Review of the manuscript: https://doi.org/10.5194/egusphere-2024-4079

Title: The global ocean mixed layer depth derived from an energy approach

Reviewer #1 Brandon Reichl

Summary

The revisions satisfied many of my comments from the previous version. I have a few remaining comments, I would prefer they are addressed before recommending the manuscript for publication.

We thank the reviewer for taking the time to evaluate our revised manuscript; we are pleased that you found our response to be adequate. In this revised version, we have carefully considered all additional points raised in this round. We hope that you will find the revised manuscript complete and suitable for publication.

We prepared a marked-up manuscript version that shows the changes we made, as well as a version without tracked changes for easy visualization. The references to pages and lines in the comments below correspond to the manuscript with tracked changes.

General comments

Comment 1: More precise language could be used throughout the text where WB is referred to only as "energy", since there can be different energy approaches and considerations (e.g., potential energy anomaly). E.g., a more appropriate title could be "The global ocean mixed layer depth derived from buoyancy work".

Answer 1: We thank the reviewer for pointing out this observation. We agree with you; the title of the manuscript now reads as follows: "*The global ocean mixed layer depth derived from an energy approach based on buoyancy work*." Additionally, in the main text of the manuscript, we emphasize that our MLD methodology is based on a specific energy approach, namely, the work done by buoyancy (*lines 193-195*).

Comment 2: I don't have a general issue stating that the details of the connection to boundary layer turbulence are beyond the scope of this work. However, that language is not always consistently reflected by the text. E.g., in the abstract it says this approach "aligns with turbulent boundary layer dynamics" yet later the claim is only that it is plausible to establish a relationship with the buoyancy flux, and hence to connect with the turbulent kinetic energy budget of the upper ocean. Since it is said the link between WB and turbulence boundary layer dynamics will be explored in future work I think it is more appropriate to leave it at that and to not overemphasize a connection that is only claimed as plausible in the text.

Answer 2: We agree that the wording can be occasionally confusing and inconsistent. We rewrote the sections related to this connection, both in the Abstract (*lines 15-18*) and the main text (*lines 121-133, 211-215, and 475-477*). Now, the wording is clear regarding the connection between WB and the turbulent approach to mixed layer formation, but it does not overemphasize a relationship that was not completely demonstrated.

Comment 3: I remain skeptical that this work establishes a precise, global and seasonally applicable range for WB. As I've mentioned throughout the review process, this is why we were reluctant to propose a global value of PE anomaly in Reichl et al. (2022). But ultimately a range of 12.5-20 J/m3 for WB is given in this work based on most of the ocean which has been profiled by Argo. This is ultimately not that different from the range of 10-25 J/m2 given as a plausible range in Reichl et al. 2022 (one I do not necessarily emphasize, I did not intend our work to come up with a PE anomaly value that reproduced Holte and Talley). So, (A) I personally don't think there is a significant difference between a range of 10-25 and 12.5-20 (yes it is a factor of 2, but neither range was formally derived). (B) Regardless of point A, the region where the values seem to differ the most from this mean value are the deep water formation regions. This is also where the quality index is lowest. This is one of the most important regions globally for interior water mass formation. I would have been interested for this study to comment more on the values of WB in those regions and its implications in the pursuit of a globally universal value. This might also emphasize regions where WB could provide additional insight beyond the delta threshold established in Section 3.1.

Answer 3: We conducted a detailed analysis of the precision of the MLD methodologies (see subsection 3.2. *MLD methodologies intercomparison*) and found that EBM is precise compared to other MLD methodologies. As described in subsections 3.1, EBM provides a realistic description of the MLD, consistent with the seasonal variation in ocean conditions across the ocean.

The WB values that determine the MLD are those shown in Fig. 4, not the 12.5-20 J m^{-3} interval. The aim of subsection 3.3 is to very preliminarily explore whether a single WB value can determine the MLD on a global scale throughout the year; for this reason, we have analyzed only three meridional transects along three oceans. The analysis in subsection 3.3 was not intended to find a definitive WB range that determines the MLD across the global ocean throughout the year. The results of this subsection aim to motivate future research. We apologize for the lack of clarity in this regard; the title of subsection 3.3 was revised accordingly (*line 446*).

The sentence referring to the range of 10-25 J m^{-2} in the PE anomaly found by Reichl et al. (2022) was removed from the Discussion section in lines 554-558. The sentence was rewritten to avoid confusion and placed in the paragraph where that study is discussed (*lines 499-502*).

As you suggested, we extended the analysis of the WB to provide more detailed comments on the values of WB in high-latitude and deep-water formation regions (*lines 300-311*), as well as the implications for pursuing a globally universal value (*lines 456-460*). In the Discussion section (*lines 563-576*), we had already described the limitations and scope of using the quality index in evaluating the MLD determination. This paragraph in the Discussion section outlines a roadmap to significantly enhance EBM by establishing a criterion to unequivocally determine the WB threshold that characterizes a well-mixed layer.

Specific comments

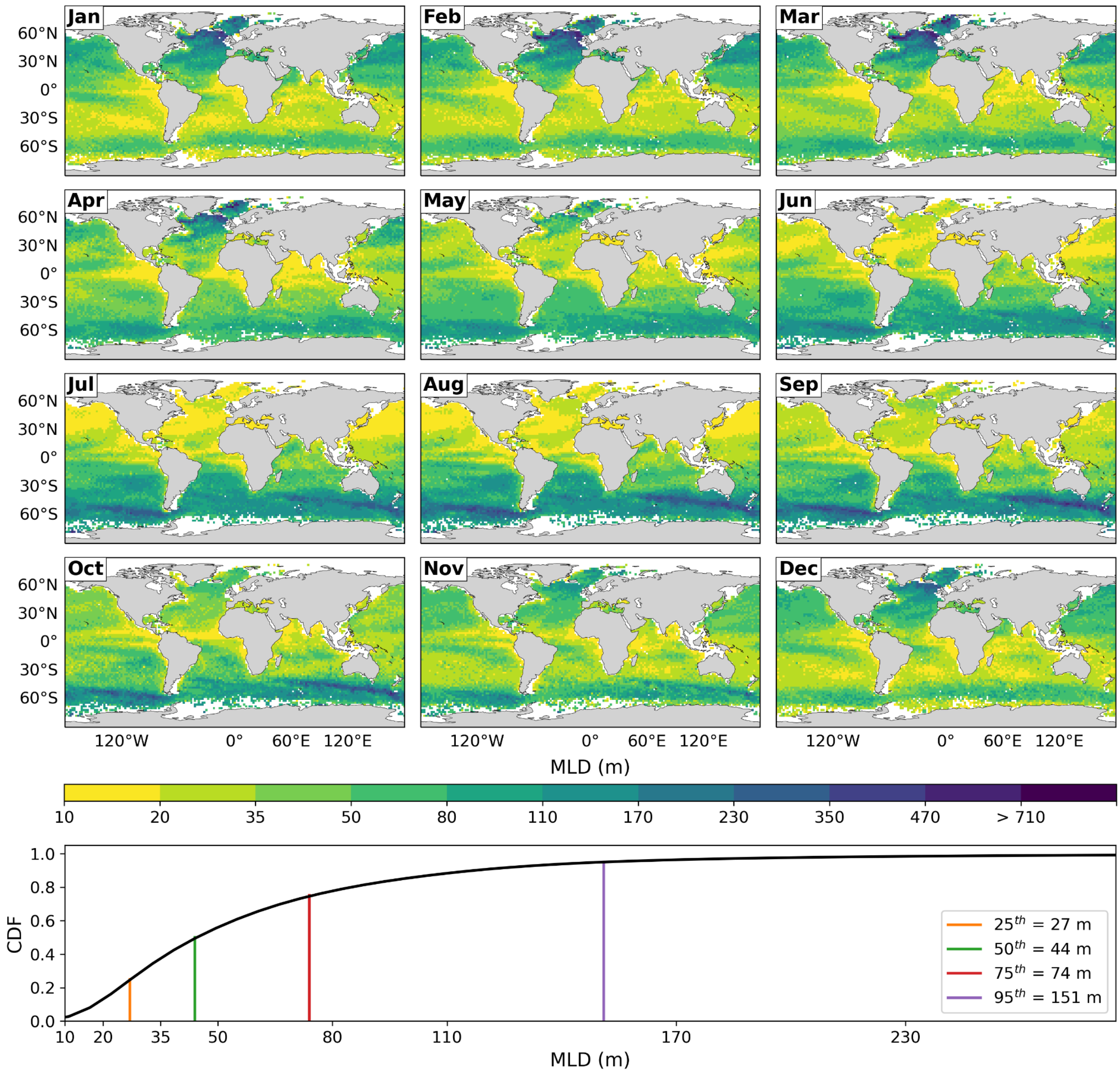
Comment 4: L47: I suggest to replace gravitational potential energy with potential energy anomaly..

Answer 4: Done (*line 53*).

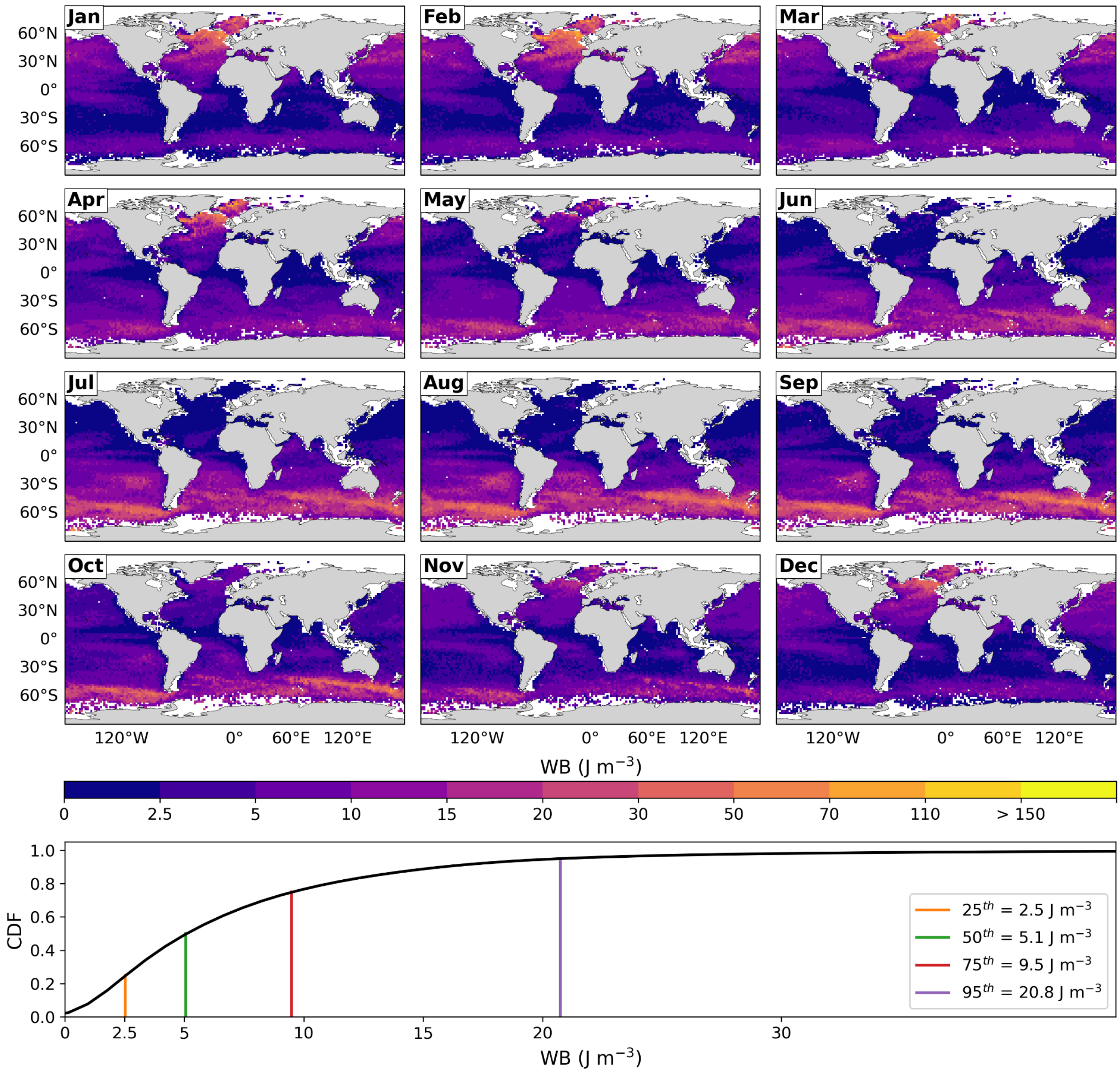
Comment 5: Figure 3: Winter MLDs in convection regions approach 500-1000m by most MLD metrics (including WB as commented in the text). I suggest revisiting the colorbar to not saturate out the deepest values, a nonlinear/logarithmic increment may help. Similar for WB in Figure 4 and the saturated regions. These regions are geographically small, but have significant roles in interior ventilation.

Answer 5: Thank you for the pertinent suggestion. We agree with you that trying to show the spatiotemporal variability of the MLD (and that of WB) in a single map is very complex. There is great variation in the MLD and WB values across space and time that a single colorbar is not capable of describing in a proper way. To clearly appreciate and distinguish the characteristics of the MLD and WB, it is better to analyze specific regions (tropics, subtropics, or polar zones) during a specific season (summer or winter). A global map is a tool useful for a holistic view of the variables of interest.

For the above reasons, we provide the netCDF file of the MLD and associated variables (https://doi.org/10.17882/106181), allowing interested readers to plot their own maps for specific analyses. However, as you suggested, to emphasize the strong convective and deep-water formation regions, we present here maps of the MLD and the WB threshold, using nonlinear colorbars.



Jan
Feb
Mar
Apr
May
Jun
Jul
Aug
Sep
Oct
Nov
Dec
60°N
30°N
0°
30°S
60°S
120°W
0°
60°E
120°E
MLD (m)
10
20
35
50
80
110
170
230
350
470
> 710
CDF
1.0
0.8
0.6
0.4
0.2
0.0
25th = 27 m
50th = 44 m
75th = 74 m
95th = 151 m
10
20
35
50
80
110
170
230
MLD (m)



As is known, nonlinear (e.g., logarithmic) scales are non-intuitive, and their interpretation is difficult. We consider that, despite the limitations, the use of colorbars with linear scales is adequate to describe the spatiotemporal variability of the different variables throughout the manuscript. As mentioned, interested readers can plot their own maps using the provided database. Moreover, we are currently working on implementing a web-based data portal to display the EBM-MLD and its associated variables. We will include regional maps and linear and nonlinear colorbars to facilitate the analysis. This data portal will be released in due course; its URL will be made available in the source code repository (https://doi.org/10.5281/zenodo.14531829).

Comment 6: L112: "buoyancy losses required to mix" implies causation, but I think this is more of a statement of consistency and not causation? I suggest "buoyancy deficit compared to that of a homogeneous column in potential density". There seems to be some approximation here in the details, by assuming the potential density of the mixed column is the mean potential density of the unmixed column.

Answer 6: Thanks for the comment. You are right in that the definition of columnar buoyancy implies causation because the buoyancy loss must be provided to an initially stratified water column. However, we want to adhere to the original definition of columnar buoyancy provided by Lascaratos and Nitis (1998) and Herrmann et al. (2008). We consider that the details and approximations concerning columnar buoyancy and WB are adequately described in the manuscript. Please, see *lines 100-107*.

Comment 7: L123: I suggest clarifying it is “potential” density here.

Answer 7: Done (*line 136*).

Comment 8: L133: A connection to turbulence and boundary layers to me implies a connection to the mixing layer (by definition). Here WB is explicitly disconnected from the timescales of active mixing. Maybe I’m being too picky, but there seems to be some contradiction here (see also general comment “2” above).

Answer 8: Please, see the answer to Comment 2.

Comment 9: L169: WB_ref defined? I think I understand it, but I suggest writing the equation here to make it clear for the reader.

Answer 9: Done (*lines 182-183*).

Comment 10: S3.3: This ignores the deep convection regions, which happen to also be the regions where the quality index figure shows the worst performance (see also general comment “3” above).

Answer 10: Please, see the answer to Comment 3.