



EGUsphere, author comment AC2
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Reply on RC2

Olivia Desgué-Itier et al.

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., EGU Sphere, <https://doi.org/10.5194/egusphere-2022-260-AC2>, 2022

Reviewer #2:

This paper presents result of model projections of the temperature and mixing under past and future climates (scenarios ssp125, 370 and 585) for four peri-alpine lakes, using the IPSL-CM6-LR atmospheric model from ISIMIP3b with the 1D lake model MyLake. It also considers future changes in thermal habitat and oxygen solubility of these lakes, making inferences about their suitability as a habitat for some fish species. Good projections, eg as used with the new ISIMIP3b data, of the potential effects of warming on these important lakes will be very valuable and an important and novel research result.

Question (Q): My main issue with the paper concerns the style. I found it quite confusing and the rationale behind the study in the introduction as well as the methods should be more clearly stated. For instance, I did not properly understand why the authors chose to use only shortwave radiation and air temperature projections for model hindcasts and future projections, and use historical means for the other model forcing variables. Also it was not so easy to quickly understand how this was done. I think you show show the comparison between the model configurations with just shortwave and airtemp, and with all forcings from the GCM more clearly and make a short justification for your chosen configuration. I also didn't understand why the authors began with an ensemble of climate and lake models, but selected only one of each for the analysis, or what the value is of performing a long hindcast. This all might sound provocative, and I am not saying this is unjustified at all, only that it was not explained sufficiently to the reader. The scientific results seem sound, but I struggled to interpret the plots (which have a resolution and font size that are too low for me to read the axes), and I think the discussion needs to be reworked, focusing on the interpretation rather than the description of the results. There are some interpretations to explain the differences between the lakes, but I think they need to go deeper, like if simply wind fetch is really causing deeper mixing in Geneva, or the effect of earlier stratification onset timing for deep water warming trends. Several formulations in the manuscript describe complex things, but need to be sharpened to make this complexity more understandable. In the sections below I have tried to point out clearly the parts that I am referring to.

Answer (A): We are grateful to Reviewer #2 for your careful revision of our manuscript, making constructive and valuable suggestions. The discussion was revised and new content was implemented to provide further interpretations on the results.

Abstract

Q: General: in this modelling context, try to use "parameters" only to refer to model parameters, and use "variables" for things like wind (eg line 62) or e.g. "substances" when talking about concentrations (eg line 20).

A: We thank you for that precision. As you suggested, we replaced the term "parameters" by "variables" to refer to forcing variables used as inputs (L 23 and L 29), and "substances" when talking about concentrations (L 21).

Introduction

You go into some detail to describe the approach, but not quite enough to make it clear, mainly in lines 75-86. Later in the methods you go into a little more detail, but I found it unclear when starting with the introduction whether you use observed radiation and airtemp to force the model, and then estimated the other variables? It would be helpful to describe the rationale behind this. The introduction should be focused more to justify and define the main research question. For instance, there is no mention of the thermal habitat and I don't recall that you reported or discussed any results based on the 4 different model configurations.

Q: Line 129-130: did you use the historical scenario for each model (I assume)? Please state explicitly. The historical scenario from the IPSL model is available back to 1850 for all required forcing variables for the lake model. As mentioned above, please explain why you use only shortwave and airtemp.

A: We thank you for that remark. As you suggested, we stated explicitly in the text the scenarios from which data were collected (L 142-144). Both historical and ssp370 (intermediate) scenarios were used to cover the whole period of observation data (1987-2019). Indeed, all required forcing variables were available back to 1850 from IPSL model. We chose to use only shortwave and air temperature to keep only variables with the highest level of confidence over the long term, as the long-term assessment of global warming on the peri-alpine lakes thermal regime was both the particularity and objective of that study. Thus, we added a sentence to the text to explain more precisely the reason why we used only those two variables to force the model (137-139).

Q: Line 132-133: sentence not clear (350 data)

A: We rephrased that sentence to be more precise. (L 144-145)

Q: Line 148-150: It is unclear what is meant by the sensitivity test – could you please briefly elucidate, even if the description is in the supplement? This hypothesis needs to be more clearly stated in the introduction

A: Same question was raised by the first reviewer. You will find below our answer with details on the sensitivity test carried out on the forcing climatic variables:

"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 $\pm 20\%$ 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.”

As you suggested, we stated it more clearly in the introduction (L 160-166).

Q: Line 151-160: it would be simpler if you described the configurations in order i), ii), iii), iv), rather than beginning with ii and iv.

A: The same suggestion was made by the first reviewer. We have modified the order of the 4 configurations accordingly (L 168, 169, 175 and Table 2).

Q: Table 2: I don't completely understand the table from the legend. Do the performance metrics represent the lake model temperature errors (units degrees Celsius)? The configurations are not really understandable in this context – when you say only air temperature and shortwave, do you mean that these variables come from the climate model IPSL-CM6-LR, while the other input variables are observed data?

A: Indeed, the performance metrics have been calculated between simulated and observed water temperature, in Celsius degrees. We added the unit in Table 2 in order to be clearer. We also added explanations of the 4 tested configurations in the Table 2 legend, in order to be more understandable. The different configurations were also detailed in the text (L 184-187). Indeed, in the configurations (i) and (iii), only air temperature and shortwave radiation were extracted from ISIMIP3B – IPSL, whereas other variables were calculated from meteorological observations. For these variables, daily means were calculated and then replicated every year to apply a seasonal pattern.

Q: Line 181-184: I didn't understand this bit – you should add more specific details (eg what is the instrumental period and why do you need to identify similar climate conditions, what do you mean by the RMSE between winter air temperatures, etc?

A: We have considered your remark raised here. Accordingly, we added some details into the text to clarify the method used to estimate the water temperature initial profile (L 210-215). The objective was to identify the year with available water temperature observation data (from OLA database), with the closest climatic conditions to the year 1850, for which no field data were available. Air temperature from January 1st to January 31st was compared between data extracted from ISIMIP3b – IPSL and meteorological data over the period corresponding to OLA database. We calculated RMSE between these years, and identified the year of meteorological data with the closest air temperature to January in 1850. Water temperature profile was then collected for that specific year and used as initial profile in January 31st 1850.

Q: Line 201-202: Be more precise about the formulation and units – Ts at 5 m (not Ts=5m), and add units for Tb (Tb at 60 m, 60m, 299 m, and 140 m depth for ...)

A: We thank you for having noted this inaccuracy and lack of units. We have modified the text accordingly (L 221-222).

Q: How did you define stratification duration – is it the total number of stratified days per year (where each period > 5 days), or the duration of the longest uninterrupted period?

A: The stratification duration was defined as the longest uninterrupted period of stratification. Indeed, it was calculated from the number of days between onset and end-of-stratification, themselves defined as the day when Ts-Tb >2°C during

more than 5 consecutive days. We added details about this method in the text to address the inaccuracy (L 230-231).

Results

Q: Note: the resolution of the figures is very low and the labels are very small, so that I am unable to read the axis labels on any of them except Fig 1. Please bear this in mind if there are potential interpretation differences.

A: We thank you for raising the issue of the resolution of the figures. All axis labels have been enlarged and the quality of the figures improved.

Q: Line277-281: rather report change/decade only for trends over time. If you make a comparison of period means (eg mean 1990-2020 with mean 2070-2100), then report the change as absolute, eg an increase of 1.5 degrees Celsius from the baseline period to the end of the century.

A: As you suggested, we added the rates of increase between the two periods as absolute to better represent the expected air temperature and shortwave radiations variations (L 305-310).

Q: Figure 4: it looks like you applied some smoothing to the time series – please describe what you did. Also, I suggest to label the panels only with letters, not combined letters and numbers

A: On the Figure 4, a locally weighted smoothing was applied to air temperature and downwelling shortwave radiation time series over the period 1850-2100, in order to better identify the trends. We added that precision in the figure title to specify the method used (L 317). As for the labelling, we agree that using only letters would add homogeneity within the panels and would be maybe more understandable. However, we feel like using both letters and numbers to refer to the two variables on one hand (air temperature and shortwave radiation) and to the three scenarios on the other hand, is easier to describe in the legend. Still, we would agree to make some modifications if you think this is necessary.

Q: Line 297: I am curious about the negative temperature trends you report here, and also interpret in the discussion. I cannot see any trend in Fig 5 – the epilimnion temperature always looks to be increasing in ssp126.

A: We thank you for raising this question. Cooling surface trends in the future were previously predicted in large deep lakes in Central Europe (Schmid and Köster, 2016). The mechanistic effect of surface cooling in the most optimistic scenario over 1970–2100 is explained by the future development of both air temperature and solar radiation predicted to decrease. Higher solar radiation trends observed over entire Central Europe in the last decades largely caused by decreasing aerosol contents in the atmosphere as a consequence of air quality measures are not expected to continue to a similar extent in the future, thereby the recent past trend of surface warming is not expected to persist in the future. This discussion was implemented on L 510-515. In Figure 5, the negative trends at -0.07 to -0.08 °C decade⁻¹ are indeed difficult to visualize in the averaged curves; they can be better visualized by the annual dots.

Q: Line 316-318 (and corresponding methods section): the non-overlap in the temperature density distributions is not sufficiently described to understand what it represents. Which temperatures are being compared exactly? They are obviously averaged, but how (eg across the water column, but then they may need to be volume

weighted)?

A: Same question was raised by the first reviewer. You will find below the answer, with precisions on the non-overlap in the temperature density distributions:

“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: Line339-351: I think it would make more sense to analyse the trend over the whole period, or if you compare the baseline 1990-2020 with the future 2070-2100, then it would be more informative to report the absolute change rather than the trends today and in the future. This would put into perspective the fact that ssp126 initially warms but then cools slightly towards 2100 reflecting emissions reductions. So although there is a cooling trend at the end of the century in this scenario, this should not be confused with the overall long-term warming trend compared to today. Also, in Table S4, MyLake has an R² of practically zero for stratification phenology in many instances – if you trust these results, you should mention the reliability in the discussion. Also, I wonder if these results would be better in a table, which makes comparison and overview of the changes much easier.

A: We thank you for this very good remark that we have taken into consideration by integrating absolute changes between the two periods (374-378). About the very low R² for stratification phenology, this can be explained by the frequency of the lakes monitoring (twice a month), which does not allow us to capture the exact dates of onset and end-of-stratification. We would require the complete time series data to be able to compare to the dates predicted by the model. As other thermal indices, such as Schmidt stability, were predicted with good precision by the model, we chose to trust variables related to stratification phenology. A paragraph was added to the discussion to clarify this choice (L 625-629).

Q: Figure 7: I suggest to reverse the colours – blue for the coolest scenario and red for the warmest.

A: We thank you for that very good point. As you suggested, we have inverted the colours, with blue for the coolest scenario and red for the warmest. We made

this change for both Figure 7 and Figure 9.

Q: Line 368: do you mean that temperature exceeded 7 degrees in all scenarios in the future period? Are these daily temperatures or a mean (Methods say averaged daily data, but this is not quite clear)? The background to my question is that it is quite different if the lake never cools below 7 degrees, even in winter, or if there are only a few days in summer when this threshold is exceeded.

A: Indeed, it appeared that in all 3 scenarios in the future period (ssp126, ssp370 and ssp585 from 2090 to 2100), the entire water column will reach 7 °C. First, water volumes and lake volume fractions with temperature above 7 °C were calculated at daily time step. Then, the annual mean of these water volumes were calculated. The results showed that annual average of lake volume fractions reached 100 % of the lake total volume, during the whole year, including winter period. We added some explanations in the text to be clearer (L 395-399). We also added new figures in the supplementary to illustrate the evolution of the lake volume fractions with temperature above 7 °C, for Lake Bourget (Figure S6) and Geneva (Figure S7) according to ssp126 scenario. We chose the most optimist scenario to show that even in that case, the whole entire water column will exceed 7°C throughout the year, at the horizon 2100.

Q: Line 371: Better say 100% non-overlap rather than 100% increase.

A: As you suggested, we changed the wording "increase" to "non-overlap" to be more specific (L 403).

Q: Lines 440 ff: Section on oxygen solubility density overlap: I feel that it does not make sense to report the non-overlap of the density distribution of oxygen solubility. This is hard for me to meaningfully interpret, and I suggest to remove this part.

A: We understand the difficulty to interpret this part. As a result, we added further details about the Kernel density method tried to explain more precisely the way of interpretation (L 475-478). If that part remained meaningless to you, we could remove it from the text.

Discussion

Q: The discussion section contains a large repetition of the results, and there should be more integration of the findings into the context of the literature, with the aim of detecting some of the important mechanisms driving the thermal changes. You may like to consult Kraemer et al (2021) and literature therein to interpret lake thermal habitat (Nature Climate Change 11, 521-529). Plus there is a lot of literature on the thermal properties of these peri-alpine lakes eg Geneva that may help identify the mechanisms why these lakes behave differently. Altogether, I think the discussion and conclusion should be heavily revised and especially remove the results descriptions.

A: The discussion section was revised accordingly on L 506-664. We are grateful for the literature indication which brought contributions to our discussion. The new citation was included on the Reference list.

Q: Line 469: check year

A: We thank you for noticing this typo. We modified that error in the text (L 510).

Q: Line 473-475: you can remove this repetition of results and just concentrate on the

interpretation. If you want to mention this, it would be simpler to say that surface water is warming faster than deep water at 0.07 degrees/decade in Lake Bourget and at 0.15-0.16 degrees/decade in the other lakes.

A: As you suggested, we changed the wording of that sentence to make it easier to understand (L 499-526).

Q: Line 481-485: as I mentioned above, you should be careful with reporting ssp126 in this way. Although ssp126 shows cooling air temperatures at the end of the century (negative trends due to emissions reductions), it still represents a warmer future, so make sure your formulations are not misleading. I find the formulation as it is now sounds like the surface is cooling compared to today. Also, I could not see the negative trends you are talking about in Fig 5, perhaps you could comment on this?

A: We agree that this sentence needs clarification. This discussion was accordingly revised on L 510-515 to provide the reasoning behind the cooling trends in the future. In Figure 5, the negative trends at -0.07 to -0.08 °C decade⁻¹ are indeed difficult to visualize in the averaged curves; they can be better visualized by the annual dots.

Q: Lines 489-497: this is all just a repetition of the results and is unnecessary.

A: Agreed. The discussion section was revised accordingly on L 519-521 to avoid repetition of the results and to provide more integration of the findings into the context of the literature.

Q: Line 511 ff: You shouldn't call this section Oxygen Trends because you didn't model oxygen, just temperature, on which the oxygen solubility is only calculated. This section is essentially a repetition of the results. You should focus on interpreting the results, explaining their significance, drivers, and context.

A: Agreed. The Oxygen trends section was revised accordingly on L 530-554 to avoid repetition of the results and to focus on interpreting the results.

Q: Lines 542-551: You need to account for the differences in lake morphology when comparing Schmidt stability. Lake Geneva will need much more energy to mix it than Lake Annecy. Perhaps comparing relative changes would be more meaningful than absolute changes.

A: We totally agree with your interpretation and the need to account for the differences in lake morphology when comparing Schmidt Stability. Accordingly, we added some explanations and comparisons in the text (L 558-562).