

We would like to thank all the reviewers for their constructive comments which helped improve the manuscript. Our replies to comments are covered below.

General comments

The manuscript presents a study on the use of light extinction coefficient values derived from MERIS satellite imagery in the FLake 1-D lake model. FLake is the most widely used lake scheme in numerical weather prediction (NWP) models. To take advantage of the coupling of lake schemes to NWP and climate models it is necessary to have data on the lakes transparency. As they are not observations in an in-situ operational way, the most promising strategy is to use satellite images. Therefore this study deals with a current scientific issue that really fit in the HESS scope. As far as my English allow, the manuscript is well written. The study is original and contains new results which are worth to be published. In my opinion the manuscript requires major revision before being accepted. Please, consider the following comments, with different levels of importance:

Detailed comments:

pag. 2 line 3 land → continental

It has been corrected, thanks.

line 12: In the first mention to the FLake model, a reference to the model may be given. This: Mironov, D. V., 2008: Parametrization of lakes in numerical weather prediction. Description of a lake model. COSMO Technical Report, No. 11, Deutscher Wetterdienst, Offenbach am Main, Germany, 41 pp. or this: Mironov, D., E. Heise, E. Kourzeneva, B. Ritter, N. Schneider, and A. Terzhevik, 2010: Implementation of the lake parametrisation scheme FLake into the numerical weather prediction model COSMO. *Boreal Env. Res.*, 15, 218-230.

Mironov et al. (2010) is already added. Mironov (2008) has been added to the new manuscript as well.

line 13 "artificially limited to 40-60 m depth". Is not artificial. Flake is not able to simulated deep lakes, as it consider only two layers. To lakes deeper than 40 meters it will be necessary to consider one third layer below the thermocline.

By using the word "Artificially", we mean for deeper lakes (depth more than 40 m), which also form hypolimnion, an artificial depth of 40 m is used in simulation rather than the actual lake depth to only reproduce lake properties in epilimnion and thermocline layer.

page4 line 3. Only 1 station for all the lake?

Only one station is selected as the purpose of this study is to investigate how satellite-derived lake water clarity can improve a 1-D lake model such as FLake in comparison with NDBC station observations, which has lake surface water temperature in situ observations available to evaluate the performance of modeling while employing different values of K_d .

page 5 line 10: T at which station, at which level?

Page 4 line 2-5 describe the location of station and the level that air temperature measurements were conducted.

line 15 – Why not use data from analysis (From ECMWF, for example)

The ECMWF data are not used because of the of the resolution differences. Authors preferred the modeling methods used in this study. The modeling also achieved acceptable accuracies.

Section 2.3 In my opinion, this section should identify the time periods in which Kd MERIS images were available. This information exists in a dispersed form in section 3.1.2..

Section 2 includes a description of the sources of data used in this study. Subsection 2.3 also has information about the satellite derived water clarity and how this data is produced and extracted in general. Section 3 cover results of applying the considered methods to derive information for Lake Erie NDBC station. Therefore, subsection 3.1.2 describe water clarity information derived from satellite observation on Lake Erie and specifically for NDBC station. This section ends with a time series of satellite-derived Kd values at NDBC station. The reader can find out from this graph, that how often and at what time of year, the satellite-derived Kd values were available at NDBC station.

line 26 – 31. It is not clear which Kd is used: in a spectral band (which ?), broadband ?

The Kd average value in the visible part of spectrum was used. This information has been added to the new version of manuscript.

page 6 Section 2.4: In this section, the set-up of the simulations should be clearly presented, namely: - How were the FLake prognostic variables initialized? - The model integration period (start and end) - The temporal resolution of the forcing and the time step of the simulations.

It has been added to the new manuscript.

lines 14/11 In this paragraph the ice parametrization used should also be introduced as it was activated. (maybe also the snow scheme).

It has been added to the new manuscript.

line 18: what means “The setup conditions”?

The condition that the observation at buoy station are collected, is setup condition; and include the parameters summarized in the same line in brackets.

line 19/20- depth of the water temperature measurements: why is it included here? The water temperature is not a forcing parameter...

“z_Tw_m” is one of the inputs in FLake model and is the depth of water temperature measurements.

line 21: why use the local depth (12.6 m) and not an averaged depth, maybe of the western basin?

The exact depth of NDBC station is used in simulations to remove the effect of depth on simulation results and only focus on the influence of water clarity.

line 21: “to configure” means force initialize, or both?

The parameters mentioned in the bracket are constant and used to force the FLake model.

page 7 line 11 (Eq. 2) and line 30 – Equation I don't understand the process to adjust a relation between K_d and SDD. The equation Eq 2 indicate an inverse proportionality, but the expression obtained is not linear.

Eq 2 is a general format of the relationship between these two parameters, where K is a constant value. This relationship was based on a pioneer study. After that, there were other studies that derived an empirical relationship between two parameters with similar inverse relationship. The relationship derived in our study is also empirical and specifically derived using data collected for Lake Erie; also it is still inverse and in a similar format as the general equation that introduced by the pioneer study.

page 8 line 8: “can explain”? or can detect?

“explain” has been changed to “detect”.

line16: $K_d = 0.87 \text{ m}^{-1}$: is a satellite-derived value or was in-situ measured? By the way, are there any in situ measurements on the selected day?

The map is derived from satellite observation and as it is mentioned in the manuscript, K_d value of 0.87 m^{-1} is for NDBC station shown on the map.

There are no in situ SDD measurements in NDBC station during that period of time of our study.

line 18: “on a monthly-basis for one year” but only four months were considered.

Fig. 6 aim to show the monthly variations of K_d . Therefore, because MERIS images were available for four consecutive months in 2010, images in this period (May-August 2010) were selected.

“for one year” has been changed to “one selected year”.

Figure 6 shows only particular days and not average values, but the text refers to monthly average values. This question is valid also for the discussion of the results presented in Figure 7. It should be indicated how the averaged values were calculated.

The average values mentioned in line 19 in page 8 are average value for the full lake in a specific day. Therefore those are spatially-averaged, not temporally. The same explanation is valid for Fig. 7. It has been clarified in the manuscript.

Figures 5, 6 and 7. With the chosen color scale, most of the field is in the same color. I think it would be better to use a color scale with higher resolution especially in lower values (could be a non-linear scale, possibly logarithmic). Also, the color scale should have a more detailed legend.

Will be corrected in the new version of manuscript.

line 21/22 and 30: the values of K_d are satellite-derived values or were measured in-situ?

The values are extracted from the maps and therefore are satellite-derived K_d values.

line 23 “full coverage of Lake Erie were only available in May of two consecutive years (2008 and 2009)”, but figure 6 show a map for May 2010. Contradiction?

This sentence means that the only month, in two consecutive years, with full coverage MERIS images were only available in May and not any other month. Therefore May of these two years were selected to show variations of Kd. The selected two years could be potentially 2009 and 2010; however, because map of May 2010 was already shown, authors preferred to compare the maps of May 2008 and 2009.

page 9 Neither in the section 2.4, nor here, the start of the simulations were indicated. line 18 and 20: Which depth were used in the Avg and Merged simulations?

All simulations are in the actual depth of 12.6, unless otherwise is mentioned, which is for CRCM-20 simulation. It has been clarified in the new version of manuscript.

In the Figure 9, results for 2007 there are plotted values for Kd for fall. Why are this values not shown in table 2 and not used in the merged simulation?

As it is mentioned in the caption of Table 2, only values that have both LSWT observations and satellite-derived Kd available, are used in the merged simulations. In fall 2007, there are no in situ LSWT (observation in black line).

Analysis of figure 9: Some explanation for the strange spike that occurred in mid September 2006 in the avg simulation? A less pronounced effect also occurred in mid

We are not sure if we understood the comment correctly!

page 10 line 3. It seems strange to defend that year average can be more representative of Kd variations than monthly averages...

Monthly averages are calculated based on satellite-derived Kd values, which might not be available due to cloud coverage in MERIS images. However, there are more MERIS images available in the longer period of one year that can potentially catch the actual variations of Kd value, rather than only a few images (or even none) in a month. Therefore yearly-average Kd could be potentially closer to the actual Kd value.

line 5 “Turbid waters in these months simulate colder LSWT”. this statement must be explained. In my opinion, the reason is not on the fact that during those months the waters are turbid, but because the water was more turbid before, during spring and summer, reducing the heating of deep water. This should be discussed further, in particular by analyzing the evolution of deep temperature and column mean temperature (Flake variables)

This statement is explaining the negative MBE in 2005 as opposed to 2006 and 2007 simulations the difference in calculating MBE for years of 2005 - 2007 is taking months of Sep-Nov into calculations. On the other hand the Kd value for same months in year 2005-2007 are in the same range. Therefore the underestimation of LSWT in 2005 cannot be related to more turbid waters before, in spring and summer.

line 11 “This can be explained” → “this is due to”.

It has been corrected in the new version.

line12 The conclusion that “a realistic lake depth and Kd value will improve model results” is obviously correct, but, specially concerning the depth, could not be demonstrated using only the two depth values considered.

This conclusion is based on having two simulation of CRCM-12.6 and CRCM-20 that only depth is changing; and between these two simulations, CRCM-12.6 is reproducing LSWT more closely to the observations. Page 9 line 9-10 also mentioned that the study by Martynov et al. (2012) also had the same conclusion regarding depth.

Figure 10 a and b: A hypothesis: If different colors were used for spring and summer fall it may be possible to see and then discuss two different behaviors

Thanks for your suggestion. Colors will be added to the figures to investigate the potential seasonal behavior.

Caption of figure 10: the means of a,b,c,d should be indicated

The means has been added in the new version of manuscript.

line 16/17 I do not agree with:”It is possible that the extent of Kd variations is best represented by the yearly average value”. Maybe the problem is that the errors in the determination of monthly mean values may result in a worse simulation...

The possible error in the monthly mean values of Kd is due to the fact that the monthly variations might not be captured by limited MERIS images. However, there are more MERIS images available in a year that can capture the actual variation of Kd value; and the average value is derived based on a larger sample of Kd values; therefore has a higher chance to be close to the actual Kd value.

line 24-26: I would be less categorical, adding for example at the beginning of the statement something like: “In the absence of reliable values of the temporal evolution of Kd,”

It has been added in the new version of manuscript.

Considering that 12.6 m is the realistic lake depth must be better justified. see my comment (page 6 line 21).

We are not sure if we understood the comment clearly.

line 33: The sensitivity of FLake to LSWT, MWCT, MLD, isotherm, ice phenology and thickness???

It has been rephrased in the new version of manuscript.

page 11 lines 2/4: which depth were used in the sensitivity simulations?

The real depth is used for all simulations, except for CRCM-20.

lines 4/5 More than the depth, what is important is to indicate the value of Kd in the RCM-12.6 simulation.

The setup condition for both CRCM simulations are mentioned in the beginning in page 9 line 15-17.

line 7 (maximum or Max) → Max will be enough.

The simulations name is abbreviated to use further in the manuscript.

line 8 The world faster does not seem the most appropriate in: “solar radiation is absorbed faster in turbid waters”. What happens is that the radiation is more absorbed in the water surface layer, as explained by the authors afterward.

Solar radiation is both absorbed faster and also more in turbid waters compared to clear ones. This is the reason for abrupt fluctuations in LSWT for turbid waters shown in Fig. 11. As it is more illustrated further in the same paragraph.

line 9 “This shallow layer exchanges heat faster with the atmosphere”, is correct but should be explained, In my opinion the main reason has to do with the fact that the as the surface water temperature is higher the sensible and latent heat fluxes increase.

Thanks for the comment, this discussion will be expanded in the revised version of the manuscript.

line 12/13 “However, in fall the loss of energy to the atmosphere is also faster due to the shallow mixing depth” This will not be the main reason. In my opinion the main reason has to do with the fact that the deep (and the mean) water temperature is lower.

We are not sure if we understood the comment correctly!

line 14: “least turbid water” The use of the word “least” here can be confusing, as it is less turbid than what considered in the CRCM-12.6 simulation

What means by “least turbid” is further explained in the bracket. The least turbid water simulation in this study is shown in Min simulation.

line 15: “Min” is enough

The simulations name is abbreviated to use further in the manuscript.

line 18: “FLake is not significantly sensitive to LSWT” It is not correct in terms of English

It has been corrected in the new version of manuscript.

line 25: Please delete the sentence: “For both clear and dark waters, LSWT is warmer than the MWCT, due to being exposed to more intense solar radiation.”. The reason is the density! (for water temperatures over 4°C)

It has been corrected in the new version of manuscript.

lines 25/28. In this discussion it will be interesting to compare also with the FLake deep temperature.

We are not sure if we understood the comment and the term “deep temperature”.

page 12 line4 “two turnover”. In my opinion the first period without stratification should not be identified as a turnover. As I can imagine, the Flake were initialized with a constant temperature profile.

We believe that the constant temperature profile from the top to the bottom of the lake should be considered as mixing, if we understood the comment correctly!

line 8/9 “As a result, the water column in clear water reaches the temperature of maximum density (4°C) much faster than turbid water ...”?? is not what we can see in Figure 11 (bottom) and in Figure 14!

Thanks for catching this mistake. The sentence has been removed in the new manuscript, since turnover can happen in different temperatures as long as the water column is in the same temperature.

lines 10 / 15 The average values over the whole period does not seem to be relevant in this discussion

The Avg simulation is considered as a base for comparison between simulations with highest and lowest Kd values (clear and turbid waters).

line 10 “In the turbid” → “In the more turbid”

It has been corrected in the new version of manuscript.

line 13 “distribute to” “be absorbed in” or “distribute energy to”

Comment is not clear. Solar radiation can be distributed in a volume.

line 16: We can not say that “The MLD is influenced by the water column thermal structure”. The MLD is itself a parameter used by Flake to characterize the water thermal structure...

Thanks for your comment. The sentence has been removed in the new manuscript.

Caption of figure 13. Please, improve the wording... The 4 individual figures should have the same caption.

It will be improved, thanks.

line 19: “but also warmer” is not valid for the whole period. I think it will be more correct to say something like: “warmer in spring and summer, and colder in fall”

It has been corrected in the new version of manuscript.

line 19/ 20: the sentences: “The reason is that solar radiation is mostly absorbed at the upper layers in turbid water. Thus, the radiation is used to warm up a thinner layer in dark waters leading to higher temperatures.” are correct but the argument is repeated sometimes on the text. In my opinion, it will be better to explain in a more integrated way, based on physics of course, the differences between clear and turbid waters.

This discussion will be expanded in the revised version of the manuscript!

line 21: “shows that the deepening of the thermocline layer in clear waters is monotonic”. I can not see this. Can you be more precise?

Figure 13 shows that in the simulation related to the clearest waters (CRCM-12.6), deepening of thermocline is faster, with a monotonic speed, as opposed to the dark waters.

line 29: before the “increased effect of cooling from the layers below” it should be noted that as the surface temperature of the turbid lakes is higher, the radiative losses to the atmosphere are greater. So, during the heating period, a turbid lake as a whole, loses more energy by radiation and therefore stores less energy..

This is mentioned in page 11 line 29.

page 13 (before Summary and Conclusions) It is difficult to analyze the discussion contained in this page without knowing the details about the initialization of the simulations. And about observations? When occurred the break-up and the freeze-up?

The timing of break-up and freeze-up for all simulations is mentioned in this paragraph. The initialization of the simulations has been added to the new version of manuscript.

line 13. “Dark waters store more heat in a shallower depth.” The sentence may be misunderstood. First consider change “depth” by “layer”. But if one consider the whole water column, dark waters store less heat. “Therefore, in the winter time”. In my opinion the “in the winter time” should be deleted, as this is also valid in summer and autumn (and may be more important during these seasons)

“depth” has been changed “layer”; and “in the winter time” has been removed in the new of manuscript.

page 16 line 21. Arkady Terzhevik should be added to the list of co-authors.

Thanks for noticing this. It has been added to the new version of manuscript.