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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Satellite-Derived Light Extinction Coefficient and its Impact on Thermal Structure Simulations in a 1-D Lake Model" *by* Kiana Zolfaghari et al.

Anonymous Referee #2

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Reviewer comments to Satellite-Derived Light Extinction Coefficient and its Impact on Thermal Structure Simulations in a 1-D Lake Model by Zolfaghari et al. 2016

General comments

This manuscript deals with water optical properties which are acquired by remote sensing, and which are then used as input to 1-D lake modeling. This approach is important and needed addition to current efforts of incorporating lakes and reservoirs to weather prediction models. Water clarity is an important factor in defining lake heat budget and thermal stratification and thus is a significant parameter for processes in the air-water interface. With millions lakes of different sizes around the world, comprehensive direct measurements of water clarity are not possible, highlighting the need for indi-

rect estimates of water optical properties. Satellite measurements show great promise in mapping light extinction coefficient, a parameter to define water clarity, and to my knowledge this study is the first one to incorporate lake modeling to water clarity defined from satellite observations. This is an important study with wide interest in scientist from different fields and therefore I find this study appropriate for HESS. However, there are a few major points which prevents me from recommending this manuscript for publication as it is.

The major topic of this manuscript is that satellite-derived light extinction coefficient, Kd, represents well the in situ measurements of Kd, that it can be used as an input to lake modeling, and that it enhances the performance of FLake as compared to the current approach of using constant Kd of 0.2 m-1. These points should be emphasized. Currently, the manuscript seems unbalanced, with much of the focus given to topics not strictly the main theme of this manuscript. Also some restructuring is needed so that the reader does not get distracted from the main focus. In general, the manuscript would benefit from reducing the amount of figures.

Since this is the first approach in combining satellite-derived Kd to lake modeling, it would be of value to describe the strengths and the weaknesses of this approach. How easy and accurate method this is for the modeling community in general; can this be used without in situ measurements of Kd or should there always be e.g. Secchi disk measurements for validation; what are the next steps needed for applying this method in broader context.

Specific comments

In regards of restructuring, I will give here an example how the figures (and related discussion) could be rearranged. After the map, I suggest to first show satellite-derived Kd at the site of FLake modeling (current Fig. 8), which is the main input parameter under focus here. For the estimated solar irradiance and incoming long-wave radiation (Figs. 2 and 3), the figures add only little to the reported statistics and thus these two figures could be removed. After showing the satellite-derived Kd, it would be logical to show their validation (current Fig. 4). Then the results of models against measurements, i.e. current Figs. 9 and 10. Current Figs. 11, 12, 13 and 14 basically show the same data in different forms. I suggest either to combine Figs. 11 and 12 and show only this, or show only Fig 13. Lastly, current Figs. 5 and 6 could be shown, which would then lead to the discussion of the strengths of satellite-derived Kd and possible future studies (see also the related comment later). This is only a suggestion for restructuring, it could also be done otherwise.

Page 4, line 9. Air humidity, which is used as FLake input, is taken from land 10 km away from the lake site. Air humidity is important for modelled latent heat fluxes. Could the authors briefly state their opinion how well the measured air humidity represent that over lake, and how or if this affects the modeled results.

Page 4, lines 19-21. Sentences starting with "Available Secchi disk" and "SDD data was" are repetitive and should be merged to one sentence. Also, it could be mentioned here that the SDD data comes from Limnos cruises.

Page 6, lines 1-2. Please clarify this sentence. Does this basically mean that only the pixels which were not rejected according to the criteria in Table 1 were used?

Page 7, Chapter 3.1.1. A lot of space is dedicated for this, and therefore a justification could be given in the first sentence. E.g. "Validating the satellite-derived Kd with in situ observations is important because…" And in the end of the chapter the outcome of the evaluation, e.g. "For these reasons, we deem the satellite-derived Kd correct and thus were confident in using them in the modelling." Also, in Chapter 3.1.1. or later, the authors could discuss whether this kind of validation is always needed with satellite observations, what are the implications, etc.

Page 8, Chapter 3.1.2. This chapter seems interesting but out of place. These results are not further elaborated, and therefore I suggest to move them to the end of the Discussion. This way the authors could show what benefits remote sensing of Kd would

bring (spatial and temporal variability, which is not achieved well with manual sampling; perhaps good input for 2D and 3D modeling), which would lead to the discussion of possible next studies. This way also the key input parameter, Kd at the NDBC station, would be shown earlier.

Page 9, Chapter 3.2.1. If the satellite-derived Kd has been validated sufficiently well and it produces better simulations, what would be needed for the simulations to match the measured LSWT more accurately? This would lead to suggestions for future research.

Page 9, Paragraph starting 'Fig. 9 shows the results…'. I suggest to first describe the observed behavior in the temperatures and then discuss how the modelled behaviors compare to these.

Page 10, lines 17-18. This is quite strong statement and probably not true for all lakes. Lakes are very heterogeneous, be more specific which type of lakes is meant here.

Page 10, Chapter 3.2.2. This chapter needs the most restructuring. E.g. the paragraph on page 11, lines 25-28, could be removed. The two first sentences are basic limnological knowledge and the last sentence does not really lead the story further. In this chapter, the theme of light penetration and absorption is discussed in many places, e.g. on page 11, lines 8-9, lines 11-12 and lines 29-34, page 12, lines 11-13 and lines 19-20. Remove excess repetition. The last paragraph on page 12 (starting Fig. 14 depicts…) repeats what is said earlier and is not the main focus of this study, therefore I suggest to remove that paragraph. The last paragraph of Chapter 3.2.2. discusses about modeled ice cover. This seems a bit out of scope and there really seems to be no ice measurements against which to validate modeling. For this reason, I suggest to either remove this paragraph or significantly shorten it.

Page 11, lines 11-12. The authors seem to mix two concepts here. Darker water color is related to dissolved substances, such as colored dissolved organic matter, not to particulate matter.

Page 11, lines 13-14. The authors over-simplify the underlying mechanisms for LSWT behavior. The loss of energy to the atmosphere is related to the surface water temperature (and wind), not only in fall but throughout the open-water season. However, the mechanism how mixed layer depth affects the rate of heat loss needs more explaining.

Page 11, lines 18-23. Tie these results from the literature more tightly to the findings in this study, e.g. by writing whether this study supports or opposes previous findings. Also, the sentence on lines 21-23 (starting 'Heiskanen et al...') could be removed either from here or from the Summary.

Page 12, paragraph starting with 'Fig. 12 shows'. Here full mixing is described in very atypical way on several occasions, e.g. by 'highest depth of mixing' and 'reaches maximum MLD'. I suggest to describe these occasions either by discussing of overturn, of full mixing or similar.

Page 12, lines 8-9. If this is the reason for earlier overturn in simulations with clearer water, how the authors then explain the results shown in Figs. 11 and 13 where it is evident that there is full mixing in the beginning of September in CRCM-12.6 simulation with temperatures of about 20 deg C? Fig. 14 also shows that the clearer the water, the higher the water temperatures in Oct and Nov. Note that in addition to convection, mixing is related to wind forcing and density gradient in the water column.

Page 12, lines 16-17. MLD is not influenced by the thermal structure, but it is part of the thermal structure. I would remove this sentence.

Page 12, lines 13-14. Fig. 13 is essentially the same data as in Fig. 12 and therefore one cannot be used to confirm the results of the other.

Page 12, lines 20-22. Deepening of the thermocline is related e.g. to wind forcing and thus it cannot be suggested that thermocline deepening in clear waters is monotonic. Also, it is not clear what is meant with 'stabilize the temperatures'. I suggest to remove this sentence.

Technical corrections

Page 6, line 23. The same result 'was' found for...

Page 8, line 32. 'leads to higher water clarity'. The authors must mean lower water clarity.

Page 9, line 18. The sentence starting 'The Kd values' and the sentence after that could be merged and rephrased. E.g. 'The monthly-averaged Kd were used to simulate the surface water temperature and produce a merged LSWT (Merged)."

Page 9, line 20. Comparing LSWT in situ observations (Obs) with...

Page 9, line 21. How can the authors compare measured and modelled surface temperatures in April when there seems to be little or no measured LSWT during April, at least according to Fig 9?

Page 10, lines 2-3. Rephrase. Do the authors mean that the annual average of Kd can occasionally be closer to the actual Kd than the monthly-averaged Kd? This same topic is also mentioned in lines 16-17, and at least to me it is unclear how yearly average value (i.e. one single number) can represent the extent of Kd variations (i.e. how big is the range).

Page 10, line 10. Please clarify what is specifically meant with 'are as affected'.

Page 10, lines 11-12. 'This can be explained by...'. Be more specific in telling how lake depth explains this.

Page 10, lines 12-13. This should be self-evident if the model is any good, and therefore I suggest to remove this sentence.

Page 11, line 9. '... causing thinner mixing depth (Fig. 12)'

Page 11, line 35. Change 'when Kd changes...' e.g. to 'when maximum (minimum) Kd is used instead of its average value...'

Page 12, line 1. Similar comment as previous. This is a bit misleading wording since it gives the idea that Kd changes naturally, whereas what is meant that different Kd is used as an input.

Page 13, line 27. Write open the abbreviation ‘LSWT’ here. It is not typical abbreviation and not clear for those who only read Summary and conclusions.

Page 14, line 3. Change ‘has’ to ’have’.

Comments to figures

Fig. 1. It would be of interest to see the main river inlets and outlets. This way it would be easier to assess how much river inflow possibly affects modeling results.

Fig. 5. Remove ‘Lake Erie boundary’ from the legend, it is not needed. Also make the color bar much larger. Same comments for other similar figures.

Fig. 8. It would be of interest to see the SDD at this location (or from the nearest location where those exist) together with these CC-derived Kd. These could be marked to the same graph with secondary y-axis.

Fig. 10. It would be interesting to see the performance for each year separately. This could shown by plotting each year with different color. Also, it is more standard to show these kind of scatter plots as box plots (both axes of same length).

Fig. 11. The measured LSWT should be shown. Otherwise, it is impossible to say which simulation performs the best. Use a) and b) for these two graphs. Also in the legend, the Kd values could be shown for each model run.

Fig. 15. Model run CRCM-12.6. is not visible. If the resolution can not be increased, describe in the caption where the line is.

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