

## ***Interactive comment on “Evolution and dynamics of the vertical temperature profile in an oligotrophic lake” by Zvezdana B. Klaić et al.***

**Anonymous Referee #2**

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In this study, the physical limnology of a stratified oligotrophic lake was investigated by using observed high-frequency water temperature data monitored at 15 depths. The authors analysed thermocline and pycnocline deepening dynamics in accordance with changes in the atmospheric drivers. Further, the impact of three types of meteorological forcings on the lake were investigated: diurnal heat fluxes, noncontinuous strong winds and nonperiodic stronger winds. The authors report that surface waves did generate baroclinic oscillations in the metalimnion under realistic-topography conditions. The study gives interesting insights into the local thermal and fluid dynamics of the study site, which are essential for future modeling projects. Further, the results are interesting (and a helpful resource) for limnologists, meteorologists and civil engineers. The methods and results are well explained, although some paragraphs seems un-

C1

ecessary for the overall study.

General points:

- the last paragraph about the multiple linear regression of water temperatures at 0.2 m depth and meteorological drivers feels - in my opinion - unnecessary for the paper, as the results seem rather weak and were not used in the study at all. I would suggest just removing it and maybe including it in a follow-up work to make the current paper more coherent.

- I wanted to say that I really liked reading the thorough descriptions of the ‘thermocline and pycnocline’ and ‘spectral analysis’ paragraphs

- Do you feel the general data supports the detailed investigation of the thermal regime? Most loggers in the suggested thermocline region have a spatial distance of 3 m, which was then linearly interpolated to spacings of 0.25 m. Adding a section in which these methodological uncertainties are discussed could benefit the reader in my opinion (this uncertainty is only briefly mentioned at P15). Or were the linearly interpolated data only used for thermocline and pycnocline calculations?

- Just a suggestion, but I would keep units as e.g.  $\text{kg} \text{ (m}^{-3} \text{ m}^{-1})$  instead of  $\text{kg} \text{ m}^{-4}$  to clarify the gradient

- Dates should be Month Day not Day Month, e.g. P22 L 6

Additional points:

- P2, L20: ‘Some authors’ is a very vague statement. Although you go into more detail in the next sentences, you could rephrase it to e.g. ‘There are several studies reporting observed high-...’

- P2, L 23: Is ‘Authors’ the link to Thorpe et al.? Otherwise this is unclear.

- P4, ‘Lake temperatures’: Could you also please state the resolution of the thermistors.

C2

- P4, 'Meteorological data': Although the meteorological station is close (less than 1.6 km away, right?), did you check for uncertainties when using the meteorological data for the interpretation of the buoy data?
- P6, L16: Maybe I'm mistaken but as Welch's method is some kind of overlapping windowed Fourier transform method, is the sentence "Therefore, the Fourier transform computation is not applicable" a bit misleading?
- P7, L21: I would substitute 'naked eye' with a more meaningful term
- P9, L 14: Why were such high gradients expected for a Mediterranean lake? It's quite hard to see the gradients of -7 deg C/m in the contour plots. As this would correspond to a temp decrease of 1.75 deg C over 0.25 m, I guess such high gradients could only be observed in Mid August?
- P9, L28: Out of curiosity, what's the reason for including both thermocline and pycnocline in this study? You're stating that both were calculated from temperature data and due to the non-linear calculation of density from water temp in freshwater lakes, they do not coincide. Still, I think the implementations of differences between both boundaries aren't discussed in the manuscript. Would just stating/showing either thermocline or pycnocline also be enough for the purpose of this study?
- P10, L11-12: Personally I would delete the sentences "The results for N2 . . . . This result is expected .. of water density." as you are not showing these results and you're mostly stating the obvious for the buoyancy frequency.
- P10, L15: Does "4-16\*10<sup>-3</sup>" mean N2 was between 4\*10<sup>-3</sup> and 16\*10<sup>-3</sup> s<sup>-2</sup>?
- P11, L13: Is the occurrence of this cold water parcel a proof for the favorable vertical mixing conditions? And is this cold water parcel 'real' or a just an artifact from the averaging to a daily contour plot? Or, are most days during summer showing these daily dynamics, or is this just because some days in November have the phenomenon?
- P11, L19: Again, I would delete the sentence about the N2 daily course as it's not

C3

shown and has, as expected, the same pattern as the density gradient.

- P12, L9: Could you please add a vertical line for the 0.0417 h<sup>-1</sup> frequency in the plots 5b-e, that would help identifying the first mode more easily.
- P12, L14: The detailed inspections of the spectra at greater depths sound interesting. I think a reference here to the supplementary material is missing (in which the N2 plots could also be added if necessary)
- P13, L6-8: The whole sentence "Higher modes, ...of a mode (Figure 5a)" is unclear to me.
- P13, L8-13: Isn't this a very important paragraph for the whole study by stating that you found a significantly higher 3rd mode amplitude at 15 m depths with a period of 8 h? I would argue in showing this figure in the manuscript. Why is the PSD for 13 m depth in the manuscript, but not for depth 15 m? Also I would not use "namely" and "respectively" in the same sentence. Further, has Fig 6 b the right unit in the y axis, as the same figure in the supplementary information has 'K2 s'
- P15, L13: Could you please explain in more detail what 'realistic lake basin conditions' means and why these currents oscillate with the same period upslope and downslope?
- P16, L 8: You're stating winds as important drivers twice in this sentence
- P16, L22-32: This whole paragraphs feels like it could either be cut or that it should be in the introduction and not in the results/discussion paragraph
- P19, L4: Can the two-layer model assuming a rectangular basin be used for this inclined lake with the barrier separating two lake basins?
- P20, L8: I would suggest showing the Wedderburn equation in the manuscript
- P20, L10-L20: This paragraph is hard to understand when not simultaneously reading Horn et al. I like how it connects the discussion with the introduction, but could you please give more information regarding the findings of Horn (2001) and Boegman

C4

(2015) without having the reader refer to the specific figures.

- P23, L7: To avoid confusion, could you please exchange (2) here with 'occasional periodic forcing of stronger winds' as otherwise it could be confused with the other (2) which is 'produced occasional forced diurnal circulations'. The same is true for L10 and L 21 on P23

- P23, L13: I think the discussion of the unsuitable two-layer model should happen before the conclusions paragraph.

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