of studying polymictic lakes but from the introduction I got confused whether this is study that forced to be related to polymictic because of any reason (data availability etc) or there was an urgent issue about this type of lake (that are more sensitive to the climate change and anthropogenic activity). If you see I have seen the words but I could not get this from the whole introduction which takes me to the second part of the paper (materials and methods), where you are talking about the study area.

We believed that the introduction is well-structured; however, you have raised our attention that it is less consistent in certain sections (e.g., lines 50–60). Accordingly, following your comments, the relevant sections will be revised to improve the logical flow and more clearly articulate the importance and motivation of the study, including its focus on polymictic lakes.

Materials methods:

line 91: As I previously mentioned I don't find any good reason of choosing this area as an example except the availability of the data. Is there any specific condition that excludes this lake from the other please state it otherwise, if it is just because its data was available and clean, please change the text accordingly.

The choice of Lake Balaton as the study area was primarily motivated by data availability. Since 2019, high-resolution (both vertically and temporally) in situ hydrometeorological measurements during the warm season have been conducted on the lake, providing a suitable basis for calibrating hydrodynamic models. In addition, the FORESEE database provides long-term meteorological data from 1951 onwards and projections up to 2100, which are suitable for calibrating the weather generator and quantifying projected changes. As the selection basis of the study area was not explicitly stated, it will be clarified in the revised manuscript.

Lines 102: It would have been good if we had either a subheading about the data acquisition and solely talking about it or making it smoother for those who has not been familiar with the technicalities of the employed model. I see the aims of using a part of data for training the weather generator to downscale …., without prior knowledge on the details of the model unless you say it is for knowledgeable readers.

We agree that the use and purpose of the two datasets may appear unclear at first glance. Accordingly, we aimed to clarify their respective roles already in the section introducing the applied meteorological data, while lines 119-125 provide further elaboration on their functions; however, we acknowledge that a clearer separation between the supporting data and their subsequent methodological application would improve readability. Consequently, in the revised manuscript, the data description will be restricted solely to the employed databases, while methodological aspects will be moved to the relevant sections of the modeling framework.

Results and discussions:

General opinion about the results discussion: Following the section is a bit hard and I suggest try to smooth it. Also, there is a lack of inconsistencies between the introduction aims and scope, and what the results are presenting which again goes back to the issues with the introduction.

With the introduction being revised, we will ensure that the aims and scope of the study are more consistently aligned with the presented results and discussion, while also improving the overall flow and coherence of this section.

Line 223: Typos, repeating “Materials and methods” instead of “Results and discussion”

Technical comments: The authors specifically designed a workflow to address the lack of sub-daily data in climate models. You validated the 6-hour downscaling against hourly ERA5 data for the 2000–2020 period. The comparison showed that while statistical consistency (mean, median, and standard deviation) was preserved, there were minor discrepancies in extreme values: water temperature was slightly overestimated (max difference of 0.3 ^{o}c), and stratification intensity () was slightly underestimated (max difference of 0.9 J m^{-3})

Another comment is related to the use of linear Bias correction. Although Future projections assume that changes scale linearly from the baseline, it is allowed to do that, but at the same time it introduces a potential source of error if climate responses become non-linear toward the end of the century.

While the assumption of linear bias correction may indeed introduce a potential source of error, the applied correction was based on the available climate projection data. Furthermore, the reliability of future projections remains inherently constrained by the extent to which future climate conditions may differ from those represented in the historical reference period. More complex approaches, such as ANN-based methods, would likewise depend on relationships derived from historical training datasets when projecting future conditions and therefore would not necessarily yield a more reliable representation of potentially non-linear future climate responses. Nevertheless, the uncertainties associated with the applied linear bias-correction approach will be included in the revised manuscript.

Major comment on the results: One of the key issues of the predictions and projections long-term or short term is the “Confidence Intervals”, and “Uncertainty levels”. It is important to know to what extent the prediction is credible upper bound and lower bound. This is statistically significant to ensure if the predictions are correct first and more importantly safe. The figures that are showing the projections and predictions, should also show the confidence intervals and shaded area of the uncertainty upper/lower bounds.

We do not fully agree with this comment. Climate model projections inherently contain substantial uncertainty; therefore, projected changes under the RCP4.5 and RCP8.5 scenarios were assessed using an ensemble of 14 climate model projections from the FORESEE database, with the resulting ranges (max-min) and distributions presented in the corresponding figures and tables. Consequently, the presented results already reflect a degree of uncertainty associated with future projections. Given the ensemble-based framework of the analysis, uncertainty is quantified by the variability among individual

climate model projections rather than by the application of classical statistical confidence intervals. While calculating confidence intervals is statistically feasible, they would not necessarily provide a more meaningful representation of uncertainty in the context of ensemble-based climate projections. However, where it is applicable, we will provide further statistical indices about confidence or uncertainty levels.