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

Comment 1: This is my second time reviewing this manuscript, which proposes an alternative definition of the ocean surface mixed layer depth (MLD) based on a buoyancy work diagnostic. I found the revision partially satisfied my previous concerns, but presented several new concerns detailed below.

Answer 1: We thank the reviewer for taking the time to evaluate our revised manuscript. In this revision, we strived to thoroughly address your questions and carefully consider all additional points raised in this round. We hope that you will find the revisions complete and consider our study a valuable contribution to the research on the mixed layer depth.

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 without tracked changes.

Comment 2: As stated from the previous version, I find the subject matter interesting, and the alternative buoyancy work "WB" metric MLD definition provides a useful contextualization of differences between traditional simple threshold MLD methods and methods like potential energy (PE) anomaly. However, I think the presentation still needs to better characterize differences between threshold methods, WB, and PE anomaly. I found the paper addresses how WB differs from threshold methods (Section 2.2 is a nice addition in that regard). But it doesn't really clarify how the WB metric differs from PE anomaly (and how they offer different perspectives).

Answer 2: We agree with you that we need to better contextualize our methodology, EBM, in relation to the PE anomaly methodology. We added two paragraphs (in section *2. Methodology and data* and section *4. Discussion*) to address that. *Page 8, lines 190-198 and page 26, lines 464-475*.

"*The MLD definition differs between the potential energy (PE) anomaly methodology proposed by Reichl et al. (2022) and the methodology presented here. The PE anomaly diagnoses the MLD as the depth to which a given energy could homogenize a layer of seawater; the PE anomaly relates to the turbulent kinetic energy budget of the ocean surface boundary layer and serves as a good proxy for mixing, resulting in MLD estimates that are representative of active boundary layer turbulence. On the other hand, EBM determines the MLD by quantifying the vertical homogeneity of the water column in terms of the required WB to displace a water parcel vertically. WB may be associated with the turbulent kinetic energy budget of the upper ocean layer since it relates to the buoyancy flux required to mix that column during a specific time period; however, we did not conduct an in-depth analysis of the connection between WB and the turbulence approach to the mixed layer formation, which we propose for future research.*"

"*Reichl et al. (2022) introduced a framework for defining the MLD based on the PE anomaly, suggesting a threshold range of 10-25 J m^{-2}. While this range offers a valuable benchmark, determining the optimal thresholds under all seasonal and regional conditions remains an open question. Future research could* *determine optimal values of the PE anomaly for global application and explore this approach in regional contexts by assessing the sensitivity of the MLD estimates to different thresholds of the PE anomaly. By comparison, with EBM, we were able to find the energy values (in terms of WB) that define the MLD globally during all seasons. Moreover, EBM provides additional information not supplied by the existing MLD methodologies; WB represents the potential energy barrier to the vertical displacement of water parcels, which could complement the analyses of oceanic processes occurring in intermediate vertical sections, commonly associated with interchanges of properties along the water column, such as the flux of particulate organic matter from the surface to sediments (Kirillin et al., 2012; Omand et al., 2020), the vertical content of chlorophyll (Carvalho et al., 2017; Briseño Avena et al., 2020), and entrainment in barrier layers (Katsura et al., 2022). The application of WB to analyze those processes is beyond the scope of this study and is proposed for future research.*"

Comment 3: I found that the tone of this paper implies that WB has provided a new superior metric compared to PE anomaly, threshold methods, and algorithm methods. I personally didn't find that to be objectively proven, and would suggest adjusting the narrative to more objectively present WB in the context of the other metrics. I therefore still suggest major revisions to the text, with more specific comments below.

Answer 3: We apologize if the tone of our original manuscript suggested that our methodology is unequivocally superior. We have modified the wording to present EBM more objectively, alongside other existing metrics. Below, we detail the specific changes made (*page 1, lines 12-14* and *page 25, lines 437-438*):

"This study promotes the development of MLD energy-based methodologies that could offer significant potential for advancing the study of dynamic and thermodynamic processes, including heat content and vertical exchanges."

"In this study, we present a methodology for calculating the MLD, based on physical principles and energy considerations, which promotes the development of energy-based methodologies."

We hope that these revisions clarify that WB is one of several viable metrics for estimating the MLD. Although it does provide the unique feature of measuring water-column inhomogeneity in energy terms, we are not asserting that it is categorically "better" in every context. We appreciate the reviewer's feedback and trust that the revised text now presents EBM in a fairer and more objective context.

General comments

Comment 4: As is already noted in the text, equation 1 is valid for a small perturbation with a locally referenced potential density. It is probably true that the error in using a surface reference pressure is usually small, but I did not find the citation provided in response to my previous comment was sufficient to justify why it is adequate to use surface referenced potential density for the purpose of computing buoyancy work over large vertical displacements. There is an error associated with this assumption, it would be prudent to quantify if/when that error matters for these calculations. I would guess that the deepest winter convective mixed layers, e.g., in deep water production regions such as the Labrador Sea, are situations where the error could be significant. (It may be useful to consider the approach we took in Reichl et al., 2022, where we evaluated the conceptually similar error associated with assuming that surface referenced potential density was the homogenized quantity for PE, rather than conservative temperature and absolute salinity.)

Answer 4: We thank the reviewer for pointing out this observation, which is relevant to our method. We reviewed the mathematical development to define WB. According to Vallis (2017), the expression for the buoyancy force (Eq. (1) in the new version of the manuscript) is valid for vertical displacements larger than infinitesimal ones. From that, we calculated WB as the line integral of the buoyancy force along a trajectory, typically from the MLD to the free surface. Indeed, recent studies have utilized the integral $\int_{\mathrm{MLD}}^{0} \left[\rho^{\theta}(z) - \rho^{\theta}(\mathrm{MLD})\right] dz$ to examine the MLD and mixing in the Indian and Southern Oceans (Lee et al., 2011; Small et al., 2021; Caneill et al., 2024). *Pages 3-4, lines 73-85*.

Regarding the error associated with using the potential density referred to 0 dbar, we addressed this when we explored the correspondence between WB and columnar buoyancy (see answer to Comment 5).

Comment 5: I spent much time trying to understand the arguments on the connection of WB to boundary layer turbulence that surrounds equations 4-7. I find the text to be vague with cursory arguments.

- Equation 4 (similar to eq 1) is only appropriate for infinitesimal vertical displacements with a locally referenced potential density. There is an error associated with the approximation applying it to large vertical displacements. It should be explained.
- The manipulation to go from equation 6 to equation 7 is not explained/justified.
- Equation 7 is dependent on the definition of the vertical coordinate "z", one could add an arbitrary constant "delta" to "z" and it changes the answer. Equation 3 is independent of the definition of the vertical coordinate, so this indicates that there is a problem with equation 7 and its interpretation.
- Equation 7 is compared to a 2008 paper by Herrmann et al., where a similar looking equation is presented. However, Herrmann et al. appear to consider bulk mixed layer dynamics in the derivation and cited work. Thus, their N^2(h) is the buoyancy frequency evaluated at the mixed layer depth ("h") under a bulk mixed layer, and it is not the same as N^2(z) in this paper. This difference is crucial because (a) by using "h" instead of "z" you should drop any dependence on the vertical coordinate, and (b) N^2(h) in Herrmann et al. should be a function of time, the stratification at the MLD base changes as the bulk mixed layer deepens and is not equal to the stratification N^2(z=h) in the initial condition (after some time/entrainment, the uniform density of the fluid within the mixed layer will be lighter due to mixing with the surface relative to the initial density at the depth of the MLD base, hence the density jump across the mixed layer base, N^2(h) in Herrmann et al., is greater than N^2(z=h) in equation 7. Maybe I missed something here (if so it could be explained better in the text), but for this reason I find it problematic to invoke the interpretation of equation 7 having the same meaning as Herrmann et al.'s equation. While the equations look similar, these distinctions seem to be critical for the connection to boundary layer turbulence energetics and, most importantly, mixing energy (that is most relevant for the TKE sink term) vs displacement energy.
- Equation 7 is interpreted to mean the "columnar buoyancy", or the buoyancy flux required to mix a water column to depth "h". I think what is important here about columnar buoyancy is that it is a buoyancy flux equivalent for the energy required to mix a water column over depth "h" (with some assumptions about TKE production and dissipation fraction). The relevant part here is not the columnar buoyancy itself, but that it yields an estimate of the energy requirement to homogenize the column.

Answer 5: Thank you for pointing out this observation; we agree with you that the explanation was unclear. We carefully reviewed the mathematical development to ensure the explanation is clear and supported by robust arguments.

Instead of using the delta of force in terms of N^2, we utilized an approximation derived by Caneill et al. (2024) to calculate columnar buoyancy in terms of the potential density referred to 0 dbar, which is valid for shallow depths. We examined the error associated with this approximation and found that it is small within the first few hundred meters, which is the focus of our study. Then, we showed that WB corresponds to the columnar buoyancy and thus, with the buoyancy flux required to mix that column during a given time period. Finally, using inductive reasoning, we suggested that it is plausible to connect WB with the turbulent kinetic energy budget of the upper ocean layer (Zippel et al., 2022) and, thus, with the physics of boundary layer turbulence. However, further analysis of this connection is beyond the scope of this study and is proposed for future research. *Page 4, lines 93-114*.

Comment 6: The text claims WB "advances the development of MLD energy-based methodologies", which might give the impression that the WB metric improves some inadequacy over the PE anomaly metric. I did not find a claim that WB advances any aspect of PE anomaly supported by the presented results. Another impression given by the text is that WB is aligned with boundary layer turbulence dynamics in a similar way to PE anomaly. I think the distinctions between WB and PE anomaly in that regard needs better clarified. While WB does use an energetically formulated criteria and it allows one to connect the threshold family of metrics to column integral aspects of the PE anomaly metric (this seems clear from section 2.2), it is otherwise distinct from PE anomaly in important ways (it measures displacement, rather than homogenization). This is not a criticism of the utility of WB, I think this concept nicely connects the threshold method to more fundamental fluid mechanics concepts via the work associated with particle displacement. But I wish to reemphasize a point from my previous review, that WB is not obviously aligned with turbulent boundary layer physics in the same way as PE anomaly, and thus does not obviously "advance" the PE anomaly method.

We discussed the energy requirement to homogenize a column of seawater (without a bulk mixed layer assumption) in detail in Reichl et al., 2022 (e.g., building to our equation 11, this is discussed by other work before ours, see references on PE anomaly within). If both energy-based approaches give the energetic distance from well-mixed (as indicated at L12, L57, L113, L131, L357, L417, L424, L432, etc.) then PE anomaly and WB should be equivalent metrics, but they are not. I therefore personally think PE anomaly is the energy metric more directly aligned with concepts mixing, at least these differences should be discussed in the text.

Answer 6: We apologize if the tone of our original manuscript suggested that our methodology advances the development of MLD energy-based methodologies. Our methodology contributes to the growth of those methodologies. We better contextualized our methodology (EBM) in relation to the PE anomaly methodology and added two paragraphs (in section *2. Methodology and data* and section *4. Discussion*) to address that. Please, see the answers to Comments 2 and 3.

Comment 7: I found section 2.2 a very useful addition to understand how WB and the threshold methods compare to one another. I found some of the emphasis on "energy" to be confusing though. "Energetically homogenous" is confusing to me, it sounds like the kinetic or potential energy of the column is homogenous? I think this is not the point, it is meant that a particle displaced in the vertical will not feel any net restoring forces along its path? This concept comes up throughout the text so it should be more clearly conveyed.

Answer 7: We apologize for the lack of clarity regarding what we mean by “energetically homogeneous”. We reviewed and rewrote the first time the term “energetically homogeneous” appears, in the hope of explicitly clarifying the concept of "energetically homogeneous". *Page 5, lines 136-138.*

“This study defines the mixed layer as the energetically homogeneous upper ocean layer; we consider a layer to be energetically homogeneous when water parcels can move with little or no WB within it.

Additionally, we believe this concept is already described (albeit implicitly) at other points throughout the manuscript (*lines 6-7, 124-126, 149-151, 175-177, 178-188, 217-219, 268-270, 273-279, 289-290, 308-310, 315-318, 374-375, 483-484, and 546-547.*

Specific comments

Comment 8: L173: I didn’t understand the justification for saying it works for “different regions and ocean conditions, such as polar seas, intermediate and deep water formation regions, and barrier and compensated layers”, the remainder of the paragraph seems to indicate why it can struggle in certain scenarios? And makes the point why MLD computations that are globally applicable are difficult (a useful narrative!).

Answer 8: We agree that this phrase is confusing here because it was not yet justified; it was removed from this section and added in the conclusions (*page 28, lines 543-547*):

“Based on energy considerations, we proposed an MLD methodology that is globally applicable and produces realistic estimates of the MLD. This MLD methodology performs robustly across different regions and ocean conditions, including polar seas, intermediate and deep-water formation regions, and barrier and compensated layer regions. The mixed layer, determined by energy processes, is quasi-homogeneous in energy, density, and temperature in most of the global ocean throughout the year.”

Comment 9: L205: Clarification: Does “ocean variables” mean that potential density, conservative temperature, and absolute salinity are each interpolated individually? So that it is not potential density computed from the interpolation of conservative temperature and absolute salinity?

Answer 9: The sentence was rewritten to be clearer (*page 9, lines 219-220*):

“Potential density profiles were interpolated to 10 m if conservative temperature and absolute salinity measurements were not available at that depth; a linear interpolation of the potential density profile was implemented.”

Comment 10: L355-359: What is meant by physically realistic is unclear here. The threshold method still gives insight into the depth where the density changes by some value. It is a perfectly valid metric in that regard. It is important to clarify instead what the metrics measure relative to each other. I personally find the PE anomaly metric a better indicator of a mixing depth based on the arguments here than WB since the PE anomaly metric directly measures homogenization and WB measures displacement. Why is the concept of PE anomaly not discussed in this context?

Answer 10: You are right; the phrase "physically realistic" is confusing, and its use is not justified. Therefore, we removed the phrase "physically realistic" from the following sentences (*lines 373-374 and lines 395-396*), which now read as:

"*If the behavior of an MLD methodology deviates from the energy definition of the mixed layer, it is not energy-consistent.*"

"*The above analysis showed that B04D and EBM are energy-consistent, although WB in B04D is almost twice that of EBM in some regions and months, making it difficult to reconcile the large WB values of B04D with our mixed layer definition.*"

We agree with you that the threshold methodologies provide valid MLD estimations. We mention this in subsection *3.2. MLD methodologies intercomparison* (*page 20, lines 369-372*),

"*All the methodologies perform well under the oceanographic conditions for which they were designed, according to the parameter being addressed; Tang et al. (2025) evaluated 12 MLD methodologies and found that each has unique merits and limitations that depend on the analyzed ocean conditions. The determination of the best MLD methodology thus depends on the criterion used to rank the methodologies.*"

We better contextualized our methodology (EBM) in relation to the PE anomaly methodology, summarizing the characteristics of both methodologies and their differences; please, see the answers to Comments 2 and 3. Finally, it was not possible to include the PE anomaly methodology in subsection *3.2. MLD methodologies intercomparison* (*page 9, lines 222-224 and page 26, lines 466-468*).

"*It would have been very significant to consider the PE anomaly in the comparison; however, it was not possible due to the lack of a specific criterion to determine the MLD on a global scale*."

"*Future research could determine optimal values of the PE anomaly for global application and explore this approach in regional contexts by assessing the sensitivity of the MLD estimates to different thresholds of the PE anomaly.*"

Comment 11: L435: Note that you could also compute PE anomaly from rho_0. These arguments about simple formulas and short scripts are entirely subjective. I personally don't think computing PE directly involved any complex formula, either. In fact, I would argue the calculation of rho_0 itself is much more complex than computing PE, particularly if using one of the high-order fits for the equation of state.

Answer 11: We fully agree with you that the PE anomaly can be computed from rho_0. However, since the metric used in our methodology is WB, we are confident that the statement written in lines 448-450 does not contradict the simplicity of its calculation or the merits of the PE anomaly. Thus, there is no need to mention other metrics in this sentence. Reilch et al. (2022) discuss the merits of the PE anomaly very well; readers can refer to their work for further information.

In the repository https://doi.org/10.5281/zenodo.1 4531829, we provide examples in Python, MATLAB, and R to illustrate the recipe for calculating WB, aiming to clarify this statement. As you clearly pointed out, calculating rho_0 could be very complex; however, that task is simplified by using the TEOS-10 toolbox.

Comment 12: L444: The energy required to homogenize a column of seawater is by definition where the PE anomaly concept comes from. If part of the utility of WB is as a "proxy" for this metric, why not use PE anomaly directly?

Answer 12: Of course, the PE anomaly diagnoses the MLD as the depth to which a given energy could homogenize a layer of seawater. The PE anomaly can be used to estimate the MLD. However, it is necessary to determine which energy value should be used to define the MLD globally across all seasons, which Reichl et al. (2022) did not provide (they suggested a threshold range of 10-25 J m^{-2}). They found that a spatially and temporally variable energy threshold should be used to reproduce, to some extent, MLDs similar to those obtained with the methodology of Holte and Talley (2009). *Page 26, lines 464-465 and page 27, 518-520.*

In addition, EBM could complement the existing MLD methodologies by providing additional information (*page 26, lines 468-475*):

"*By comparison, with EBM, we were able to find the energy values (in terms of WB) that define the MLD globally during all seasons. Moreover, EBM provides additional information not supplied by the existing MLD methodologies; WB represents the potential energy barrier to the vertical displacement of water parcels, which could complement the analyses of oceanic processes occurring in intermediate vertical sections, commonly associated with interchanges of properties along the water column, such as the flux of particulate organic matter from the surface to sediments (Kirillin et al., 2012; Omand et al., 2020), the vertical content of chlorophyll (Carvalho et al., 2017; Briseño Avena et al., 2020), and entrainment in barrier layers (Katsura et al., 2022). The application of WB to analyze those processes is beyond the scope of this study and is proposed for future research.*"

Comment 13: L450: The discussions surrounding PE anomaly (based on mixing rho_0) vs "delta PE" (based on mixing conservative temperature and absolute salinity) metrics in Reichl et al. (2022) seem extremely relevant to this conversation.

Answer 13: In the statement you mentioned, we are referring to commonly used MLD methods, not to energy-based methods. We properly included discussions regarding the EBM and PE anomaly methodologies in other more appropriate sections of the manuscript (see the answers to Comments 2, 6, and 7).

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 #2 Hervé Giordani

Summary

Comment 1: I thank the authors for responding accurately to my questions and for extensively editing the manuscript. I realize that it was a huge but necessary effort to ultimately produce a much clearer and more convincing version. I suggest to the authors to turn off the ”trackchange” mode to have a manuscript that is easier to read. In this final form, I accept the article for publication.

Answer 1: We thank the reviewer for taking the time to evaluate our revised manuscript and for finding our study a valuable contribution to the research on the mixed layer.

We have prepared a revised version of the manuscript, incorporating the comments of another reviewer. The changes in this version are the following:

1) We better contextualized our methodology, EBM, in relation to the PE anomaly methodology proposed by Reichl et al. (2022).
2) We reviewed the mathematical development to define WB.
3) We reviewed the mathematical development to explain the relationship between WB and columnar buoyancy, ensuring the explanation is clear and supported by robust arguments.

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 without tracked changes.

Comment 2: Lines 983-1002: I appreciated this discussion. You mention that the EBM-MLD intrinsically depends on the $\Delta\rho^{\theta}$ threshold, which may negatively influence its performance. I am wondering how to overcome this threshold. Following Equation 8, if you impose WB = 0 (or WB small), then $\rho(h)$=ρ. In that way, we can construct the following iterative process to obtain the MLD h:

$$h^{n+1} = \eta - \frac{1}{\rho(h^n)} \int_{h^n}^{\eta} \rho(z)dz \qquad \text{where } n \text{ is the iteration} \tag{1}$$

h is defined when $|h^{n+1} - h^n| \leq \epsilon$ where ϵ is your convergence criteria.

Answer 2: Thank you for your comment and suggestion to overcome the dependence on the $\Delta\rho^{\theta}$ value to define the MLD, which is very interesting. We will explore this proposal in future analyses to refine the MLD, which could potentially lead to revisiting the definition of the MLD.

Minor points

Comment 3: Line 163: Replace the lower bound of the integral z_{ref} by z_{eq} in Equation 3.

Answer 3: We apologize for that typo; we amended it. Please, see *Equation (2)*.

Comment 4: Line 194: Replace “the time integral of the buoyancy flux” by ”the time integral of the surface buoyancy flux”

Answer 4: Thank you for your comment. We rewrite that sentence, which now reads (*page 4, lines 111-114*):

“*Columnar buoyancy represents the buoyancy loss required to mix the water column from the surface down to a depth of h during a specific time period (Lascaratos and Nittis, 1998; Herrmann et al., 2008); dividing columnar buoyancy by this time period gives the buoyancy flux required to mix that column during the same time period (Faure and Kawai, 2015).*”