**Hydrology and
Earth System
Sciences**

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Discussions

Interactive comment on "A simple lumped model to convert air temperature into surface water temperature in lakes" *by* S. Piccolroaz et al.

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Received and published: 28 June 2013

We would like to thank the Referee # 2 for the review which we believe helped us to improve the manuscript. We found some of the suggestions very useful (mainly with respect to the outline of the manuscript, the clarification of the role played by wind and external forcing other than air temperature and all the technical aspects evidenced), whereas we do not completely agree with some other comments. In the following pages, we respond to each of the Reviewer's comments, trying to elucidate the critical points raised by the reviewer.

Please notice that pages and lines in this document refer to the original manuscript present in open discussion.

In order to facilitate the review of our response, we include as a supplement the revised version of the manuscript, in which all modifications are highlighted.

1 General comments

1. Overall, the manuscript suffers from extrapolating results beyond reasonable predictions and the authors inadequately develop a "story" that appeals to general readers of HEES. Although all models simplify a system or process they are trying to represent, my main concerns here rely on the oversimplification of the key processes responsible for the heat budget in lakes.

   Although our model is based on simplifications and assumptions (ultimately, our purpose is actually to develop a simple model), we think the term "oversimplification" is not suited here. These simplifications/assumptions are not introduced in order to neglect or ignore important physical processes (which is not the case since all the significant processes are included), but are indeed necessary in the attempt to limit the amount of input information required to estimate surface water temperature of inland water basins. As a matter of fact, long term, high resolution records of meteorological data (e.g. air temperature, wind speed and direction, solar radiation, humidity, cloud cover etc.), that are required to implement rigorous process-based models, are usually not available. In order to face this data scarceness, we developed the simple physically-based model Air2Water, which is able to estimate surface water temperature of lakes given air temperature data only. Air2Water has the primary objective of being an alternative tool to more complex process-based models, without claiming to be substitutive. Our model could be a useful tool in all those cases where available data are few, which is the case for many medium and small size freshwater basins located in unmonitored or very remote zones. This reasoning has been added at the end of the Conclusions section.

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2. Specifically, the "reasonable assumption" of air temperature being the main variable influencing the heat balance of the lake is not "reasonable" (e.g., Schneider and Hook (2010) - Geophysical Research Letters). Rather, in appendix A the authors show most of the model components (i.e., Hs, Ha,He, Hc, Hp) are strongly related to solar radiation, cloudiness, and wind.

We agree with the Reviewer's comment that wind, cloudiness, humidity and solar radiation play a role in regulating the dynamics of surface water temperature. However, we would like to stress the fact that air temperature can be used as a proxy for estimating the general evolution of surface water temperature, as it is a significant index of the overall meteorological condition (e.g. Livingstone and Padisák, 2007). Furthermore, air temperature data is the most easily available meteorological variable, especially over a long time scale. We also note that seasonal patterns of meteorological variables other than air temperature (e.g. solar radiation, wind, cloudiness, humidity) are implicitly accounted for in the formulation of the model, by means of the periodic terms (see the comments about coefficients c_1 and c_2 at page 2724 lines 7-13).

In the light of Reviewer's comment, we reformulated the paragraph at page 2703 between lines 10 and 15 emphasizing the role played by meteorological variables others than air temperature:

"The only meteorological variable explicitly included in the model is T_a, while the remaining meteorological forcing (e.g. wind speed, solar radiation, humidity, cloudiness which besides air temperature are the major factors controlling the heat budget of lakes) are inherently accounted for in the model's parameters. In particular, the formulation of the model implicitly accounts for the seasonal patterns of these external forcing terms through the data-driven calibration of the parameters, while higher frequency fluctuations are not considered, consistently with the main aim of the model that is to reproduce the evolution of T_w at long time scales (i.e. monthly, annual, interannual)"

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Concerning the paper by Schneider and Hook (2010), they refer to the work by Austin and Coleman (2007) to point out that in some regions of the world (e.g. around the Great Lakes, namely our case study) "water bodies appear to warm more rapidly than the surrounding air temperature". Schneider and Hook (2010) suggest that "changes in insolation, ice cover, and other factors are important contributing factors in explaining" why water surface and air temperature trends do not always agree. In general, we are in full agreement with these conclusions. However, we believe that our model (which uses only air temperature as main meteorological variable and implicitly includes the periodic patterns of the other variables) is able to suitably reproduce, over long time scales, the overall process with a fairly good accuracy and reliability. In order to support this statement, we included Figure 1 in this document, which shows the evolution of measured air temperature, and measured and simulated summer (July-September) water temperature from 1985 to 2012. Data refer to buoy 45004 - Marquette, which is one of the buoys that also Austin and Coleman (2007) used in their work. Note that simulation results are able to well capture the trend of summer water temperature and reasonably provide an estimate of the rate of warming within the considered period: $+0.15\ C°yr^{-1}$ and $+0.12\ C°yr^{-1}$ for measurements and simulations, respectively, with water temperatures warming up more rapidly than air temperature in both cases ($+0.09\ C°yr^{-1}$). Even if there is a small discrepancy between measured and simulated trends of water temperature, we can assert that the general behavior is qualitatively (but to some extent also quantitatively) well reproduced. Furthermore, interannual fluctuations are also well reproduced (see point 17 in section "Specific Comments" for a more detailed discussion). In the light of these results we claim that the model presented here is able to reproduce the main processes governing lakes' surface water temperature by using air temperature as the only input meteorological forcing.

3. But, then the authors choose to simplify the heat budget by emphasizing air tem-

perature which may be misleading especially when predictions of climate change
may have the greatest impacts to the overlooked fundamental drivers.

In order to properly predict future changes in surface water temperature of lakes
one should adopt robust predictive models fed by input meteorological forcing
obtained by GCMs or RCMs projections. However, the use of detailed process-
based models is not straightforward. Significant difficulties can in fact arise mainly
due to the necessity of downscaling climate projections from the coarse resolu-
tion of the climate models to a more suitable point scale. Furthermore, in order
to apply the downscaling procedure a significatively large amount of historical
data are required, which is not always the case. Dettinger (2013), for example,
found that for the case of Lake Tahoe" at daily scales, some of the downscaled
variables (especially, temperatures and longwave fluxes) reproduced historical
variations very faithfully, whereas downscaling of other variables (especially, pre-
cipitation and winds) did not follow day-to-day observed fluctuations ...". In our
view these evidences show that the evaluation of climate changes impacts on
lakes' water temperatures is not simple and immediate in the case of detailed
process-based models which require as input a large amount of data often diffi-
cult to obtain through a robust and reliable downscaling procedure. On the other
hand our modeling approach requires as input a variable whose downscaling pro-
cedure is very roust, i.e. air temperature, and thus can be seen as an alternative,
simpler tool to be adopted in the presence of data scarceness.

4. Hence, the current version the model is a bit oversold for what it is and the authors
still need to address the limitations of the model.

In the revised manuscript we stressed the limitations of the model and an addi-
tional sentence has been added in the conclusions:

*"In principle, the simple model presented here is likely to be effectively applied
to lakes with different characteristics, although some inconsistencies could arise
in those cases where the assumptions on which the model formulation has been*

based (see Appendix A) are no longer valid (e.g. tropical lakes characterized by intense evaporation, basins in which the through-flow is consistent, lakes located in regions where the variability of meteorological forcing is significant at sub-annual frequency).

5. For example, the model may be sensitive to spatial variability (not appropriately addressed here).

We agree with the Referee that the spatial variability has not been explicitly addressed in the present manuscript. However, we want to note that this was not our original goal since we were more interested in creating a simple tool able to predict lakes' surface water temperatures in the case of data scarceness. Furthermore, the simulations that we performed showed that the model can be successfully applied to different points of the lake (e.g. using measurements from different buoys, as discussed in the original manuscript at page 2718 lines 7-20), thus allowing to reproduce (if the dimensions of the lake are relevant and water temperature data are available) spatial variability of surface water temperature in an aggregate framework, i.e. by assigning a competence area to the point where Air2water model is applied.

6. In addition, there is a problem with the mixing of sections (results and discussion) and in some parts the authors should avoid colloquial language. There is some redundancy in the discussion (pages 20-21) as well.

We agree with the Referee's comment. The whole part describing the use of GLERL data (from page 2715 line 25 to page 2716 line 28) has been moved at the end of Section 4 ("Results"), introducing a new sub-section 4.5 titled "Satellite data". Text has been revised as well in order to avoid colloquial language and redundancies.

7. A more appropriate review of literature starting with Chapters 5 and 6 from the book by Wetzel "Limnology: Lake and River Ecosystems" Elsevier Press may be

useful. I encourage the authors raise these concerns and I hope that they will find this review useful.

We thank the Referee for the suggestion. We found in the book by Wetzel very useful hints. Concerning the review of the literature we accepted the comment and modified the introduction section accordingly, see points 3 and 8 in section "Specific comments".

2   Specific comments

1. Page 2 Line 12-13: Atmospheric temperature is not the main factor driving the system. It is well known that solar radiation and wind are more important than air temperature (see the model description in Appendix A and relevant literature).

   Following the Reviewer's suggestion we clarified the role played by the other variables. For a detailed answer see our reply in point 2 of section "General comments".

2. Line 15-16: The authors oversold the model here. In my opinion this model is not recommendable for predictions in the future. There are key factors driving the heat budget that are missing due to an oversimplified approach (e.g., Schneider and Hook, 2010).

   In this we do not agree with the Referee as we already specified in points 2 and 3 of section "General comments".

3. Page 3 Line 2-4: Please provide proper citations at the end of this sentence.

   The sentence has been modified as follows in order to include an additional reference:

"As a matter of fact, water temperature can affect both the chemical (e.g. dissolved oxygen concentration) and biological (e.g. fish growth) processes occurring in the water body (e.g. Wetzel, 2001)."

4. Line 13-14: Please provide proper citations at the end of this sentence. This sentence is also a bit confusing.

We reformulated the sentence and included an additional reference. The text has been modified as follows:
"Water temperature in lakes follows complex dynamics and is the result of a combination of different fluxes, whose sum is often small compared to the single terms (e.g. Imboden and Wüest, 1995). Therefore, relatively small errors in the estimate of the single contributions may result in a significantly large error in the evaluation of the net heat flux. This is particularly true for the well-mixed surface layer, usually termed as epilimnion during stratified conditions, which experiences strong oscillations at a variety of temporal scales: from short (hourly and daily) to long (annual and interannual) up to climatic (decades to centuries)."

5. Line 20: Provide examples of these difficulties.

We accepted Reviewer's comment and modified the text accordingly.
"Closing the heat balance correctly at the different scales and predicting the future trend of surface water temperature is therefore challenging, and not always possible (e.g. if meteorological data are not sufficient). As a consequence, some hydrodynamic lake models prescribe surface water temperature as surface boundary condition instead of computing the net heat flux at the water-atmosphere interface (e.g. Goudsmit et al., 2002; Piccolroaz and Toffolon, 2013). In general, large uncertainties are associated to the estimates of the various heat exchange components; however the variables involved in the different processes are either not all independent from each other or do not present strong interannual variations, suggesting that some simplifications can be possibly adopted."

6. Line 28: But this also depends on cloudiness (e.g., Wake (2012)- Nature Climate Change 2, 230 doi:10.1038/nclimate1480).

We included the dependency of shortwave solar radiation from cloudiness in the revised manuscript as follows:

"For instance, shortwave solar radiation substantially depends on the latitude of the lake and on cloudiness, with the former presenting a rather regular annual trend and the latter being important mainly at short time scales (from hourly to weekly)."

7. Page 4 Line 6-15: This paragraph includes some apparent contradictions with the idea that air temperature is a good proxy for estimating lake temperature. For example, the reference of using coarse grid size of GCMs.

We agree with the Reviewer that the period was unclear and we reformulated the text as follows:
"Thankfully, long-term, high-resolution air temperature observational datasets are in general available, both for historical periods adopted to calibrate General Circulation Models (GCMs) and Regional Climate Models (RCMs), and for future periods where air temperature is a variable commonly derived from GCMs or RCMs projections. On the contrary, water tempeature measurements are far less available and future projections could be only obtained through the adoption of predictive models fully coupled with atmospheric and land surface models, which at the present stage is not a common practice (MacKay et al., 2009). In order to overcome these limitations (i.e. scarce availability and difficult estimation), several simple models have been formulated which use air temperature (widely accessible both for past and future periods) to derive surface water temperature of lakes."

8. Line 16-29 and page 5 line 1-10: Since streams and lakes work differently in terms of heat budgets, I recommend eliminating those unnecessary citations from

streams and replace them with more relevant and seminal literature from lakes

The text has been rewritten in order to clarify that models developed for streams and rivers are not necessarily suitable for the case of lakes, since the heat budget is different:

" Regression-type models, either linear or non-linear, have been successfully applied to estimate the temperature of rivers and streams, giving rise to a rich literature (e.g. Kothandaraman and Evans, 1972; Crisp and Howson, 1982; Webb et al., 2003; Benyahya et al., 2007; Morrill et al., 2005). Notwithstanding, these models cannot straightforwardly be extended to the case of lakes, especially for those water basins that have a significant seasonal hysteresis."

9. Page 5 Line 24: But the model proposed in this study is missing fundamental physical basis (i.e., solar radiation and wind).

The main assumptions and limitations of the model are now acknowledged in the revised manuscript as already discussed in points 1 and 2 of "General comments" section.

10. Page 6 Line 25-26: But similar heat exchange occurs at the interface between the epilimnion and hypolimnion.

The sentence was not sufficiently clear and has been rewritten as follows:
"The main heat exchanges occur at the interface between the epilimnion and atmosphere, and between the epilimnion and deep water (i.e. hypolimnion)."

11. Page 10 Line 10: If this model is using only one type of data (air temperature) to predict lake temperature the authors should be addressing the spatial variability of air temperature across the area.

We already discussed the possibility of applying Air2Water in a distributed manner in point 5 of "General comments" section.

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12. Page 11 Line 9-10: What other variables are not included?

The original sentence was unclear and we removed it as not necessary for the general comprehension of the paragraph. Our idea was to clarify that for model calibration only air temperature and surface water temperature are required, thus we did not consider the other variables that are recorded at the NDBC buoys (e.g. wind speed, pressure, water level).

13. Line 17-18: The location in the center of the lake is reasonable in terms of lake temperature. However, the authors should provide some information of variability across sites with available air temperature (e.g., Standard deviation across sites per month).

In this we do not agree with the Reviewer. Our goal is not to provide a spatial representation of lake surface water temperature, though we already clarified that this purpose can be achieved with the application of the model to different locations, or to analyze the spatial variability of air temperature, but rather to show that our modeling framework is flexible enough to reproduce surface water temperature by using as input meteorological forcing data from different locations or sources (i.e., buoys versus satellite imagery). In order to strengthen our reasoning we included in Figure 2 of the present document a simulations where the comparison between measured and simulated surface water temperature is provided (for the case of 8-parameter model) for a single location (45004 – Marquette) by adopting as input air temperature data from the different stations: STDM4 - Stannard Rock station ($35\ m$ above lake level) and PILM4 - Passage Island station ($22\ m$ above lake level). Though air temperature time series are significantly different, the model simulations are fully comparable and the Nash-Sutcliffe efficiency is greater than 0.9 in both cases. In our view this analysis clearly evidences the robustness and flexibility of our modeling approach.

14. Line 23: This model is very sensitive to the only predictor used (air tempera-

ture). Thus, detailed information of the database used should be provided. For example, a simple table with the proportion of gaps per year and season (e.g., in Table 1 when the missing data occurs). Are the gaps in the air temperature data interpolated? If yes, explain the procedure.

We agree with the Referee and reformulated the paragraphs in which the datasets are described (see points 15 in section "Specific Comments" and point 4 in section "Technical Corrections"). We honestly believe that a more detailed analysis of the dataset is not necessary (i.e. the proportion of gaps per year and season), and could just burden the reading of the manuscript. Furthermore we included the following sentence at page 2708 line 8:

"[...] is used for model validation. Missing data in the water temperature series have not been replaced (they do not contribute to the evaluation of the efficiency of the model); on the other hand, gaps in the air temperature series have been reconstructed with estimates obtained as an average of the available data in the same day over the corresponding period (i.e. calibration or validation). The datasets used in this work are listed in Table 1 [...]"

15. Line 27: What is a non-significant gap?

   We substituted "significant" with "systematic".

16. Page 18 Line 25: The authors need to provide a less subjective analysis of the "very good agreement" between observed and simulated temperatures. I strongly recommend a simple analysis of the residuals across the range of observed temperatures.

   In our view the "very good agreement" term is fully supported by the values of the Nash-Sutcliffe efficiency indexes obtained with model simulations for all the different configurations and data adopted. This index is introduced in Section 4.1 "Sensitivity analysis and model calibration". In all cases the Nash-Sutcliffe efficiency index is greater than 0.9, suggesting that model outputs are remarkably

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good. Nevertheless, we have deepened the analysis and comment of the results adding a new table (Table 1 in this document), listing Root Mean Square Error (RMSE) and Mean Error (ME) between observations and simulations (see also point 17 in this section).

17. Page 19 Line 20-21: The authors cannot argue the benefits of using this model (low error in predictions) because these results are not presented here.

We included these information in Table 1 in this document (which correspond to Table 3 in the revised manuscript). We also added a new figure (Figure 3 in this document, corresponding to Figure 11 in the revised manuscript). The text has been modified as follows:

"The physically-based, semi-empirical model presented here has been shown to provide an accurate description of surface water temperature of lakes, with high values of Nash-Sutcliffe efficiency index $E \simeq 0.9$, and a root-mean-square error between observations and simulations of the order of $1°C$ (see Table 3). This error in prediction capability is comparable to those obtainable using process-based numerical models (e.g. Fang and Stefan, 1996; Stefan et al., 1998), which however have the strong limitation of requiring high resolution weather data and the calibration of numerous internal parameters.

The close agreement between measurements and model estimates is further confirmed in Figs. 11a and 11b, which illustrate the parity diagrams for monthly-averaged surface water temperature during the calibration and validation periods of GLERL simulation, respectively. No systematic deviation (bias) is observed and the dispersion along the diagonal does not exhibit significant trends. Both these characteristics are confirmed by the small values of Mean Error (ME) and Root Mean Square Error $(RMSE)$ listed in Table 3. Figures 11a and 11b also illustrate that the model is able to adequately describe interannual fluctuations, as is indicated by the range of variability of monthly-averaged temperatures associated to the coldest (March, blue dots) and warmest (August, red dots) months.

This evidence is also confirmed by Figs. 9 and 10, where the model coherently reproduces the occurrence of relatively colder (e.g. 2004) and warmer (e.g. 1998) periods."

18. The same comment for page 22 line 7-9).

The simulations presented in this document (see Figure 2) related to the application of the model with data from a different weather station (the PILM4 - Passage Island) support our statement present in the original manuscript. We honestly believe that model capabilities to predict lake surface water temperature with data originating from completely different sources have been fully demonstrated and, if it is in line with the policies of the Journal, we would prefer to keep that sentence as it is written in the submitted manuscript without the need of including additional simulations whose results are however qualitatively identical to those already presented.

19. Page 19, line 23-24: This limitation is in agreement with my previous comment (Page 10 Line 10; Page 11 Line 17-18 and 23).

In this case the limitations refer to process-based models and not to our model. The paragraph has been rewritten (see point 17 in section "Specific comments").

20. Page 22 Line 14-16: Where are the results to support capturing the inter-annual variation? Also, in page 23 line 22 this part is mentioned and needs to be deleted

Figures 5, 7, 9 and 10 on the original manuscript show that the model is able to suitably reproduce the long-time series of data, well capturing the occurrence of relatively colder and warmer years. In order to strengthen our analysis we added a new figure (Figure 3 in this document) and a new paragraph aimed at better pointing out this fundamental capability of the model (see point 17 in section "Specific comments").

The sentence at page 2719 (23) line 22 is part of the general conclusions of the manuscript and for this reasons we prefer to keep it.

21. Line 20-26: This part is a bit speculative based on the problems of GCMs and the spatial resolution of these models. Only in a few cases of large lakes when the main forces of heat budget (solar radiation and wind) are strongly synchronized to air temperatures may you consider this application. I recommend deleting this part and not using it to over sell the model.

We believe that this point is not speculative. For instance, Piccolroaz (2013) successfully used an equivalent model (i.e. a previous version) to estimate surface water temperature of Lake Baikal from air temperature provided by a GCM, both under current and expected climate conditions. In order to better clarify this part, we rewrote the paragraph at page 2718 lines 20-26 as follows (see also point 7 of section "Specific comments":

"Therefore, in principle, air temperature series provided by GCMs and RCMs can be used as well. In this regard, the model is particularly attractive for climate change impact studies, since predictions of air temperature are usually more reliable and available than other meteorological variables (e.g. Gleckler et al., 2008). Based upon these considerations, Piccolroaz (2013) exploited the same approach to reproduce the current status and to predict future modifications of surface water temperature of Lake Baikal (Siberia)."

22. Page 23 Line 25-26: This statement is not supported.

In points 2 and 3 of section "General comments" and points 17, 20 and 21 of section "Specific comments" we already commented on several aspects related to this issues showing how the model is able to reproduce observed trends in surface water temperature and thus to predict future state of this variable when forced by projections provided by climate models.

23. Rather, the authors could focus on explaining what we gain with this model versus simple correlation models between water and air temperatures?

We think the benefits of using this model instead of simple correlation models are fairly evident: i) capturing the hysteresis cycle between air and water temperatures, ii) reproducing the interannaul variability, iii) providing information about the variability of the epilimnion thickness. Furthermore, simple correlation models are stationary models which do not allow reliable projections for future different conditions. With this respect, we added a new sentence in the conclusions in order to further stress this point (page 2719 line 23)

"[...] and the inverse startification process which typically occurs in dimictic lakes. In our view, Air2Water represents a valuable alternative tool to correlation models, which require the same data in input as our model but are not able to address some fundamental processes (e.g. the hysteresis cycle between air and water temperature). Furthermore it can be used in place of full process-based models when meteorological data are not sufficient for their effective application."

24. Page 39 Fig. 5: A most relevant analysis should consider only extreme values rather than every single value. See that most of the differences occur over extreme values. This figure can be improved showing two panels, the first with air temperature and the second with the stream temperature (observed and simulated). Each panel may have a different scale for temperature (lake temperature should be expanded to improve visualization). Similar comments for figures 7-10

The model has not been designed to reproduce surface water temperature at daily scale, but rather at monthly to annual scales (this aspect is stressed in the revised manuscript). For this reason, we think that the analysis of daily residuals is not particularly relevant. Notwithstanding, in order to provide a more detailed analysis of results, we included the new Fig. 3 and Table 1, as already discussed in point 17 of section "Specific comments".

Concerning the second part of the comment, we think it is better to plot air and surface water temperatures (observed and simulated) in a single panel. Plotting the temperatures in two separate panels would not allow one to clearly appreciate the hysteresis cycle between air and water temperature.

25. Page 45 Fig.11: Provide time scale of measurements (daily values?)

We modified the captions as follows:

"Comparison of the hysteresis cycles between daily air and surface water temperatures, as derived by the data and by the 8- and 4-parameters versions of the model. Hysteresis cycles refer to the mean year, calculated over the period 1994-2005, using GLERL and NDBC data for T_w and T_a, respectively (GLERL$_{my}$ simulation)."

3 Technical Corrections

1. Page 6 Line 5: Provide a short list of these external forces.

   For the sake of brevity we substituted "external" with "meteorological".

2. Line 15: Provide proper citations.

   In order to clarify the meaning, the sentence has been modified as follows: *"The key objective of the present work is thus the definition of a modeling framework which could allow for a consistent description of the physical principles governing lake surface temperature, and ensures a general applicability of the model (e.g. over the entire year)."*

3. Page 7 Line 11: The authors mention that wind is the major driving force, but previously they mention air temperature playing this role. Please, clarify and be consistent with the use of concepts and driving forces.

We substituted *"primarily wind, which is a major driving force for lakes"* with *"e.g. wind speed, solar radiation, humidity, cloudiness, which besides air temperature are the major factors controlling the heat budget of lakes"*.

4. Page 12 Line 1: Provide the temporal scale (daily? weekly? monthly?).

In order to answer this comment and some comments of the Referee # 1, the part describing the GLERL dataset has been revised as follows (from page 2707 line 24 to page 2708 line 4):

"Concerning GLERL dataset, daily temperature maps have been used for the period 1994 to 2011. Data refer to the daily lake average surface water temperature obtained from NOAA polar-orbiting satellite imagery. The series does not present systematic gaps (missing data, see Table 1, are concentrated in the first, warm-up year and hence do not contribute to the evaluation of the model efficiency, see Sect. 4.1), thus providing surface water temperature also in winter, which, on the contrary, is almost completely uncovered by the NDBC dataset. A mismatch between NDBC and GLERL datasets is visible in the rising limb of the annual cycle of temperature (i.e. between April and July, see Fig. 8a), which is likely to be a consequence of the different spatial scales of the two series of data: while the NDBC dataset represents surface water temperature measured nearly at the center of the basin, the GLERL dataset provides values averaged over the whole lake. In the latter case, the spatial variability of surface water temperature (e.g. in spring, lake water heats from the shores towards the offshore deeper zones) is intrinsically included in the estimates, thus determining smoother annual cycles of temperature. Despite this discrepancy, Schwab et al. (1999) compared GLERL data with measurements at some of the NDBC buoys finding an overall good agreement. In particular, for the case of the 45004 - Marquette buoy used in this work, the mean difference between the two datasets for the period 1992 - 1997 is less than $0.28°C$, the root mean square error is $1.10°C$ and the correlation coefficient is 0.96."

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5. Line 10: Provide the software package used to numerically solve the equations.

The code is written in Fortran, and no solver packages have been used. Since the explicit Euler scheme is straightforward, we think that a reference to a specific book is enough. The text present at page 2708, line 10 has been modified as follows:

"The differential Eq. (6) has been solved numerically by using the Euler explicit numerical scheme (see e.g. Butcher, 2003), with a daily time step (concerning NDBC data, mean daily temperatures have been preliminary calculated from the original data)."

We also modified lines 11-13 at page 2710 as follows:

"Just as a sidenote, 100 000 000 model runs over a period of 18 yr with a daily time step and adopting Intel(R) Xeon(R) CPU X5680 @ 3.33 GHz took around 2 h; the code is written in Fortran 90."

4 Tables and Figures

Table 1. Efficiency index (E), Root Mean Square Error ($RMSE$) and Mean Error (ME) during calibration and validation periods (NDBC, GLERL and GLERL$_{my}$ simulations).

n par.	calibration			validation		
	E $[-]$	$RMSE$ $[°C]$	ME $[°C]$	E $[-]$	$RMSE$ $[°C]$	ME $[°C]$
	NDBC (cal: 1985-2002; val: 2003-2011)					
8	0.91	1.40	-0.07	0.90	1.71	-0.01
6	0.91	1.35	-0.09	0.90	1.71	-0.02
4	0.89	1.50	-0.25	0.89	1.77	-0.01
	GLERL (cal: 1994-2005; val: 2006-2011)					
8	0.95	1.17	-0.13	0.97	1.02	0.30
6	0.95	1.16	-0.07	0.97	1.01	0.33
4	0.95	1.21	-0.05	0.97	1.08	0.38
	GLERL$_{my}$ (cal: mean year 1994-2005; val: 2006-2011)					
8	0.99	0.48	0.06	0.97	1.07	0.23
6	0.99	0.43	0.01	0.97	0.98	0.15
4	0.99	0.47	0.03	0.97	1.11	0.29

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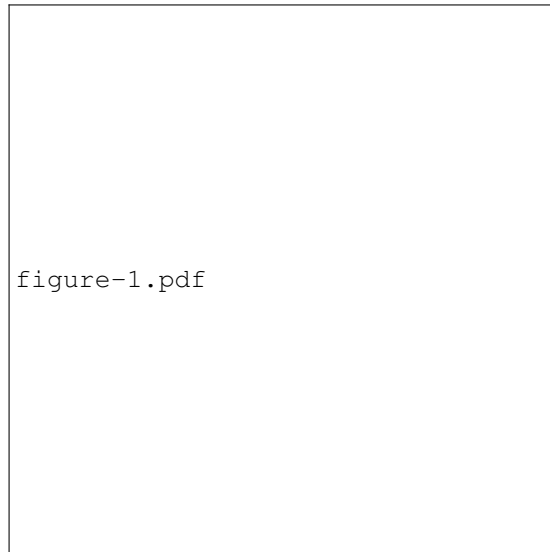Interactive Discussion

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figure-1.pdf

Fig. 1. Comparison between summer (July - September) mean observed air temperatures (cyan), mean observed water temperatures (blue) and mean simulated water temperatures (black). The linear trend of the three series are depicted as well. Air data refer to STDM4 - Stannard Rock lighthouse and water temperature to 45004 - Marquette NDBC buoy.
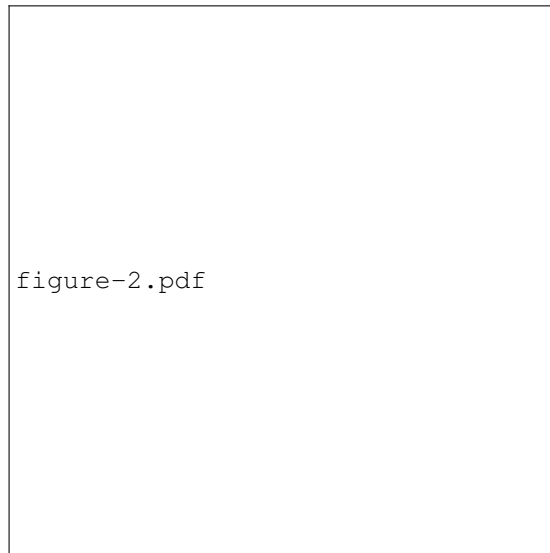
figure-2.pdf

Fig. 2. Comparison between air temperature, and measured and simulated surface water temperature (8-parameter model): a) using air temperature data from the the STDM4 - Stannard Rock station ($35\ m$ above lake level) and b) using air temperature data from the the PILM4 - Passage Island station ($22\ m$ above lake level). Despite air temperature series are significantly different, model results are fully comparable and the Nash-Sutcliffe is greater than 0.9 in both cases.
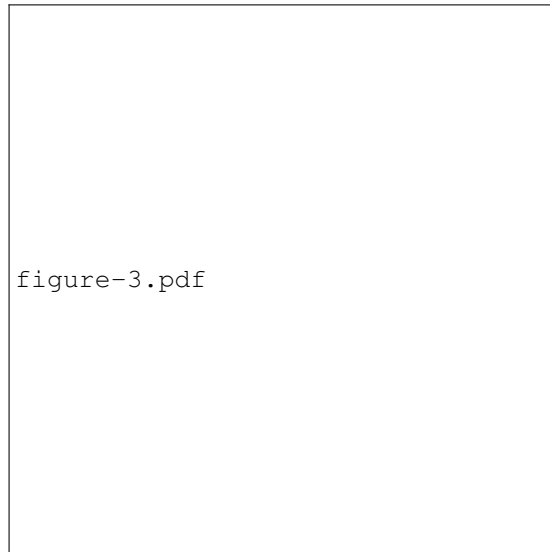
Fig. 3. Parity diagram for monthly-averaged surface water temperature (8-parameters version of the model): a) calibration and b) validation period of the GLERL simulation. Blue dots refer to March, red dots to August and grey dots to the remaining months of the year.

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