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Reply on RC1

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Author comment on "Past and future climate change effects on the thermal regime and oxygen solubility of four peri-alpine lakes" by Olivia Desgué-Itier et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-260-AC1, 2022

Response to the reviewers' comments

Reviewer #1:

Comments:

Major points:

Question (Q) : The abstract (L25: "[…] several 1D lake model's […]") as well as the introduction and methods part make the impression that the study will be mainly about running an ensemble of lake models on peri-alpine lakes, but in the manuscript only MyLake results are shown and interpreted (all other ensemble model output are in the Supplementary). Here, I think several points are missing: (1) an explicit statement explaining why MyLake was used instead of the other ensemble members, (2) a revision of the text to make it clearer that the study is highlighting MyLake, and (3) a discussion paragraph looking at other ensemble model members. To (1), table S4 is showing that MyLake has the best model fits, and it makes sense to use it. I think this should be stated instead of L115 "[…] as this model is well adapted to Northern and alpine regions" as this is also true for Simstrat which was explicitly developed and applied on Alpine Lake systems. Further, Simstrat also incorporates TKE production by seiche which was mentioned as a shortcoming of the current study in replicating thermocline depth dynamics (L612). Further, the Supplementary is missing information on which model parameters were calibrated for FLake, GLM, GOTM and Simstrat. Also, as the title suggest the focus on oxygen dynamics, why wasn't for example GLM chosen to simulate these dynamics directly instead of MyLake which in the LakeEnsemblR version does not quantify dissolved oxygen dynamics at all?

Answer (A) : We want to thank Reviewer #1 for his/her many suggestions to improve our paper. Following suggestions, we clarified wording: 1) a statement has been completed to clarify why MyLake was selected among the 5 models (L 119-122), 2) we precised that MyLake was the model selected for the trend analysis both in the abstract (L 30) and introduction (L 88), and 3) a paragraph comparing the five models was added to the discussion (L 655-664). A Table has been added to the Supplementary (Table S6) with the model parameters calibrated for each model. Concerning the ability of Simstrat model to incorporate TKE production by seiche and GLM model to simulate oxygen

dynamics, that's a very good point. Over the five different models, MyLake had an overall better performance during our limited calibration exercise. Yet this study does not intend to rank the models. Here, we made the choice to select only the best performant model for the four lakes, based on the lower RMSE's.

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.docx).

Q: Resolution of figures: the resolution of most figures (2, 3, 4, 5, 6, 7, 8, 9, 10, 11) is very low and their axes labels are not readable. Please improve the resolution.

A: All figures have been saved in .pdf for a higher quality. The font size of the axis text has been increased for better readability.

Q: L479: This sentence seems wrong to me. First, wouldn't increasing density gradients especially in deep lakes reduce vertical mixing and prevent hypolimnetic water temperatures from increasing. Further, Lake Geneva's surface area is big (which I suppose you mean here by wind exposure) but the wind energy itself as well as the reach of TKE is supposed to be decreasing in the future (see Castro et al 2021, Nature Comm Earth&Env). Second, how should enhanced deep winter mixing (assuming you refer to convective overturn) lead to plunging of cold water from melting glaciers in deep layers? Is this referring to the Rhone River receiving increased meltoffs? But in that regard, increased mixing would lead to increased deep water temperatures and that would cause a colder Rhone inflow to entrain in to deeper water layers.

A: We appreciate the arguments raised here. Accordingly, we added the explanation suggested and the reference to the literature Castro et al. 2021 (L 505-509). We also deleted the sentence in relation to the cold water coming from melt glaciers plunging into deep layers as inflows temperature was considered as constant in that study.

Q: L502: The statement that the simulated warming is different from current observations due to high wind exposure is not enough in my opinion. Couldn't this be a model shortcoming and bias. Are you sure only the wind mixing pathway is the main difference here? Please focus more on discussing why the future projections are different from recent observations.

A: Thank you for pointing this out. We implemented the discussion on the potential causes of the different responses between future projections and recent observations on Lines 510-526. No temporal bias was exhibited in the error metrics over time. The difference might be explained by less and less frequent complete mixing in deeper lakes, leading to longer inter-mixing periods during which water temperature increases. Larger and deeper water bodies like lakes Geneva and Bourget are expected to have pronounced effects under higher temperatures in the future in face of their capacity to integrate the effects of meteorological forcing over longer periods of time. Their great heat storage capacities are able to carryover more heat from one year to the next and tend to cool more slowly in autumn and winter (Ficker et al., 2017).

Minor points:

Q: L80: It's unclear how oxygen solubility is a biological indicator. I agree that dissolved oxygen itself is one, but solubility (also as represented here) is merely a function of temperature.

A: This is a good point. Indeed, the dissolved oxygen concentration is a biological indicator, dependent on the abundance of phytoplankton communities but also on water temperature. Here, we consider oxygen solubility as a proxy for the species' habitats and their capacity to cope with temperature change, an indirect biological indicator (L 83). Indeed, as the oxygen solubility decreases with increasing water temperatures, a decreasing in dissolved oxygen concentrations could be expected and have an impact on fish communities' survival that need a certain amount of oxygen.

Q: L85: I'd change "dissolved oxygen" to "dissolved oxygen solubility"

A: We agree with this change, which is more accurate.

Q: L97: You're describing all lakes as monomictic but I recall that Lake Geneva did not experience a full turnover for over a decade now. Could you provide references for that statement please?

A: I thank you for that comment. Indeed, Lake Geneva is generally considered as monomictic but over the past few years, the incomplete mixing of the water column brings it closer to a meromictic lake. I added that precision in the statement (L 102).

Q: L138-142: In most parts of the manuscript, you refer to the scenarios as ssp126, ssp370 and ssp585 only, but here you also refer to them as SSP1, SSP3 and SSP5. Please stick to one naming convention.

A: I replaced SSP1 and SSP5 by SSP126 and SSP585 to stick to one naming convention, as you recommended (L 150).

Q: L148: Which sensitivity test was carried out and how?

A: Sensitivity tests were carried out on each climate forcing variables (air temperature, shortwave radiation, wind speed, cloud cover, relative humidity, rain and surface pressure) from MyLake water temperature simulations over a 10-year period, for the 4 perialpine lakes. We added ±20 % to each climate variable daily values and then calculated the performance metrics (RMSE and R^2) from model outputs and observation data. We calculated the difference between model performance with raw data and after applying the 20% coefficient to the input values. Variables with the greatest variation of its performance metrics were identified as variables with the most important effect on MyLake model performance. All results were summarised in Table S1, in the supplementary.

Q: L152: The mentioning of (ii) and (iv) without (i) and (iii) is slightly confusing. Would it be possible to name these scenarios differently, e.g., (i) airT and SW no correction, (ii) all no correction, (iii) airt and SW with correction, (iv) all with correction?

A: I have noted the confusion caused by the mentioning (ii) and (iv) without (i) and (iii). As you suggested, I have renamed the 4 configurations in order of appearance in the text (L 168, 169 & 175). I also applied this change in Table 2 to be consistent with the text.

Q: L155: Wouldn't especially Lake Geneva have more meteorological data since the launch of LeXPLORE?

A: Indeed, meteorological data for Lake Geneva are available on the LeXPLORE data web portal. However, data can be collected only from 2019, which is a

shorter period than the 11-years of data available on MeteoSuisse website. Further, meteorological data were only used to reproduce a seasonal pattern and to apply an altitudinal correction factor. Thereby, we considered that a 10-years period of data was long enough to meet these two objectives.

Q: Table 2: Are all fits calculated for all observational data of temperature?

A: All performance metrics were calculated comparing simulated and all observed data available over a 10- year period, corresponding to the model validation period, specified in Table 5.

Q: L183: I agree with your methodology, and I love how you also compare the fits to long-term time periods, but nonetheless it seems strange that you first mention that L70 "Models are in large extent calibrated against very few years of limnological records so far" but then you are also focusing the calibration/validation on ten years each only. What is the reason for this?

A: We totally agree with you that the originality of the paper lies to the long-term validation against more than 50 years of limnological records. Nevertheless, we chose to show both 10-years and long-term validation periods for the results to be comparable to the literature in which models are usually calibrated against very few years of limnological records. Furthermore, the 10-years validation period verified the model performance for the four study sites, then the long-term validation validated the robustness of the model for a long-term study.

Q: L207: "[…] epilimnion extent and temperature, hypolimnion extent and temperature, metalimnion upper and lower depths, […]"

A: This is a good idea to provide clarification on the thermal indices calculated. We added those terms to the text (L 227-228).

Q: L221: "When normal distribution of residuals […]"

A: Thank you for that suggestion. We added the term "residuals" to be more precise (L 242).

Q: L223: What were the conditions for either choosing Mann-Whitney or Kolmogorov-Smirnov?

A: The wording may not be very clear in the text, but the difference between the two statistical tests was mentioned in parentheses: Mann-Whitney test was used when normal distribution of residuals was not followed and variances were equal while Kolmogorov-Smirnov test was used when the distribution of residuals was not normal and variances were different.

Q: L252: The thermocline depth fits are quite bad (would they be better if you would only quantify them during stratified summer periods?), but what is the reasoning to quantify them here as percentage of lakes total depth? Although these values sound low, I don't see any reasoning for expressing them this way as an error of 20 m in thermocline depth is still significant and it does not matter if the lake is 300 m deep as the thermocline depth will be in the upper 50 m part anyway.

A: We appreciate the suggestion raised here. Indeed, when quantifying the performance metrics (especially RMSE) only during the stratified summer period from June to September, the thermocline depth appeared to fit better. We added

these new values to Table 7 and modified the text accordingly (L 277-280). We also considered your remark about the irrelevance of estimating the errors as percentage of total depth, as the thermocline depth is generally in the upper 50 m. We have therefore modified RMSE values in the text, and we kept the meter as unit (L 273-277).

Q: L253-260: Why are these fits (DOYs) not included in Table 7?

A: At first, the reason why these fits were not included was to reduce the size of the table to make it more readable, the complete table being accessible in the supplementary. However, we considered your remark and completed the Table 7 in the manuscript with these fits mentioned in the text.

Q: L345: I recommend deleting "delay" here

A: As you suggested, we deleted the term "delay" here (L 375).

Q: 3.3 Water volumes: habitat: This paragraph is unclear to me. Where these temperature volumes chosen as average temperature output per year, or are these daily data? Also the comparison of non-overlapping distribution areas is quite confusing between two different decades. Could you add more information please?

A: We apologize for not being clear enough and will add some details into the text to be more precise on the methodology used in here. The water volumes were calculated from daily average water temperature simulated by MyLake model. The bathymetric file allowed us to associate a water volume to each depth. Here, we had a daily average temperature for a certain water volume. Then, we selected only water temperatures exceeding the 3 thresholds (>7°C, >9°C and >12°C) and calculated the sum of these volumes per day. Each day was associated with a certain water volume whose temperature exceeded the 3 characteristic thresholds. In order to compare these water volumes between present (2000-2010) and future (2090-2100), we calculated a daily average over these 10-years periods. At this point, a certain water volume exceeding the 3 thresholds was associated to a day of the year (DOY), for both periods (present and future), according to the 3 scenarios. For the results to be comparable within the 4 lakes, these volumes were reduced as a percentage of the lake total volume. Now, for the 2 different periods, depending on the 3 scenarios, an average lake volume fraction with temperature above 7 °C, 9 °C and 12 °C was calculated for each day of the year. The next step consisted in applying Kernel density to compare both the lake volume fractions with temperature > 7, >9 and >12 °C between the present and future, and also the number of days in the year associated to these lake volume fractions. The non-overlapping distribution showed an increase in both lake volume fractions and number of days associated. We added some methodological details to the text in order to be more specific (L 395-399).

Q: L409: Please keep an eye out for the different digits (. or ,) as here you wrote 12,21 (also the same in the Supplementary tables, please change to dots)

A: We thank you for that remark. As you suggested, we corrected the number 12.21 (L 441) and replaced all commas by dots in tables in the Supplementary.

Q: L442: Please add references to the ecological thresholds of 10 and 11 mg/L

A: We thank you for raising that relevant point. These thresholds for dissolved oxygen concentrations (10 and 11 mg/L) were chosen because they correspond

to the mean values measured in the epilimnion of the four lakes by OLA (Observatory of Lakes). Thus, we found it interesting to study the evolution of water volume exceeding these thresholds, well adapted to the survival and development of organisms living within the surface layers.

Q: L474: The formulation of delta raise per decade of epi-hypo is very confusing. Could you please state this differently (like just write delta T decade-1)?

A: We thank you for raising that confusing formulation. As the second reviewer made the same remark, we changed the wording in the text (L 506-517).

Q: L489: Could you please discuss why hypolimnetic temperatures are increasing although water column stability is also increasing? How do these systems still have enough energy for vertical mixing? The same also for ssp126 results in L504: why has that scenario such different results here for epilimnetic and hypolimnetic temperatures?

A: As you required, we added some explanations to the text about the differences in increase observed in epilimnion and hypolimnion. First, the difference between the upper and deep layers could be linked to a deeper stratification and increase of Schmidt stability. The second argument is relative to the frequency of deep mixing of the water column, which has an effect on the rate of water warming in deep layers (L 517-521).

Q: L519: I'd argue that lateral flow paths and production in littoral zones would also be important for deep and large lake ecosystems.

A: We thank you for that argument which we added to the text (L 548-549).

Q: L547: I'd argue that Lake Geneva was only the most extensive due to its high depth. Could you check Schmidt Stability trends for the first 50 m of each lake and see if Geneva is still an outlier?

A: As you suggested, we checked Schmidt stability trends for the first 50 m of each lake to see if Lake Geneva is still outlier. It appeared a small difference from the results observed over the entire water column, as Lake Geneva was still predicted to experience the highest increase of Schmidt Stability according scenarios ssp370 and ssp585, with +2237 and +2947 J m^{-2} $decade^{-1}$, respectively. A smaller difference was observed but still remained. Yet, same trends were observed, as you can see on the Figure S5 in the supplementary. We also added a table (Table S7 in the supplementary) showing every result for both Schmidt stability calculated on the entire water column or only on the first 50 m.

Q: L591: Please add information to which approach you are referring here, I assume it's the focus on air temperature and short-wave radiation, right?

A: Indeed, this is the approach developed in that study we were referring to in that statement. As you suggested, we specified more details in the text to be clearer (L 600-601).

Q: L645: The sentence about "Differences in the oxygen solubility response to climate change are also observed" seems very vague to me.

A: We agree that this sentence was not very clear. Thus, we chose to remove it from the text, as it did not provide any further clarifications and was redundant to the previous paragraph.