

In my opinion, the authors improved the manuscript and responded satisfactorily to most of the questions that I put in my first review. However, I still have disagreements regarding 5 issues. In particular, the question 4 is to me a major issue.

The review is based on the of file hess-2016-82-author\_response-version1.pdf

1. page 6, line 21/22

In my first review, I asked: depth of the water temperature measurements: why is it included here? The water temperature is not a forcing parameter...

The authors reply that:

“z\_Tw\_m” is one of the inputs in FLake model and is the depth of water temperature measurements.”

Which is not correct.

I'm not sure about what is z\_Tw\_m (It is not present in the standard Flake code, see: <http://www.flake.igb-berlin.de/docs.shtml>), but I have no doubt that Flake don't need any “depth of water temperature measurements” as input.

2. Page 6 line, line 25: I asked “to configure” means force initialize, or both?

The authors reply: The parameters mentioned in the bracket are constant and used to force the FLake model.”

I have to insist. The authors made some confusion between configuration parameters and forcing. The height of wind measurement (5 m) and the height of air temperature sensor (4 m) are in fact constant used to configure the model, but the measured meteorological parameters and model-derived irradiance were used to **force** the FLake model (and not to configure).

3.

page 7 line 22 and equation 2

I also insist about equation 2. The authors find the following relation between kd and SDD:

$$K_d = 1.64 \times SDD^{-0.76}$$

I have nothing against this result, but they should not say that it is of the type of eq 2...

4. Page 10, lines 17-18:

“Dark waters in these months contribute in reproducing colder LSWT for Avg and Merged simulations in 2005.”

I can not understand why water turbidity (or darkness) tends to decrease the lake surface water temperature. As the authors correctly stated along the manuscript, in dark waters the radiation is more absorbed in the surface layer, and so the LSWT tends to be higher by comparison to clear waters. What I proposed in my previous review is that:

The reason lies not in the fact that during those months the water is more 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).

The authors reply: “According to Table 2, the Kd value for same months of year 2005-2007 are in the same range. But the difference in calculating MBE for 2005 compared to 2006 - 2007 is taking months of Sep-Nov into the calculation of MBE for 2005. Therefore, the underestimation of LSWT in 2005 cannot be related to darker waters before, in spring and summer. It is more related to the months that are taken into the calculation of MBE.”

In my opinion the authors don't have reason and must examine and take into account the evolution of lake water bottom temperature (LWBT) and column mean temperature (MWCT). Alternatively they

have to provide a physically explanation about how “Dark waters in these months contribute in reproducing colder LSWT(...)”.

5.

Pag 13 line 11:

I still consider that the use of the term monotonic is not appropriate