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Interactive comment on “The influence of temperature and salinity variability on the upper ocean density and mixed layer” by R. W. Helber et al.

Anonymous Referee #2

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The paper by Helber et al. presents a global analysis of the difference between SLD and MLD. I have several concerns about the content of the work and its presentation.

The title is somehow misleading. I can understand an interest in presenting the SLD global variability and the difference between SLD and MLD. But the title is much more general. In fact, considering the title, I do not see any general interest and novelty in the paper if the interesting point reduces to discussing the differences between MLD and ILD, coming very late in the paper. Moreover, there is a relevant literature regarding the effect of salinity and temperature on the upper layer stratification and, specifically, on the presence of a MLD shallower/deeper than the ILD. This literature, that often focus

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on the existence and characteristics of so-called barrier layers, has to be discussed in details and the actual novelty of this paper have to be explicated. Finally, if this is the case, why discussing the SLD at all?

Specific comments:

The change in curvature of the temperature at the base of the ILD can be of positive or negative sign. The sign is relevant for an understanding of the dynamics. Can you discuss this point? How this relates to the results obtained with other methods (e.g., Mignot et al., OS, 2009)?

The discussion on the role of salinity and temperature in determining the SLD is too qualitative. A more detailed discussion of the state equation is needed (and Fig. 1 is unuseful as it is, considering the limited range of T values). E.g., from the state equations do we have to expect larger differences between MLD and SLD in the tropics or in polar regions ?

The final selection of the profiles is based on a series of criteria. One of them is that the depth of the deepest measure has to be at least 200m, a rather shallow depth. What happens if the algorithm fails to find a MLD or SLD? Do you set the value at 200m?

The use of fixed depths for computing Turner Angles is also highly questionable. The deep reference depth can be well below the MLD in the tropics or much shallower than the MLD in some regions (e.g., regions of formations of mode and deep waters in both Hemispheres). Which is the impact of this choice on the results? Why not using other approaches, e.g., by characterising the turner angle at the base of the MLD, whatever is its value?

The algorithm for the definition of the SLD has to be explained better and its robustness has to be showed. The examples in Fig. 5 are actually rather puzzling.

Instead of using the ILD for discussing the influence of temperature, can it be discussed by comparing to a SLD defined using only the temperature in the computation of the

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sound speed? Much like as it is done for identifying barrier layers.

In summary, on the base of the text presented, I do not see a specific interest for the analysis of the SLD to understand the difference ILD-MLD or the Turner Angle distribution, already discussed in literature. Assuming that there is an interest for the SLD for other fields of study (e.g., animal behaviour?), I would suggest to make a much shorter note on the SLD global distribution per se and the factors that determine it, taking into account the existence of the Helber et al. (2008) paper. Only secondarily it can be showed and discussed how it compares with the MLD, barrier layers etc. I also suggest to make the SLD climatology available on the web, to enhance its use in other studies. The alternative it is to eliminate any reference to the SLD and focus on characterising the transitional layer below the MLD (e.g., Johnston and Rudnick, JPO, 2009).

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