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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Evolution and dynamics of the vertical temperature profile in an oligotrophic lake" *by* Zvjezdana B. Klaić et al.

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First of all, we would like to thank Referee #2 for very useful comments. Our response is as follows.

Comment: 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.

Response: We removed the section 4.5 Multiple linear regression model for near-surface lake temperature from the revised manuscript.

Comment: - I wanted to say that I really liked reading the thorough descriptions of the 'thermocline and pyncocline' 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 pynocline calculations?

Response: Vertical temperature and density profiles suggest that the Nyquist wavelength is approximately 10–12 m, implying that the vertical profiles are adequately captured with our vertical sampling rates of 2-3 m. This further means that linear interpolation onto the 0.25 m vertical grid is acceptable and, anyhow, the interpolation is used only while calculating the thermocline and pycnocline depths.

Comment: Just a suggestion, but I would keep units as e.g. kg (m-3 m-1) instead of kg m-4 to clarify the gradient.

Response: kg m-4 is replaced by (kg m-3) m-1 throughout the entire text.

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

Response: According to the manuscript preparation guidelines https://www.hydrology-and-earth-system-sciences.net/for_authors/manuscript_preparation.html, the date should be given in format Day Month Year. Therefore, we have kept the present date system.

Comment: 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-...'

Response: The text is changed as suggested by the Referee.

Comment: P2, L 23: Is 'Authors' the link to Thorpe et al.? Otherwise this is unclear.

Response: Yes, thank you. We corrected the text. 'Authors' are replaced with 'The authors'.

Comment: P4, 'Lake temperatures': Could you also please state the resolution of the thermistors.

Response: Temporal resolution is described in the following sentences: "The sensors measure temperature every second, while the averaging interval of the stored data is specified by a user. In the present study we stored the 2-min means.", whereas the spatial distribution is specified in the following sentence: "Fifteen factory calibrated sensors were fastened to a string at fixed depths ranging from 0.2 to 43 m (specifically, at 0.2, 0.5, 1, 1.5, 3, 5, 7, 9, 11, 13, 15, 17, 20, 23, and 43 m). The string was attached to a buoy that was moored to ensure its fixed position in the deepest part (46 m) of the lake (φ = 44.8902°N, λ = 15.6038°E; Figure 1, right)."

Comment: '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?

Response: Please see our response to the first comment of Referee #1.

Comment: 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?

Response: Thank you for pointing to the vagueness of this sentence. Welch's method is an overlapping windowed Fourier transform method. What we meant here is that the straightforward Fourier transform method is inapplicable (Solomon Jr., 1991, in References of the present study). The revised text is corrected accordingly.

Comment: P7, L21: I would substitute 'naked eye' with a more meaningful term

Response: The "naked eye" is removed.

Comment: 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?

Response: Although the Kozjak Lake (535 m above the sea level) is geographically close to the Adriatic Sea (approximately 50 km distant), climate conditions in the lake area are not quite Mediterranean. Namely, the longest and one of the highest Croatian mountains (Velebit Mountain, 145 km long, 1757 m high), which stretches along the Adriatic coast, separates coastal areas (with Mediterranean climate) from the inland area (where the lake is located). In addition, the lake itself is in a mountainous region, between the mountains 1280, 1640 and 884 m high (e.g., Babinka, 2007, in References). Previous climatological studies of the greater lake area Makjanić (1958; 1971–1972), and the study of stable isotopes of oxygen and hydrogen in precipitation over Croatia (Hunjak, 2015), show that the lake area is at the border between the maritime and continental climate regions. Specifically, the lake area is at the border between two climatic zones as defined by the Köppen climate classification (e.g., Kottek et al., 2006): the temperate climate zone (C) and the snow climate zone (D).

And yes, it is difficult to see such high gradients in the contour plots showing entire lake depth. However, the high gradients are not so rare in the uppermost part of the lake from July to mid-September. For the information, in Figure 1 of this response we show gradients for the first three layers throughout the entire period.

Additional references:

Hunjak, T. (2015): Prostorna distribucija stabilnih izotopa kisika i vodika iz oborine na području Republike Hrvatske. Doktorska disertacija, Sveučilište u Zagrebu, Prirodoslovno-matematički fakultet, Zagreb, 82 pp.

Kottek, M., Grieser, J., Beck, C., Rudolf, B. and Rubel, F. (2006): World map of the Köppen-Geiger climate classification updated, Meteorol. Z., 15, 259–263, DOI:

10.1127/0941-2948/2006/0130.

Makjanić, B. (1958): Prilog klimatografiji područja Plitvičkih jezera, u Nacionalni park Plitvička jezera, Josip Šafar (urednik), Poljoprivredni nakladni zavod, Zagreb, 357–390.

Makjanić, B. (1971–1972): O klimi užeg područja Plitvičkih jezera, Geografski glasnik, 33–34, 5–24.

(Results of the above studies published in Croatian are summarized by Klaić et al. (2018)).

Comment: 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?

Response: Both temperature and density profiles may be encountered in the literature and by considering both of them we wanted to draw readers' attention to possible differences between the results – as, for example, thermocline and pycnocline depths – stemming from the two.

Comment: 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.

Response: These sentences are deleted.

Comment: P10, L15: Does "4-16*10-3" mean N2 was between 4*10-3 and 16*10-3 s-2?

Response: Yes, it does. The text is corrected accordingly.

Comment: 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?

Response: No, for the times between 5 and 7 LT (close to the sunrise), a large number of profiles exhibit such behavior. That is, at depths between 0.2 and 1.5 m the water is frequently somewhat colder than the water below. On the contrary, such pattern is not so frequent at other times of the day. As an illustration, Figure 2 of this response shows diurnal variation of absolute frequency of positive vertical temperature gradient within the layer between 1 and 1.5 m. Positive gradient means that within this layer the lake temperature increases with depth, thus producing conditions favorable for vertical mixing. As seen from the figure, for July, August and entire dataset such conditions were most frequent between 4 and 5 LT, for September they were most frequent for 3–4 and 5–6 LT, while for October they were most frequent for 3–6 LT. (November is not shown, since there where only a few November days with lake temperature measurements.) On the contrary, during the daytime, from 8 LT to approximately 18–20 LT (depending on particular month), absolute frequencies of positive vertical temperature gradient are substantially lower than those around the sunrise. For September and October, high absolute frequencies (comparable with the "sunrise" frequencies) are also found for nighttime hours.

Comment: P11, L19: Again, I would delete the sentence about the N2 daily course as it's not shown and has, as expected, the same pattern as the density gradient.

Response: The two sentences regarding N2 are deleted.

Comment: P12, L9: Could you please add a vertical line for the 0.0417 h-1 frequency in the plots 5b-e, that would help identifying the first mode more easily.

Response: Vertical lines in updated Fig.5b-f are added.

Comment: 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).

Response: The supplementary material with hourly spectra at individual depths for both the entire observational interval and the interval without strong winds is added (SUPPLEMENT 1). For the entire interval (left panels), 24 h period is prominent at greater depths (from 7 to 23 m), while the same is not found for the interval characterized by weak winds (1–20 September, right panels).

Comment: P13, L6-8: The whole sentence “Higher modes, ...of a mode (Figure 5a)” is unclear to me.

Response: Thank you for pointing to the error. The “modes” are replaced by the “harmonics”.

Comment: 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’

Response: Hourly results for 15 m are now shown in Figure 5b and the text is modified accordingly. The sentence with “namely” and “respectively” is now rewritten. The error in units is corrected in supplementary material (SUPPLEMENT 2). Correct units are as in Figure 6b, that is “K2min”. Also, in the current Figure 6 we replaced results for 13 m by results for 15 m.

Comment: 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?

Response: The text is now rewritten as follows: “These barotropic oscillations of the

lake surface produce oscillating (upwind-downwind) lake currents which have the same period as oscillations of the lake surface. In an idealized case (over a flat lake bottom) these currents would be horizontal. However, in the realistic lake basin (that is, the basin with the inclined bottom) the currents are forced to oscillate upslope and downslope.”

Comment: P16, L 8: You’re stating winds as important drivers twice in this sentence.

Response: The text is rewritten as follows:” Accordingly, they are generated by the same surface or atmospheric disturbances as surface seiches, such as earthquakes, variable winds, atmospheric pressure disturbances, tides, or heavy precipitation, with the winds being considered to be the most important driving agents.”

Comment: 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.

Response: Although in this paragraph the past studies of internal seiches are reviewed, we decided to keep it here since this Section is devoted solely to internal seiches. Thus, the reader can more easily (without scrolling through the text) compare our lake characteristics and results with those given by other authors.

Comment: P19, L4: Can the two-layer model assuming a rectangular basin be used for this inclined lake with the barrier separating two lake basins?

Response: Obviously not. This is now stated in both the paragraph following equation (9): “As seen from the figure, during the episode the calculated periods were between 6.07 and 6.24 h, which is considerably lower than the observed 8.0 h. Thus, we conclude that the idealized two-layer model is not suitable for estimation of the period of internal seiche in Kozjak Lake.” and in the Conclusions: “An idealized two-layer model (Eq. 9) suggests a period of internal seiche that is much smaller than the observed 8.0 h. Thus, a two-layer approach is not applicable for estimation of the period of internal seiche for a lake basin as complex as Kozjak (which includes submerged barrier

together with the two sub-basins of different depths and an islet, and therefore considerably departs from an idealized rectangular shape)."

Comment: P20, L8: I would suggest showing the Wedderburn equation in the manuscript.

Response: The formula for Wedderburn number (W) is now added, as W-1 = zmax / he, where zmax is the amplitude of the initial disturbance and he is the depth of the epilimnion (new Equation (10)).

Comment: 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 without having the reader refer to the specific figures.

Response: Figure 2 of Horn et al. (2001) (which is also published in Boegman et al. (2005a) as Fig. 1) is derived from laboratory experiments and field data. It defines separate regimes based on the values of inverse Wedderburn number and the ratio he/H, where H is the maximum lake depth. We find it quite difficult to describe several nonlinear curves that separate regimes one from the other, and, regrettably, we do not have permission to reproduce the figure. However, if not otherwise, Boegman et al. (2005a) is easily accessible via ResearchGate.

Comment: 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.

Response: The text is corrected as suggested by the Reviewer.

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

Response: Unsuitability of the two-layer model is additionally emphasized by the

following: "Thus, we conclude that the idealized two-layer model is not suitable for estimation of the period of internal seiche in Kozjak Lake." (Section 4.4)

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-559/hess-2019-559-AC3-supplement.pdf

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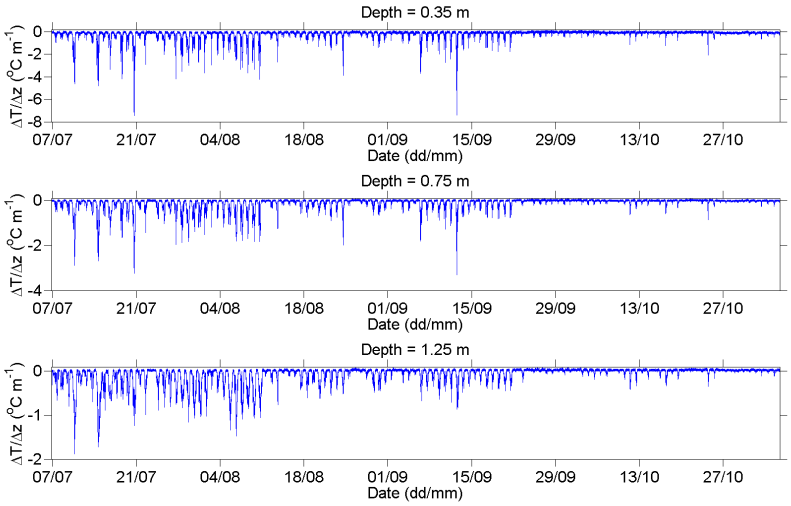

Fig. 1. Vertical gradients of water temperature for the first three layers (0.2–0.5, 0.5–1.0 and 1.0–1.5 m).

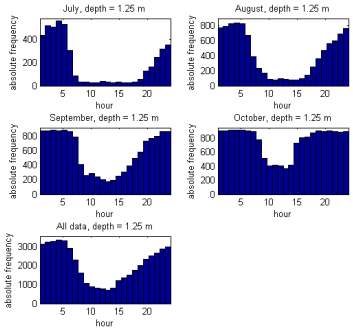

Fig. 2. Diurnal variation of absolute frequency of observations with positive vertical temperature gradient (conditions for vertical mixing of the lake water) within the layer between 1 and 1.5 m.