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Discussions

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

Anonymous Referee #1

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General comments

The article evaluates the epilimnion depth estimate in high-frequency data made by four different methods, including the effect of defining different thresholds that these methods require the user to choose. It also made this estimate using a hydrodynamic physical model. The aim of the study was to highlight the variability of the epilimnion depth estimates and how this variability could impact inferences about lake processes. This study draws attention to the need for researchers to unify their consensus on this topic, allowing comparisons between the results of different studies.

This is a very important study that addresses a hot topic in limnology and oceanography. With the increase in studies evaluating different bodies of water and the ease of obtaining increasingly sensitive measuring equipment, with the ability to perform and

store an increasing number of measurements, we have entered the era of Big Data. With the increasing availability of data, the need for models capable of extracting the correct information from them also increases. The correct adjustment of these models depends on studies of this type.

I believe that the result obtained and discussed in the article mixed the estimates made in clearly stratified water columns, with estimates made in weakly stratified water columns with estimates made in water columns with the presence of multiple stratifications. Therefore, the described variability is not only due to the distinction between methods and limits, but also to the application of the methods under different conditions, and this should be clearer.

Specific comments

P. 8 L. 281-283: I think that authors should make a clearer distinction between primary and secondary thermocline. For example, the method described by Read et al (2011) allows us to estimate the two thermoclines and, therefore, is more sensitive than the other methods, which do not consider this possibility. The comparison between the methods must consider the presence of these superficial micro-stratifications. For example, in the graphs b) of figure 4, there is clearly the presence of these micro-stratifications that are hampering some methods of identifying the main thermocline. In other words, it is not fair to fit a model that expects only one stratification with profiles that show various stratifications. It is obvious that the estimate will not be satisfactory. It is extremely necessary to make a pre-filter, removing the superficial layers of values before the estimate is made. This method was applied by Pujoni et al. (2019).

P. 8 L. 288-289: In this same line, we must discuss and define a threshold of what we call "homogeneous water column". I don't think it makes sense to compare the methods using profiles with low stability of the water column. If we no longer have a clear stratification, the methods should not be applied, as they will look for a thermocline that does not exist. I may be wrong, but the water column in the graphs c) in figure 4 is homogeneous and should not be subjected to comparison with these models.

P. 9 L. 350: Why did the authors use the range to estimate variability? The range is sensitive to outliers. Why not use standard deviation, which is a more robust estimate of variation?

P. 10 L. 379-382: I would suggest showing some graphs of density profiles with the estimated depths of the methods so that it would be easy to see why there were such differences and whether one method made a "better" estimate than the other.

I would suggest that the authors discuss a little about the visual assessment of profiles. Should we rely on this visual assessment to try to "correct" the biased estimates made by the models?

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