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

We appreciate all your comments. After a careful revision that incorporated your comments and those of the other two reviewers, this version has greatly improved. We trust that you will find the revisions substantial and consider our study a valuable contribution to mixed-layer depth research. Below is a description of the major modifications made to the manuscript:

- The methodology (subsection 2.1) was extended by including details concerning the connection of the work done by buoyancy (WB) with the turbulent kinetic energy budget.
- We revised our definition of the mixed layer and how to calculate its depth (subsection 2.2. Defining the mixed layer). The mixed layer depth (MLD) was not calculated with a unique WB equipotential.
- Since we adjusted the MLD calculation, we rewrote the Results and Discussion sections entirely. The Conclusions and Abstract were also rewritten accordingly.
- The Supplement was rewritten to provide a detailed comparison of our MLD methodology with the common ones; we included global monthly climatologies of the MLD and the WB at the MLD.
- The dataset containing the monthly climatology of mixed layer depth and the derived variables calculated in this study is now publicly available at SEANOE (https://doi.org/10.17882/106181).

Responses to specific comments are shown below.

<u>Summary</u>

Comment 1: The MLD is not an objective quantity, it requires defining timescales and spatial scales since the ocean surface boundary layer is under consistent and variable external forcing. Furthermore, identifying a mixed layer invokes some qualitative analysis to quantify what is meant by "mixed". This ambiguity makes the present goal of deriving a "global ocean" mixed layer depth challenging, and I acknowledge the efforts the authors have devoted to this matter. I find the proposed WB method for estimating MLD is potentially interesting, but the paper presently relies heavily on ad-hoc and empirical arguments. This limitation makes it difficult to judge the value of the WB approach versus other approaches. The study would therefore benefit significantly if it can identify quantifiable metrics to bolster its claims.

Answer 1: We agree with you on the challenges in defining the MLD. In this version, we clarify and better describe the aspects of the mixed layer addressed in our study in *subsection 2.2 Defining the mixed layer (page 5, lines 124-129)*.

We really appreciate that you find our approach to estimating the MLD interesting. We acknowledge that in the prior version of the manuscript, we derived our results based on empirical and non-robust arguments. In *subsection 2.2*, we proposed a quantitative procedure to calculate the MLD to fix that (*page 5, lines 129-133 and pages 7-8, lines 156-184)*.

Due to the adjusted definition of the mixed layer, the new results are completely different from those of the prior version of the manuscript. We conducted a detailed analysis (qualitative and quantitative) of the resulting MLD climatology and the WB threshold that defines it. We also compared our methodology with others on a global scale, considering different metrics. We consider that we greatly improved the significance and interpretation of our results (*Results and Discussion sections).*

Comment 2: The desire for a MLD metric led to proposing PE anomaly in the Reichl et al. (2022) analysis of MLDs, as it quantifies the energetic distance of a column of sea water from being well mixed. This naturally leads to the PE anomaly as a possible basis for identifying the MLD, but it is not obvious that the same energetic distance should define the MLD in all regions or on all timescales. WB itself may provide another interesting metric for quantifying mixed layers, but it differs in important ways from the PE anomaly. WB provides an integrated measure of stratification of the column by considering the displacement of a particle from the mixed layer base. This yields some insight into the stratification, but less information than if you evaluated the buoyancy work associated with displacement of all particles within the mixed layer. It is also not obvious that a single WB quantity should define the MLD at all locations and on all timescales.

Answer 2: You are right; WB differs from the PE anomaly in that they describe different physical variables. However, we consider that WB is physically suited to calculate the MLD (*subsections 2.1 and 2.2*). Via the WB profile WB(z), we actually provide the work done by buoyancy to vertically displace any water parcel within the mixed layer to the top of the mixed layer. We used the WB value at the MLD as the energy threshold characterizing the mixed layer.

We agree that it is not obvious that a unique WB equipotential should determine the MLD globally year-round, which was obtained from Eq. 8 and represented in Fig. 2; please refer to the detailed analysis of the WB metric in *subsections 2.1 and 2.2*. Results of the WB threshold characterizing the MLD are shown in *Figs. 4 and 11*.

Major concerns

Comment 3: The WB method for estimating MLD has merit over a density threshold method since it invokes a dynamical quantity. However, I am unsure that this dynamical quantity truly addresses two main shortcomings of the threshold method. It would help if this paper better identified what it does and doesn't improve upon other methods.

- WB does not offer any physical guidance for choosing a threshold value, so the threshold remains ad-hoc. The paper argues that 20 J/m3 is a universally applicable value, but this was derived empirically based on arguments about its vertical gradient over limited regions of the ocean. There is no physical significance offered for the integrated buoyancy work of 20 J/m3, which would justify it from first principles for identifying the upper ocean mixed layer in all seasons and locations.
- The new method is still sensitive to the details of the choice of the threshold value and the reference depth. Interestingly though, some of this sensitivity is reduced since WB is an integrated quantity and therefore less responsive to noise in a profile.

Answer 3: Considering this and the previous comment, we conducted a detailed analysis of the WB metric and its physical significance (*subsections 2.1 and 2.2*). We also revised our definition of the mixed layer and proposed a quantitative procedure for calculating its depth, including details concerning the density inhomogeneity along the mixed layer, the WB threshold, and the reference depth (*subsection 2.2*). The *Discussion section* extends the analysis of our MLD methodology and its downsides.

Comment 4: I did not agree with the claim in the text: “There is a correspondence between our methodology and that of Reichl et al. (2022), suggesting that our energy-based methodology is consistent with the turbulence approach of the mixed layer formation”. I do not find the correspondence between the WB and PE anomaly to be obvious, other than both using energy based criteria. PE anomaly is a column integrated energy value that quantifies the distance of the column from being perfectly homogenous, hence it has units of J/m2. WB only considers the energetics of a single parcel advected through the column, hence it has units of J/m3. One could construct different idealized profiles that give the same WB MLD yet have different PE anomalies, which was identified as a shortcoming of other MLD methods in Reichl et al. 2022. Perhaps the WB method has less PE anomaly sensitivity than other methods, but an evaluation of WB in terms of PE anomaly was not attempted in this paper. These important differences from the PE anomaly do not support the statement that this method connects to boundary layer turbulence in the same way as argued for PE anomaly.

Answer 4: We are very sorry for this inaccuracy. Now, in *subsection 2.1 An energy measure of the vertical homogeneity of the water column*, we show an alternative expression of WB that allows us to appreciate its connection with the turbulent kinetic energy budget and, thus, with the physics of boundary layer turbulence. We did not provide further analysis of such a connection because that is beyond the scope of this paper and is proposed for future research (*pages 4-5, lines 93-113)*.

Via the WB profile WB(*z*), we actually provide the work done by buoyancy to vertically displace any water parcel within the mixed layer to the top of the mixed layer. We used the WB value at the MLD as the energy threshold characterizing the mixed layer.

Comment 5: The article claims the WB MLD with an energy value of 20 J/m3 is “accurate, robust, and of global applicability”. This claim is based on looking at “challenging regions”, which are defined where a suite of existing methods disagree with each other in MLD magnitude. One issue is that this approach appears to weight the analysis to deep MLD regions (usually convective regions), but this appears to deemphasize shallow MLD regions (e.g., the Arctic Ocean (more on this below) and summer time mixed layers in biologically productive regions) that are just as valuable. Furthermore, I did not find a convincing and quantitative argument in section 3.2.2 for why the WB method estimates a better and/or more consistent MLD than other methods in these regions; it mostly relies on ad-hoc arguments.

Answer 5: We agree with you that local results can not support global conclusions. To circumvent this, we compared (qualitatively and quantitatively) our methodology with others on a global scale, considering different metrics (*subsection 3.2. MLD methodologies intercomparison*). We removed the limitation of focusing only on “challenging regions”. We found that there is a lack of consistency among the methodologies in determining the MLD, but also that all the methodologies perform well under the oceanographic conditions for which they were built, according to the parameter being addressed. Since it is almost impossible to ensure that the true value of the MLD in any location and time is known, the accuracy of any methodology can not be determined, that is, the closeness of any MLD estimation to the true value. When comparing MLD methodologies, we can only evaluate their precision: the closeness between the different MLD estimates. Thus, we evaluated the precision of our methodology on a global scale and removed ad-hoc arguments from the description of our results.

Comment 6: I found the analysis of the climatology lacked a significant new/novel result that advances beyond previous work on mixed layer climatologies. It would be useful if issues in previous climatologies could be identified and to discuss why the WB method was able to provide a more meaningful estimate of a MLD climatology (or for what contexts it may be more meaningful).

Answer 6: Our study's significance relies on the energy definition of the mixed layer, which represents an important advancement in this topic (*subsections 2.1 and 2.2*). In the revised version of the manuscript, we carried out an intercomparison of MLD methodologies (*subsection 3.2. MLD methodologies intercomparison*) that showed to what extent our MLD estimation could be more meaningful than others; such an analysis was further extended in the *Discussion section*.

Comment 7: The article states that it offers globally applicable mixed layer depths and provides a mixed layer depth climatology. Constructing a gridded mixed layer depth climatology requires many decisions, and little technical detail on how this is done was provided. Nor is the resulting climatology data made available, which would severely limit any potential impacts of this work. The climatology is produced only using Argo data, which neglects a lot of additional oceanic profile data that exists. This also leads to coastal oceans and the entire Arctic Ocean being absent from this work.

Answer 7: In the revised version of the manuscript, we rewrote how we constructed the MLD climatology, the data we used, and its limitations in spatial coverage (*subsections 2.3 and 2.4, pages 8-9, lines 185-214*), hoping to be clearer. We acknowledge the limitations in the spatial coverage of Argo data; this study could not explore the MLD in coastal zones, and the robustness of the findings in the subpolar oceans may be limited. For future research, we propose incorporating additional observational datasets covering the regions not extensively mapped by Argo to expand the scope and robustness of this study.

The dataset containing the monthly climatology of mixed layer depth and the derived variables calculated in this study is now publicly available at SEANOE (https://doi.org/10.17882/106181). This information was added to the *Data availability section*.

Other Comments

Comment 8: What reference pressure is used to define the potential density in equation 3? Can a fixed reference pressure be used as a suitable approximation?

Answer 8: We are sorry for not providing that information. According to Stewart (2008), the potential density referred to as 0 bar is adequate for vertical displacements not exceeding a few thousand meters (*Page 3, lines 77-79*).

Comment 9: The WB method may offer some physical insights into when and why different MLD methods differ, and it is closely related to the threshold method such that it may better ground the threshold values used in present studies. One can rewrite the WB criteria (equation 3, by rewriting the 2nd terms RHS w/ H defined as the MLD and _Hmean defined as the depth integral mean of potential density over the MLD): WB = g*H*[_Hmean - rho_ref], which is now expressed similarly to the threshold method: delta = [rho(z) - rho_ref]. This makes two notable differences of WB from the threshold method more apparent: (1) the density difference is now from the average of the density over the column instead of at the MLD, and (2) the density difference is effectively weighted by the

MLD (and gravity). These have some interesting implications: (1) integrating the potential density makes the MLD less sensitive to noise in the profile, and (2) weighting the threshold by the MLD reduces the depth of deeper MLDs with the same energy value. The first effect seems advantageous for practical use. The second effect makes sense from the perspective that it takes more energy to raise a parcel from a deeper depth, but since deeper MLDs often experience more turbulence and mixing, it is not straightforward to me that the second effect is obviously "better" for MLD identification. The ocean does not experience uniform levels of energy inputs to turbulence. (This is a main reason I am not convinced there should be a single universal energy value for either the WB method or the PE anomaly method.) Furthermore, the 2nd point above about MLD weighting also offers an interpretation of why shallower depths are associated with larger density differences, e.g. Figure 7.

Answer 9: We agree with you that our methodology is a threshold method. The procedure to determine the MLD in terms of WB is based on a threshold of the density inhomogeneity along the mixed layer (*subsection 2.2. Defining the mixed layer*). By considering the vertical integration of density, our methodology could represent an improved or well-founded version of the threshold density criterion proposed by de Boyer Montégut *et al*. (2004) to define the MLD. One of our results showed that MLD methodologies based on density thresholds of about 0.125 kgm-3 along the mixed layer produce overestimated MLDs and are inadequate to define a well-mixed layer in energetic terms. Our methodology serves to better support the threshold values of existing MLD methodologies. However, its main feature is that it represents a unique and complete methodology to estimate the MLD.

We rewrote WB as you suggested and conducted a detailed analysis of WB from the resulting expression (*Eq. 8* in the new version of the manuscript). The features you mentioned were included and properly described (*pages 5-7, lines 135-155*). The second effect you mentioned reflects the relationship between the density inhomogeneity and the associated WB along the vertical, but does not negatively affect the MLD estimation. In fact, this relationship (*Eq. 8*) was used to propose the procedure to calculate the MLD.

We agree with you that a unique WB equipotential should not determine the MLD globally year-round. A detailed analysis of the WB metric (*subsection 2.2 Defining the mixed layer*) and our new results (*Figs. 4 and 11*) confirmed your observation. Turbulence and its associated energy levels are spatially and temporally variable on a global scale; thus, the water column's stratification and vertical homogenization are not spatially uniform through the seasons.

Comment 10: L336: "It is also easy to implement numerically." Easy compared to what?

Answer 10: We are sorry for not being clear in that sentence. The numerical implementation of our MLD methodology only requires the potential density profile referred to 0 dbar, which is easily obtained from simple survey ocean data or numerical data. The script to compute the MLD is very short, and its formulae are not complex. In that regard, our methodology is easy to implement numerically. We rewrote this paragraph, hoping to be clearer (*page 25, lines 435-437)*.