

Interactive comment on “New profiling and mooring records help to assess variability of Lake Issyk-Kul and reveal unknown features of its thermohaline structure” by Peter O. Zavialov et al.

Anonymous Referee #2

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The paper presents the physical and biogeochemical data on Lake Issyk-Kul, measured during three field campaigns carried out in 2015, 2016 and 2017, which merits being published.

There are, however, some issues that should be clarified and some formal aspects which I would recommend modifying to improve the presentation.

On page 3, line 101 (P3L101) it is said that measurements were taken at 75 stations and Figure 1 is introduced. In Fig. 1, however, there are not 75 stations. We can guess that 75 does not refer to the number of stations, but rather the number of times that profiling measurements were taken during the three campaigns. Please clarify this in

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the text.

On P3L101 it is said that at each station CTD profiling was performed. However only physical data at the deepest location of the lake (and continuous measurements recorded at the Tyup and Jyrgalan estuaries) are presented. To prevent the reader having any false expectations, it would be better to advise them that the recorded CTD profiles will not be presented.

Authors emphasize the peak they found in the indirect measurements of salinity in 2016. The amplitude of this peak, less than 0.01 g/kg, is below the standard resolution of the salinity obtained by the well established UNESCO formula in most of the CTD profiles in the ocean. It is also said that such a peak does not affect the density profile (in Fig. 2b please plot the isopleths of constant density). Does this not mean that such a variation is within the noise level? It is true that the peak is a coherent structure (Fig 2) but it is highly correlated with an abrupt change in the temperature profile which is not observed in 2017 when the salinity peak is not observed either.

Authors use the formula provided by Peeter et al (2003) to obtain the salinity from CTD measurements for Issyk-Kul. Furthermore, they say that Peeter et al (2003) present a similar structure although they do not discuss it. Maybe this is because they are aware that these are below the resolution?

Even if such a peak can be correlated with some signal in the chemical measurements and could be explained by the supposed circulation in the lake, the reliability of this peak should be clearly states especially as the authors want to present it as the most important finding of their work i.e., this being the only idea included in the title of the manuscript. There are, however, other aspects which the data presented can support (i.e. bottom advection due to differential cooling).

P5L175-177: If there are no sources or processes that can justify a wide range of salinity values, why does a low variability of salinity all over the lake indicate intense mixing?

P5L180: Frontal zone or a small gradient and a frontal colour artefact?

P6 L 203: ...“the interannual differences observed in the lake surface temperature were more significant than those in salinity”. Please comment on your idea behind this, if any.

P6L203: “All over the lake, the surface water temperature in June of 2017 was warmer than that in June 2015”. Is there any reason for it not to be warmer or cooler or equal?

In Fig. 8 the air temperature is presented but the relevant variable is the total surface heat flux as the authors indicate in the text (P6L211-212). Please include the total heat flux in Fig. 8. This same comment applies to Figure 15.

In Figure 9, the vertical profiles of some chemical parameters are presented and compared to the values presented by Kadyrov 1986. Because this reference is in Russian and therefore the information may not be able to be easily accessed by many potentially interested readers, I recommend very briefly summarizing the data, which are presented by Kadyrov, in the text.

P6L228: It is said that a difference in the pH from 8.71 to 0.68 is not of importance. This means a reduction of 7% on $[H^+]$ if, due to methodology, the authors consider that this is below the measurement resolutions then please provide the pH values to a precision of one tenth.

P8 L285-290. Please move all methodological issues to the data sections. Explain better how the pigment index is computed.

P8L310: Bottom currents due to differential cooling is a nice hypothesis somewhat supported by the data presented which, here, is also related to the biological parameters. It would be good to emphasize such a relation later in the text.

P9L32: In the text, advective and convective cooling events refer to bottom and surface cooling signals. Surface cooling events do not have to necessarily be only convective. I suggest using the classification of surface cooling and bottom advection instead.

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Table 1. Bottom advection is a continuous event, although at a point there are velocity and temperature variabilities according to the variation of the differential cooling if this is its origin. Please explain why the durations of advective events are presented as some tens of minutes.

Figure 12. Please add the name of the variables and the units to the Table.

Figure 4. In accordance with the text (P3L183), change the units of the longitude from degrees to metres. This same comment applies to Figure 13 as well.

Figure 6. Is there any particular reason for presenting the differences in surface temperature and surface salinity between June 2017 and June 2015? The information is clearer if presented in absolute values, as it is done for 2016 (Figure 3).

Please, modify the abstract, the conclusion and the title as per the comments.

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