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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Variability in epilimnion depth estimations in lakes" *by* Harriet L. Wilson et al.

Anonymous Referee #2

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The study addresses one of the fundamental paradigms of limnology, the three-layer structure of the stratified water column. Here, the authors compare different algorithms to quantify the depth of the epilimnion, defined as a well-mixed, homogenous surface layer. As a general and agreed on mathematical definition of the distinction between epilimnion, metalimnion and hypolimnion is missing, different arbitrary thresholds for the epilimnion depth were investigated on two lake systems. As this paper aims to quantify the variability of epilimnion depth estimations and the methodological differences between alternative algorithms, it is of huge interest for a wide audience of limnologists, water managers, oceanographers, modelers, and environmental engineers. The study design, methods and results are well explained, although some paragraphs should be improved. Overall, the results of the study are important for future limnologi-

cal research and are challenging our current conceptual paradigm.

General points:

- Quantifying the data variability by computing the range has potential shortcomings, e.g. bias by outliers, no information regarding the distribution of data. Specifically, in this study the authors did state the maximum and minimum values enabling every reader to calculate the range themselves. The authors should think about computing and stating alternative metrics like the variance, standard deviation and/or the interquantile range.

- The manuscript stresses that the data used were sampled on a high frequency (1-2 min), but I do not see the advantage of using high-frequency data here in this study as diurnal epilimnion depth trends/oscillations were not discussed at all. At the end, as mostly seasonal averages, ranges or data point fluctuations were discussed, short-term dynamics were neglected. Would a study that uses for example bi-weekly sampled vertical profiles over 50 years give the same results?

- I'd advise the authors to discuss the challenge of multiple pycnoclines by microstratification more intensively in the manuscript. The occurrence of these profiles especially in Erken, which seems to behave more polymictic than Feeagh, is more interesting to real-world applications (and which method should be used then) than discussing shortcomings of the classical three-layer structure.

- Just to make future studies more concise, it would be better to investigate lakes whose monitoring programs did include vertical temperature, density and velocity profiles, e.g. estimating eddy diffusivities by ADCP. The addition of GOTM to this study to estimate turbulence seems a bit half-hearted as the calibration-validation was not described nor any figure showing the M5 results included in the manuscript.

Specific points: - L15-16: I'd recommend dropping the quotation marks around epilimnion and metalimnion in the abstract as it's a bit confusing to the reader. Further,

as the first lines discuss the three-layered structure, I'd recommend shortly mentioning the hypolimnion here.

- L23: If needed you could also exchange 'approaches' and 'methods' with 'algorithms' throughout the manuscript.

- L28: The phrase 'complex water column structures' is a bit confusing here. Are you referring to cases when the three-layer paradigm is violated? Maybe rephrase to complex thermal water column structure?

- L35: The phrase 'less problematic' is quite vague, do you mean 'introduces less bias'?

- L40: What do you mean by 'rapid gradients'? I'd recommend 'steep gradients'

- L48-50: I'd recommend stressing these important statements more throughout the discussion at the end

- L74-75: Seems like sentences L48-50 already explained that in reality there are no exact cut-offs, so how could there even be a consistent method used throughout limnology?

- L86: Is the vertical turbulence profile referring to a profile of turbulent eddy diffusivities? Here, the authors could also discuss field methods which measure turbulence in lakes, e.g. through velocity loggers. Or methods estimating diffusivities from water temperature profiles, e.g. gradient flux method by Heinz et al (1990) or heat budget method by Jassby and Powell (1975)

- L116: I'd recommend moving the sentence "The lakes differ in many characteristics, [. . .]" to the beginning of the paragraph

- 2.3 Simulated data: Tab. 1 suggests that the model was calibrated and validated. Which time periods were used? In this paragraph, the investigated fit criteria should also be mentioned. Also, in line 153, was the parameter of the minimum turbulent kinetic energy calibrated or, alternatively, which parameters affecting the min. turbulent

kinetic energy were calibrated? This sentence is rather unclear.

- 2.5 Analysis methods: This paragraph would benefit from sub-headings to make it easier for the reader to follow (similar structure as the Results would be beneficial)

- L208: Here the colors are mentioned but the figure is not referenced. If you do not want to cross-reference the figure yet, maybe just write about 'color-coding'.

- L232: 'logical and numerical schemes' is unclear to me. What do you mean by logical scheme here?

- L248: Is there a reason why the sensor deployment sensitivity was not tested on monitored data (e.g. by removing some loggers)?

- 3.2 Comparison between water density based methods: In my opinion, this paragraph is a crucial finding of the study as it discusses how deviations from the three-layer paradigm affected the results of the algorithms. Can you state how many profiles/observations points were either a) (classical paradigm), b) (multiple pycnoclines) or c) (weakly stratified)? Were most profiles following the three-layer structure or is the complex case b) dominating?

- L301: Here it would be beneficial for the reader to state the average epilimnion depth per lake plus the variance and the quantiles.

- L330: Shouldn't it be '[. . .] epilimnion depth was identified at a depth above [. . .]' instead of "greater"? As everything is referenced to the surface, wouldn't greater mean deeper?

- L332: It seems there's a word missing here

- L336: Do you mean 0.025 kg/m3/m instead of 0.25 kg/m3/m which would be higher than the maximum investigated threshold?

- L360: The phrase "[. . .] M2 had typically a higher threshold range than M3 [. . .]" confuses me. Do you mean that the range between the thresholds was higher?

- 3.4 Sensitivity of epilimnion depth: Can you test if the differences between the averages were significant?

- L405: The phrase ‘particularly distinct systematic difference’ seems a bit excessive

- L406: Is ‘[. . .] was equivalent to using different threshold values [. . .]’ referring to density threshold values?

- L424: Do you actually mean ‘acceleration of epilimnion deepening’ instead of shallowing here? Shouldn’t the epilimnion deepen relative to the surface height during stratification onset?

- L429: But these methods are detecting a layer that is specifically not isothermal relative to themselves, right?

- L448: The phrase ‘[. . .] not be suitable for use with water density metric [. . .]’ is unclear to me. Which water density metric are you talking about?

- L453-466: A figure showing these results would be beneficial for the reader, or how M5 compares to the other models.

- L465: Are profile data here referring to water temperature profile data?

- L515: I’d recommend not to write about ‘problematic’ here, maybe ‘less bias’, ‘conservative assumption’?

- Table 3: Could you exchange the table with either a correlation matrix or correlation plot?

- Fig. 2 is great, I like it a lot! It makes the whole study easier to understand.

- Fig 3.: Why is there a thin blue shaded area below the red shade in Lake Erken?

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