The reviewer's comments are in black, and the author's reply below are in red.

# Review of McDougall:
# Thermodynamic concepts in physical oceanography

Review provided by Stephen Griffies
Princeton University
October 22, 2025

First, the author, Trevor, would like to sincerely thank the reviewer, Stephen Griffies, for such a thorough review. Thanks for reading it so closely, and for all the great suggestions.

## 1 Summary

This is a useful review of thermodynamics used in ocean physics, drawn from the author's published work and based on his 2025 EGU Wegener Medal lecture. The author is the world's leader in ocean thermodynamics and its applications. I offer only some minor comments focused on points of clarification. So an official recommendation is "minor revision."

## 2 Specific comments

### Why salinity at the start of the manuscript?

It is not clear to me why the salinity section is the lead section after the Introduction. There are few concepts here. Instead, it details various recipes and papers where rationales are presented. I suggest moving this section to later in the paper, to thus start with the major conceptual piece concerned with the First Law.

Thanks. I've now moved the salinity section.

### line 116

Explain "SI-traceable manner".

"SI-traceable" means that a measurement can be traced back to the definitions of the fundamental units of nature (length in m, time in second, and mass in kg). Practical Salinity is not an SI-traceable measurement. In contrast, using a vibrating beam densimeter can give an SI-traceable measurement of mass-fraction salinity. These issues are quite detailed and are summarised in the Wright et al. (2011) paper. I did not elaborate in the text but left it to the reader to read other papers. I think the word "SI-traceable" is well understood.

### line 116

I suggest adding a few words to indicate where $u_{PS} = 35.16504/35$ comes from in equation (1). At the least, provide a few conceptual bread crumbs for the interested reader.

Thanks. The reason for this strange $u_{PS}$ number has to do with the history of how salinity was measured a century ago, and this ancient practice was then cooked into the definition of Practical Salinity. We broke with that tradition when we changed the definition of salinity under the TEOS-10 standard. I have added an explanation of the reason for the strange $u_{PS}$ number in the new section 7.

### line 165

"total derivatives" →"total differentials" (as correctly used on line 169).

Thanks. This is now fixed.

**intensive variables in equation (4)**

Throughout this manuscript, $h$, $u$, and $\eta$ are referred to as enthalpy, internal energy, and entropy. In fact, these terms are all per unit mass. This point should be noted around equation (4), the fundamental thermodynamic relation, FTR,

$$dh - vdP = du + Pdv = Td\eta + \mu dSA. \qquad (1)$$

Thanks. I have added text to make it clear that we are talking about specific variables.

**lines 169-170: reversible versus quasi-static**

The fundamental thermodynamic relation holds between two thermodynamic equilibria. On line 169-170, McDougall further states that the total differentials represent reversible differences between such equilibria. A similar statement was also present in an early version of McDougall et al. (2023). However, as I noted in my peer-review of that paper, the physics literature, such as the gold standard texts from Reif (1965) and Callen (1985), allow for the FTR to hold for the broader category of processes known as *quasi-static*. Consequently, the FTR, in their presentation, holds for both reversible and irreversible quasi-static processes. I note that earlier treatments of thermodynamics did not make the distinction, but the distinction became important when more work was done to understand irreversible processes in the 20th century (Callen's thesis work in 1947 was on irreversibility). As a result, we can extend the FTR to include friction and diffusion, which is clearly important for ocean physicists to use the FTR in a moving and real fluid.

In response to this earlier reviewer comment, McDougall et al. (2023) added a nice discussion in their Section 1.2 about some basic conceptual issues. However, nowhere in that discussion did they argue for the FTR holding just for reversible processes rather than the broader class of quasi-static processes. They focus on the need for thermodynamic state functions, in particular entropy, to remain state functions when working with a moving fluid, thus allowing for differentials to transfer over to material time derivatives. But these properties are precisely the properties maintained by quasi-static processes.

So in summary, the present manuscript, with the word "conceptual" front and centre in the title, is an ideal place to raise these conceptual points. How we in the ocean physics community make use of the First Law has a lot to do with the hypothesis of local thermodynamic equilibrium (Section III.2 of DeGroot and Mazur (1984)) and the distinction between quasi-static versus reversible processes. That is, the present paper is just the place to address, head-on, what McDougall et al. (2023) refer to as "otherwise annoying conceptual issues", which were sidestepped in that paper.

Thank you for this extremely valuable comment. You are 100% correct, the FTR has no apply for the quasi-static series of states, as Callen (1985) explained. Otherwise, we could not use the First Law in fluid mechanics! I have now made this very clear. I don't know how this didn't make it into the first draft.

**Line 171**

"We tend to regard" is a rather qualified and tentative language. Is there a reason for such language?

Thanks. This has been changed.

**line 192**

I am puzzled why the kinetic energy per mass is not afforded its own symbol, such as

$$K = \tfrac{1}{2}\mathbf{u}\cdot\mathbf{u} \qquad (2)$$

The current use of $0.5\mathbf{u}\cdot\mathbf{u}$ throughout the energetic discussions is clumsy.

Thanks. I have adopted this suggestion.

**line 197**

I suggest dropping "quality" from the following sentence, since it implies that those books not treating the First Law have low quality.

> "This evolution equation is derived in detail in quality fluid dynamics textbooks..."

Namely there are any number of fluid dynamics texts that are only concerned with hydrodynamics, waves, vorticity, etc., and choose not to treat thermodynamics. I do not think you wish to imply that all such books are low quality.

Thank you for this comment. I have added the word "many" so the sentence becomes

> "This evolution equation is derived in detail in many quality fluid dynamics textbooks..."

**lines 236-239**

These lines are rather packed with ideas yet not supported by the maths. I think it useful to include some equations. Additionally, wording in these lines can lead to confusion. Namely, molecular diffusion vanishes in thermodynamic equilibrium since in situ temperature and chemical potential are uniform. But the way it is written, it seems one needs to assume zero molecular diffusion in order to realize thermodynamic equilibrium. Some rewording would be useful.

I recommend adding some of the text from the TEOS-10 manual to clearly explain why thermodynamic equilibrium is irrelevant for the ocean, given flow has a turbulent nature and so homogenizes potential properties rather than in situ temperature and chemical potential. This point is only hinted at here, by noting that a thermodynamically equilibrated ocean has a huge and unobserved vertical gradient of SA. But there is no statement to the effect that "the observed ocean is nowhere near thermodynamic equilibrium for reasons A and B." Instead, the reader is meant to infer that conclusion, which is not a good strategy for a review paper aimed at a broad audience.

Thanks. This discussion did need expanding. The discussion of thermodynamic equilibrium (zero molecular fluxes) versus the mixing of "potential" properties by turbulence, is now discussed in the paragraph following Eqs (7) and (8) in section 2.3.

**lines 269 and 277**

The right-hand side of equation (12) has $-\nabla\cdot\mathbf{F}^{C}$. With the minus sign this term represents the "convergence of the flux, $\mathbf{F}^{C}$." Hence, I suggest calling it the flux convergence rather than the flux divergence.

Thanks. Done.

**Arguments surrounding equation (16)**

The discussion leading up to equation (16) makes use of some standard Leibniz-Reynolds arguments for finite volume budgets. However, I am puzzled by details of the chosen control volume used to throw away the boundary integral term while maintaining constant pressure, $P = P^{m}$. A picture of the control volume would prove very useful here.

In particular, the restriction $P = P^{m}$ needed for equation (15) means we are restricting attention to the isobaric surface. So when performing a three-dimensional finite volume budget, how can all of the region remain with $P = P^{m}$, unless you are assuming constant pressure within the volume, which seems odd. For these reasons, I am unsure why we can drop the $\mathbf{u}^{\text{dia}}$ term projected onto the normal direction of the boundaries. I think a carefully drawn schematic of the finite volume would help heaps.

Also, I do not understand why molecular mixing is absent (line 301). Is it instead neglected due to dominance by turbulence? Thanks. There is no problem with retaining the molecular diffusion of heat. The text has been changed to make this clear.

So, the bottom-line question I have is the conclusion on line 304. Namely, the arguments leading to equation (16) are finite volume arguments in 3-dimensions, whereas turbulent mixing at $P = P^{m}$ is a local process that occurs at a point or on a surface.

Thanks. This comment by the referee, Stephen Griffies, caused me to think a great deal. The result is the last long paragraph of section 3 and section 5.3. The ideas contained in this new material have been swirling around in my head for a few years, but this probing by the referee made it clear that this material was necessary in this review. The question answered in section 5.3 seems a simple one, namely

> Consider a tall tank of seawater (aka a deep mixed layer) that is heated (perhaps by an electrical heating element at some known depth, or perhaps by a surface heat flux) and is also being vigorously mixed by mechanical stirrers so that the "potential" properties Absolute Salinity, entropy η, and Conservative

> Temperature are always almost spatially uniform. The question that we ask is "how fast does the Conservative Temperature of this mixed fluid evolve with time?"

Section 5.3 makes clear that the appropriate specific heat capacity is the one evaluated at the mass-averaged pressure. Back in section 3, here I addressed the question of mixing fluid parcels (note that here they are undergoing mixing as opposed to section 5.3 which is concerned with a external flux of heat) that are not infinitely thin, and the same conclusion is reached, namely that the relevant pressure to take when considering the reference pressure for the enthalpy that is conserved, is the enthalpy referenced to the mass-averaged pressure.

**line 305**

"The almost conservative behaviour of $h^m$ ...". Where in the previous text is it shown that $h^m$ is almost conservative rather than fully conservative?

Thanks. This has now been reworded to say what was intended, namely that the locally referenced potential enthalpy is conserved apart from the Joule heating of the dissipation of kinetic energy.

**Equation (18) and following**

The one-sentence paragraph ending with equation (18) introduces $\hat{h}_\Theta$ and $\hat{\eta}_\Theta$, yet without defining the hats and the subscripts. I presume the subscripts are partial derivatives, but that needs to be stated. But what are the hats for?

A reminder of the meaning of the over-hat nomenclature has been added here. Also, the paragraph was meant to be more than the one sentence, and it now is.

My guess is that this section is a cut-and-paste from another document, with incomplete transfer of symbols. Indeed, on line 336 we find a reference to equation (R.02), but that equation is nowhere in the present manuscript. Thanks. Actually, none of this section was a cut and paste. There is a fair bit more here than was in the McDougall and Feistel (2003) paper on the adiabatic lapse rate. The (R.02) typo has now been fixed.

**Equation (23)**

Again, we find the undefined symbols $\hat{h}$ and $\hat{\eta}$. Thanks. A reminder about the meaning of the over-hat notation is now included here.

**line 461**

Need to define $T_0 + t$. I note that it is unfortunate that $t$ is used here for a temperature whereas it is used earlier for time.

Thanks. These temperatures are now defined here.

**line 474**

It would be useful to give numbers for the heating due to dissipation of turbulent kinetic energy, in both the ocean interior and in boundary layers. Also, state why we in the ocean physics community can ignore this dissipation when modelling the large-scale circulation, whereas atmospheric modelers find it essential.

Thanks. The magnitude of the non-conservation of Conservative Temperature is now discussed in section 7.3, and again in the Summary section 14.

**line 483**

It is useful to remind the reader that mixing occurs at constant pressure, which then indicates why enthalpy is conserved during mixing.

Thanks. This is now done here.

**line 491**

The second $m_1$ should be $m_2$. Thanks. Fixed.

**line 519**

spelling "derivates". Thanks. Fixed.

**line 523**

evaluate →evaluated. Thanks. Fixed.

**line 685**

"in terms of". Thanks. This has now been fixed.

**line 742**

I suggest "$2\frac{1}{2}$%" is better written as "2.5%". Thanks. Done.

**line 815**

Need to add "= 0" after "$\beta\delta S_A - \alpha\delta\Theta$." Thanks. This typo has been fixed.

**line 903**

"..where the over-hats indicate that these variables are functions of ($S_A$,$\Theta$,$P$)." Does this specification also hold for all other hat variables in the manuscript that were written earlier? If so, then the reader would welcome having that information stated way back near equation (18).

Thanks, I have added this meaning of the over-hat in the section about the adiabatic lapse rate, and have repeated it here as a reminder.

**line 944**

The sentence starting "In that case..." is awkward. I suggest rewording. Thanks. This has been changed.

**Section 13**

Section 13 is title "Neutral Surface Potential Vorticity," which is the title of McDougall (1988). However, as noted early in Section 13, we are here only concerned with planetary geostrophic potential vorticity. I thus recommend changing the section title to "Neutral Surface Planetary Geostrophic Potential Vorticity." Although a bit more of a mouth-full, it does serve as a more honest advertisement for what is to come.

Thanks. I have adopted this recommendation, even though one could add relative vorticity to $f$ and the equations still follow through exactly the same.

**Expression for potential vorticity**

Is the neutral surface potential vorticity defined by equation (48) materially invariant in the absence of irreversible processes? Was this shown in McDougall (1988)?

No, materially invariance is not possible to show because the approximately neutral surfaces inherently involve epineutral mixing which upsets the material invariance. Nothing so tidy as this is possible when dealing with approximately neutral surfaces.

**line 1113**

Reference is made to equation (113). This manuscript has no such equation. Perhaps another cut-and-paste error. Thanks. This typo has now been fixed.

### Equation (61)

This reviewer admits being a bit exhausted by this point, so I can certainly accept if I missed something. But what exactly is $\kappa$? That math symbol does not appear in equation (47), which is claimed to have its curl render equation (61). Perhaps it is defined earlier and I forgot where?

Thanks. I have now added the definition of $\kappa$ in a few places in the text, including here.

### Figures

It is a shame the figures were not inserted in their proper place, thus making the reading far simpler. The old days of relegating figures to the end of the preprint seem unnecessary in today's world, particularly when few readers actually print a hard copy. OK.

Some of the figures (e.g., 2, 3, 11) do not render well when zooming. Perhaps they are taken from their original publications from decades ago. It would be a shame to have blurry figures in this otherwise thorough and impressive review.

### Figure 2

I have a tough time finding the black dots. Sorry.

## References

Callen, H. B., *Thermodynamics and an Introduction to Thermostatics*, John Wiley and Sons, New York, 493 + xvi pp, 1985.

DeGroot, S. R., and P. Mazur, *Non-Equilibrium Thermodynamics*, Dover Publications, New York, 510 pp, 1984.

McDougall, T. J., Neutral-surface potential vorticity, Progress in Oceanography, 20, 185–221, doi:10.1016/0079-6611(88)90002-X, 1988.

McDougall, T. J., P. M. Barker, R. Feistel, and F. Roquet, A thermodynamic potential of seawater in terms of Absolute Salinity, Conservative Temperature, and *in situ* pressure, *Ocean Science*, 19, 1719–1741, doi:10.5194/os-19-1719-2023, 2023.

Reif, F., *Fundamentals of Statistical and Thermal Physics*, McGraw-Hill, New York, 1965.

The reviewer’s comments are in black, and the author’s reply below are in red.

Review of "**Thermodynamic Concepts used in Physical Oceanography**"
by Trevor McDougall (2025) for Ocean Science
Geoff Stanley
Canadian Centre for Climate Modelling and Analysis
05 November 5, 2025

First, the author, Trevor, wishes to thank the reviewer, Geoff Stanley, very much for his thorough reading of the manuscript, and for his many insightful comments.

## General Comments

This review paper covers a big topic in Physical Oceanography that is often overlooked and/or misunderstood. The author has led progress in this field, working with a relatively small group of collaborators, for ~40 years, and so possesses a great store of knowledge that is vital to share with the wider community in an accessible format. This is a welcome review that goes a long way towards that end. I recommend it be published with "Minor Revisions".

## Specific Comments – Major

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880 - 882 - "at such an extremum of CT on a neutral surface, a new branch of the γ(S_A, Θ) function arises". It's not the extrema where new branches arise, but at the (contours through the) saddle points. The only way that the extrema can bound a space where a branch of the function lives is if there are only two extrema (a max and a min) and then there is only one branch and we're back to being single-valued. It's saddles (and islands) that open up new branches. What I think you mean here is "an extremum of CT on a neutral trajectory" not a neutral surface. But this in general this is incorrect -- unless you have very luckily chosen the neutral trajectory that goes through the saddle of CT on the neutral surface containing that neutral trajectory. Otherwise and generically, the neutral trajectory will cross the contours of CT on this neutral surface that go through the saddle of CT, and it is these contours that are the boundaries between different branches of the multi-valued function, so this neutral trajectory will actually go through at least 3 branches, not 2. And more importantly, for these generic neutral trajectories (ie that do not go through the CT saddle on a neutral surface), the extremum of CT does *not* occur at a point where the function switches branches. The picture I have in mind here is of a typical saddle, going up (warmer) to the east and west, and going down (colder) to the north and south; then draw neutral trajectories as straight north-south lines. Any neutral trajectory that does not go through the saddle will have its maximum somewhere inside the lobe to the east or west of the saddle. Thanks for this. Actually, a saddle point is not needed to explain why a single-valued function $\gamma(S_A, \Theta)$ cannot describe an approximately neutral surface. Instead, it can be due to the different SA-CT-P data in the two hemispheres. And it can occur with zero neutral helicity everywhere. I have rewritten what was in the text to now read “The Conservative Temperature and Absolute Salinity on these all-Atlantic approximately neutral surfaces reach maxima at the approximate latitude of the Mediterranean Sea, and in general, the pressures at pairs of locations on the approximately neutral surface that have the same Conservative Temperature, are different. This means that the whole surface cannot be fitted with a single-valued function $\gamma(S_A, \Theta)$, as is illustrated in Figures 5, 9(a) and 10 of McDougall and Jackett (2007) for the hydrography of the Atlantic. This effect occurs even when neutral helicity is zero everywhere; the multi-valued nature of a function $\gamma(S_A, \Theta)$ that would be needed to fit such an approximately neutral surface does not imply that there is path-dependent induced dianeutral upwelling through the approximately neutral surface (McDougall and Jackett, 2007). A more detailed explanation of

the reasons for the non-neutrality of the $\gamma(S_A, \Theta)$ functional form can be found in McDougall et al. (2017)."

950: "This is guaranteed, ... has been proven untrue". I didn't notice this until now, but this is actually not a proof. I agree up to and including that $\oint(\alpha/\beta)\,d\Theta = 0$. I agree that if α/β is a function of S and Θ only then this integral is zero. But α/β being a function also of pressure does not necessarily make the integral non-zero --- because the pressure field could be carefully tied to the S and Θ fields. Looking ahead to your next paragraph, it's actually the neutral helicity H being non-zero that means neutral surfaces don't exist. And H is basically T_b times the triple scalar product $\nabla P \cdot \nabla S \times \nabla \Theta$. So we could conceivably have $T\_b \neq 0$ (ie α/β is a function of pressure) and the triple scalar product = 0, so that H = 0, and neutral surfaces exist (locally). So the reason neutral surfaces don't exist is because *both* $Tb \neq 0$ (ie α/β is a function of pressure) and the triple scalar product $\neq 0$. The argument about $\oint(\alpha/\beta)\,d\Theta$ only considers that $Tb \neq 0$, so it turns out to not be a proof. It feels like $\oint(\alpha/\beta)\,d\Theta$ ought to be non-zero, but that is just the same feeling that the triple scalar product is non-zero. Thanks. I have rearranged the text of this paragraph to make this clear.

How to fix this? The easiest is to delete everything from "Let us begin" to "proven untrue" and move "Figure 1 ..." to the next paragraph. If you want to keep this $\oint(\alpha/\beta)\,d\Theta$ discussion (it is illuminating, after all), then I think you'd need to include some of the above, stating that a contradiction was not in fact reached and that we must go further to examine the neutral helicity. Thanks. I did this.

Section 14 -- I would love to see some more discussion here about avenues for future work (or perhaps avenues that are known to not prove fruitful) in seawater thermodynamics, particularly on the salinity and temperature variables and the equation of state. As young Trevor is getting younger every day, and there are not very many other people who have worked on these topics with him, this seems a good location to impart some direction for the field. For instance, do you think any heat-like variable will ever surpass Conservative Temperature? Oceanographers forty years hence would like to know. Thanks. I now do exactly this in the last paragraph of the summary section 14: a call to arms.

## Specific Comments – Minor

171-174 - I would like to see further discussion here about how it is that S_A, T, P can be considered state variables. In particular, it jumps out that T is different from S_A and P, in that P and S_A appear as dP and dS_A, whereas T appears as T multiplying dη --- yet S_A, T, and P are all state variables on an equal footing in the end. There are many possible sets of three thermodynamic variables to describe an ocean state. I have added "Of the various choice of three variables to describe the thermodynamic state of a seawater parcel, the $(S_A, T, P)$ combination is rightly popular because each of the three variables are (close to being) measurable quantities."

229 - here F^Q is taken to be the molecular flux of heat, but on line 209 it also included the radiative flux of heat. Thanks. I now make clear that F^Q stands for the sum of boundary, radiative and molecular fluxes of heat.

252 - 261 - I quite liked this discussion with some surprising thermodynamic properties of seawater! However, if I understand correctly, the change of E caused by a hydrostatic pressure change of 1000dbar is not actually due to the pressure change, but due to moving the fluid parcel through the gravitational field until a 1000dbar change has occurred. The word "hydrostatic" does a lot of work on line 260. In contrast, the enthalpy change due to pressure is direct; putting seawater in a soda bottle and squeezing it, without moving it up or down, would change the enthalpy, but very little change to the total energy E (only due to changes of u, which as you say are much smaller). I'd recommend clarifying this point. Thanks. I have now made this point clear.

314 - For this discussion of why E is not conserved despite dE/dt equalling a divergence, it may be good to link to Figure 8 here. Thanks. I do now give the reader a pointer to the up-coming section 7.2 and Figure 2.

Eq 18 - this is the first use of $\hat{\eta}$. Probably just need to explain what the hat notation means, presumably a function of (S_A, Θ, P). Thanks. Done.

364-5 - Do you really know this for ALL liquids? Thanks. I changed this to “For seawater …”.

431 - "The Gibbs function is unknown and unknowable to the extent ..." This always sounds to me like there is some true value of the Gibbs function out there, but we just cannot and will never be able to determine it. Wouldn't the commonly used phrase "arbitrary up to the addition of ..." be better? (Also, "the extent" doesn't indicate that the expression that follows is meant to be in addition to g.) Thanks. I have changed this description a little, including adding the sentence “Feistel and Wagner (2005) note that the common practice of setting the entropy of a substance to be zero at the temperature of zero Kelvin (the third law of thermodynamics) does not work for ice since it undergoes unexplored phase transitions between different types of ice as it is cooled from planetary temperatures to absolute zero Kelvin.”

492 - this can't be right, because setting $m_1 = m_2$ yields 1/8 = 1/2. Thanks. This typo has been fixed.

Eq (26) and (27) - Would it be worth commenting on how mixing two parcels of equal θ (or Θ) but different S_A leads to the mixture having different θ (or Θ)? Thanks. I have added “Interestingly, when two seawater parcels at the same potential temperature but contrasting salinities are mixed, the potential temperature of the mixture is different to the initial potential temperature, this being due to the $\tilde{h}_{S_\mathrm{A} S_\mathrm{A}}$ term. The effect goes by the confusingly named “enthalpy of mixing effect”; confusing because enthalpy is conserved during this mixing process.” At the sea surface, $\hat{h}_{S_\mathrm{A} S_\mathrm{A}}$ is zero so there is no production of Conservative Temperature there.

559-563 - this "In conclusion" paragraph is a bit repetitive of what was just stated at the start of the previous paragraph (line 551 - 552), which already was a recap of what was said on lines 514-517 (albeit just for CT, not specific volume). So maybe there could be some streamlining. Thanks. I have made a number of additions to this subsection, being now section 7.2, and I hope that it now reads well.

577-9 - This also requires assuming ΔS_A = 0, right? No. The mixing between dissimilar seawater parcels will cause the non-conservative productions that are discussed in this paragraph. The Absolute Salinity, while being different in each seawater parcel, is conserved on mixing.

713 - "~2%" --- should this be "~2% or greater"? Otherwise, a *lot* is resting on how accurate that "~" means! Thanks. This has been fixed.

Eq (36), 829 - I really dislike ρ^Θ to refer to potential density. It looks like it has something to do with potential/Conservative Temperature. Just saying! Thanks. Yes, it is a bit clumsy. I can’t think of a better alternative and given that it is written like this in the TEOS-10 Manual (IOC et al. 2010), I decided to stick with this notation.

835 - 838 - To get that these three quantities vary quadratically in space requires assuming that that P and Θ vary linearly in space. This applies to both line 838 and 837. I think that should be stated. (Also line 925.) Thanks. This has now been stated immediately after Eq. (43).

892 - "Θ is the thickness-weighted Conservative Temperature of density coordinate averaging". This might be interpreted as doing TWA using in-situ density, which of course is not so different from geopotential-based averaging. This mistake has been made in the literature. So I'd recommend changing "density" to "neutral density". Thanks. These sentences have now been changed to reflect this.

931 - 932 - "epineutral front" does not appear to be used in the literature (a google scholar search for this term in quotes returns only this preprint), so it is worth defining it here or using another term. Thanks. The term “epineutral front” has now been explained.

932 - 936: I'm a bit sceptical of this idea that the area integrated effect of thermobaricity and cabelling is independent of K and $\nabla_n\Theta$, from Klocker and McDougall (2010; "Influence...", p 1698). Their argument relies on assuming the epineutral $\Theta$ flux is constant with distance across the front. So at the very least that assumption should be stated here. But, how good is that assumption? The relevant integral is $\int K\,(\partial\Theta/\partial y|_n)^2\,dy$, and KM10 take $K\,(\partial\Theta/\partial y|_n)$ out of the integral, which feels like a major leap, and I think is not well justified by KM10 (though I may be missing something, not having reviewed that whole paper). So I am questioning whether it is a good idea to promote this idea in this review paper. Thanks. I am comfortable with including this in the review, but I have now added a sentence explaining the idea in more detail. The idea is to take to product of the average epineutral heat flux and the overall temperature difference. This product is proportional to the cabbeling dianeutral transport (per unit length along the front).

957 - 958 - "the scalar product ... with its curl must be zero everywhere in order for all the neutral tangent planes to join up" -- I fear many readers will interpret this as "H = 0 implies neutral surfaces exist". The correct interpretation, assuming we are on Earth which has islands and not on an aquaplanet, is "H = 0 is a necessary condition for neutral surfaces to exist". Many thanks for this. I had forgotten to refer to the work of Stanley (2019a) on this. This is now done.

972 - 973 - the "That is, ..." part is weaker than what comes before it. The second part, that H = 0 requires $\nabla\Theta \times \nabla S_A$ be in the plane of constant P, is easy to see and should be stated first, I think. The first part, that $\nabla\Theta \times \nabla S_A$ is parallel to $\nabla P \times \nabla\rho$, requires some additional steps and so should come second, at least, if not with the extra steps. The steps I think are: show that $\nabla\theta \cdot \nabla P \times \nabla\rho = \rho_S\, \nabla\theta \cdot \nabla P \times \nabla S = 0$ and similarly show $\nabla S \cdot \nabla P \times \nabla\rho = 0$, so $\nabla P \times \nabla\rho$ is orthogonal to both $\nabla\Theta$ and $\nabla S$, and so conclude. Thanks. I have rearranged this discussion as suggested.

981 - This is only about the geostrophic velocity, right? If so, suggest changing "Eulerian mean horizontal velocity" -> "Eulerian mean geostrophic velocity". Also, this paragraph could use a citation. Thanks. Yes I have added the word geostrophic, and have provided a reference.

1014 - "but the data fills only a few percent of the area described by connecting every data point to every other point". It's not clear what this means: what is the area of "the data" that forms the numerator of this ratio? The denominator is has a well-defined area, but the area of a finite set of points is 0. Thanks. I have now explained this better.

1039 - 1041 - This makes it sound like Stanley et al. (2021) reduced the number of iterations required to converge (which is true) by using the Poisson formulation (which was not the cause of the reduced iterations). The cause of the reduced iterations was using the better "vertical solver", ie doing carefully what you described a few lines above for the Klocker et al (2009) algorithm. I suggest replacing "finds the increment of height on the $(x, y)$ vertical cast corresponding to this increment $\Phi(x, y)$ in $\ln\rho^l$" with "converts $\Phi(x, y)$ to a height increment using the local stratification", and also inserting something like that text that was replaced to go after "Poisson equation solver" and before "converging to the final...". Thanks for this. I have made the changes.

1085-1086 - I find this surprising that $g^{-1} N^2 / (\alpha_p\, \Theta_z - \beta_p\, S_z)$ would be essentially unaffected by heave. If the p derivatives were not present on the $\alpha$ and $\beta$, then this ratio would just be a constant; but isn't the linear combination that uses $\alpha_p$ and $\beta_p$ totally different from that using $\alpha$ and $\beta$? Thanks. I think it’s OK. The heave affects $\Theta_z$ and $S_z$ in both $N^2$ and $\left(\alpha_P\Theta_z - \beta_P S_{\mathrm{A}_z}\right)$.

1192 - 1193 - Should "dashed" be "solid" here? To get from cast A to 2' on cast B, you need to go on the solid curve. Also, "pint" -> "point". I could use a pint at this point, thank you! Thanks. I did have the full and dashed lines mixed up, and this has now been corrected. The pint will have to wait; for now, it has been replaced by a point.

1245 - "This area of oceanographic research has attracted very little research and very few papers, and any conclusions drawn above about this topic must be regarded as preliminary; as unfinished business." I think this is overly pessimistic. True, there are relatively few papers on this topic, so maybe you mean that existing papers on the topic are "preliminary" to all the future papers that will come. But I think most people would read this as saying the existing papers on the topic are "preliminary" in that they are themselves incomplete and not very good. That is hardly the case! Thanks for this comment; I agree with it. I have now changed the tone entirely to be "This area of oceanographic research has attracted very little research and very few papers, but nevertheless there are several options for forming surfaces that are approximately neutral, and these options are discussed in section 12.2. ".

## Technical Corrections

166 - volumes -> volume. Thanks. Fixed.

282 - this is the first use of Θ. Should be explained what it is. Thanks. I now have explained what Θ is the first time it appears after the Introduction section.

289, 306, ... - Leibnitz -> Leibniz. Thanks. Fixed.

336 - "(R.02)" -> (19) ? Thanks. Fixed.

439 - 441: "Dear reader ... trash bin where it belongs." Excellent! No change needed here. ;). Thanks for this positive comment, but I decided to delete this as it sounded a little bit conceited.

482 - in -> is. Thanks. Done.

491 - $m_1$ and $m_1$ -> $m_1$ and $m_2$ Thanks. Done.

523 - evaluate -> evaluated. Thanks. Done.

523 - "term in pressure integral of..." sounds like it's talking about the integrand, but you actually mean the integral itself. So delete "term in". Thanks. Fixed.

532 - "that of" -> "that, of". Thanks. Fixed.

556 - that -> the. Thanks. Fixed.

558 - ";" -> "," Thanks. Fixed.

685 - "terms entropy" -> "terms of entropy". Thanks. Fixed.

707 - "(and to Absolute Salinity)" -> "(and Absolute Salinity)". Thanks. Done.

709 - named -> namely. Thanks. Done.

726 - delete ")". Thanks. Done.

742 - "2 1/2 %" looks rather odd. Suggest "2.5%" Thanks. Done.

748 - effects -> effect. Thanks. Fixed.

750 - ":-" -> ":" Thanks. Done.

835 - Eq (38) isn't about neutral variations, so change "three" to two" and delete "(38)". Thanks. Fixed.

838 - delete "an". Thanks. Fixed.

880 - "neutral surfaces" -> "neutral trajectories". Thanks. Fixed.

986 - The expression here only involves Pressure and CT, so delete "salinity". Thanks. Fixed.

1024 - Citation typo: "Jackett and McDougall (1985)" -> "Jackett and McDougall (1997)". Thanks. The citation is now fixed.

1029, 1043 - the $\omega$ looks like it has an overbar on it. Can that be removed? Thanks. I have changed these to the normal omega font, and I found several more omegas that needed changing as well.

1050 - "to a few by $10^{-9}$ m $s^{-1}$" --- delete "by". Thanks. Done.

1071 - It seems like "the vertical distance between adjacent neural surfaces" is collapsing the world over. Brain rot is taking hold. "neural" -> "neutral" ;-). Thanks. It's now neutral.

1238 - "closer" -> "is closer". Thanks. This has been changed.

The reviewer's comments are in black, and the author's reply below are in red.

Review of "Thermodynamic Concepts used in Physical Oceanography"
by Trevor McDougall (2025) for Ocean Science
Julian Mak
02 January 2026

I read this from fresh and made some comments, then went back to cross-check with the other referee comments and it seems my comments largely echo theirs (which I hope means I am not spouting completely rubbish). Some of their comments I will repeat below. There are quite a few comments, but most of them are clarifications and writing.

- Comment about targeted audience: As discussed with the author already, there was a suggestion about this being an entry for the encyclopedia of geophysics or whatever it is called. The present content and presentation is targeting a sufficiently specific audience for that. I don't mean this as a criticism at all however: technical subjects require using technical language and content, and some things do need to be "worked on" to be understood, and this is probably one of those. While it is true some presentation and points here could be clarified (probably will some additional schematic diagrams maybe), I personally would think it is a shame if the technical content is removed/lessened in favour of a wider appeal. OK.
- I agree with referee 1's point about "salinity" being placed where it is, it did seem slightly odd to me. The author's call on this: does going after the current section 4 "potential and conservative variables" work? Thanks. I have now moved the salinity section down to what is now section 8.
- line 75: "Pawlowicz eT al, 2012" (missing a "t" in "et"). Done.
- Line 80 and elsewhere: Most usages of "which" I would personally have used "that" (or put a comma before the "which" and a comma to wrap the end of the secondary clause to denote the things between the commas as additional material supporting the principal clause). The author's call on whether to adjust accordingly. I have gone through the whole paper and checked every occurrence of "which" and changed a few to "that".
- current section 3: Referee 1 raises the point about "quasi-static" vs. "reversible", would probably be good to make the point here (since we are going reasonably technical already). Indeed. I should have addressed this in the first draft. It is discussed thoroughly in section 2 of the new manuscript.
- line 175: Comma after "FTR" to break up the sentence a bit. Thanks. Done.
- line 177: Comma after "change" to break up the sentence a bit. Thanks. Done.
- line 181: Comma after close bracket. OK. This sentence has been changed a bit now.
- line 183: I would remove the commas surrounding \mu but put comma after "represents" instead. Thanks. Done.
- line 185: "Fundamental Thermodynamic Relation" -> "FTR", since it's been defined already. OK. Done.
- line 193: Comma before "which". Done.
- line 194: More of a comment here since the important part of the statement is true. Landau & Lifshitz (1959) seems a bit of an old reference (and I don't agree it is the clearest text on the subject; David Tong's lecture notes on thermodynamics and/or statistical physics now turned into books spring to mind). I have changed the words "clearest text" to "classic text". Landau & Lifshitz (1959) is very mathematical, very exact, and I like that style. It is trustworthy, and every step is obvious. Of the older thermodynamic texts, very few are written for fluid flow (total derivatives, advection, source terms as flux convergence etc.), and Landau & Lifshitz (1959) was a watershed for fluid dynamicists interested in thermodynamics.

- line 197: Echoing referee 1's point, I suggested "quality" -> "standard" or just remove that adjective, since there are perfectly good quality fluids textbooks that don't deal with thermodynamics. Thanks. Done.
- line 211: "obtaining" -> "resulting in" Thanks. Done.
- line 215: I would do "...models; such equations..." since the two parts are sufficiently connected. The two sentences have been changed to address this point.
- line 235: "BE positive" feels more natural to me (because of the presence of "both" I think). Thanks. Done.
- paragraph including line 235: Echoing referee 1's point, can probably expand this a bit more with some additional linking equations. Thanks. I have now expanded this paragraph.
- line 269: Would add a comma after "flux". Thanks. This whole sentence has now been reorganised.
- line 283: Would add a comma after "quantity". Thanks. Done.
- line 289 and 306: "Leibniz" (no "t"). Many thanks. Done.
- line 292 and eq 16: I would have "Eq. (16):", then show equation, and then start the text again. Thanks. Done.
- eq 16: Echoing referee 1's point about definition of control volume and probably having a schematic. Thanks. I have addressed this issue of mixing over a finite volume with an extra paragraph at the end of section 3 and with an extra sub-section, 5.3. This was a probing question to address. The extra material in these two places in new, but as referee 1 suggested, the issue needed to be addressed in this review.
- line 307 and eq 17: As above, show the equation and then justify the steps, having "Eq. (17):", show equation, and then start the text again. Thanks. Done.
- text after eq. 17: (Geoff's comment) Useful to link to Fig 8. Thanks. Done.
- eq 18: Echoing all comments, define the meaning of hats here (bring/repeat some of the text from line 829 forward to here). Also needs a full stop to end the equation. Thanks. Done both these things.
- line 336: No "expression (R.02)" here. Thanks. Fixed this typo.
- line 345: Suggest comma before "while". Thanks. Done.
- line 355: Suggest comma after "correctly". Thanks. Done.
- line 365: Comma before "while". Thanks. Done.
- line 367: Common to replace the semi-colon. Thanks. Done.
- Section 6 title: Doesn't really talk or define temperature? If this section included section 7 then it's probably fine. Thanks for this. Yes, the definition of Conservative Temperature was missing. I have added material and renamed this section to be “Ocean heat content and Conservative Temperature”, and sections 5.1 and 5.2 together provide the motivation and definition of both ocean heat content and Conservative Temperature.
- line 417: Suggest comma before "and". Thanks. Done.
- line 436: Sentence "Because the four coefficients..." is redundant since it is covered by the sentence before already. Thanks. I have now deleted this sentence.
- line 439 to 441: Authors call on this; I just think this sentence is unnecessary. Thanks. I agree. This sentence has now been deleted.
- line 461: Instead of "t" (which is time), suggest \Delta T or \delta T but define it explicitly. I have now explained these different temperatures.

- line 475: Remove first two "nor"s, and "nor potential temperature" -> "or potential temperature", ending the sentence there (the bit after the semi-colon is unnecessary). Thanks. Done.
- line 491: Second "m_1" -> "m_2" Thanks. This typo has been corrected.
- line 492: I assume since m_1 + m_2 = m, so either

  m_1 = m_2 = 1/2 m => 1/2 m_1 m_2 / m^2 = 1/2 * (1/2 m)^2 / m^2 = (1/2)^3 = 1/8, or

  1/2 m_1^2 / (2 m_1)^2 = 1/2 * 1/4 = 1/8,

  so I think one of Geoff's comments can be ignored? This typo has been corrected.
- line 533: "on" -> "upon"? Thanks. Done.
- section 9.1: I thought the point about BGC affecting salt content and therefore density is interesting, may want to echo this at the end (see a later comment). Thanks. I do now make a much bigger point of this in the Summary section 14.
- line 726: Remove floating closed bracket. Thanks. Done.
- line 742: "2 1/2%" -> "2.5%". Thanks. Done.
- line 746: missing degree sign in "2C". Thanks. Done.
- line 756: Comma before "and", otherwise too many "and"s floating around. Thanks. I've fixed both issues by breaking the sentence into two sentences.
- line 760: Full stop replaced by colon since opening the list. Thanks. Done.
- line 763-764: Round brackets instead of square brackets probably. Thanks. I've now used round brackets.
- line 786: Comma before "a". Thanks. Done.
- line 798: Remove "of". Thanks. This typo has now been corrected.
- line 815: add "= 0" to in-line equation, and probably a comma as well. Thanks. Both things have been fixed.
- line 829: "...pressure \tilde{P}, T_b is the..., AND the over-hats AGAIN indicate that..." Thanks. Done.
- line 845: Just because it looks a little odd having a sentence start with a lower case letter, would suggest passive voice as "A variable called orthobaric density \rho_v, which is a function..., was introduced by de Szoeke et al (2000)." Thanks. Done.
- line 892: "neutral" density? Thanks. Done.
- line 924: Semi-colon to colon. Thanks. Done.
- line 932: Clarify and/or expand what is meant by "epineutral" (echoing Geoff's comment). Thanks. I have included the explanation in a bracket.
- 1st paragraph of sec 12: See Geoff's comment about this not being a "proof", and clarify accordingly, and some content in the paragraph of line 971. Thanks. I have now addressed this in the text, and in my response to Geoff Stanley.
- eq 47: Suggest comma instead and move the bracketed text before to after the equation and remove the brackets (state first, then define). Thanks. Done.
- line 1080: NSPV is already defined so can just use it, remove expanded text. Thanks. Done.
- line 1093: Remove brackets (to be consistent with two points before). Thanks. Done.
- eq 52: Needs a comma after the zero vector. Done.
- eq 53: Suggest full stop and starting next sentence with "Taking the epineutral gradient of this equation, WE find that..." Thanks. Done.

- eq 54: Need a comma and a full stop here. Thanks. Done.
- line 1106: "its" -> "the" Thanks. I've changed this sentence.
- line 1108: Can just use NSPV again without the expansion (or just don't use the acronym at all actually). OK. Done.
- eq 55: Need a comma and a full stop here. Thanks. Done.
- line 1113: No "Eq. (113)" here. Thanks. I've fixed this typo.
- line 1122: Can just use NSPV again without the expansion. Thanks. Done.
- eq 57: Needs a comma after the zero vector. Done.
- eq 59: Needs a comma after the zero vector. Done.
- line 1135: Can just use NSPV again without the expansion. Done.
- eq 61: Is "\kappa" defined? Might be good to redefine it as a reminder if so. Done.
- eq 65: As above, state equation, replace full stop with comma, and move the bracketed text to after the equation removing the square brackets. Thanks. Done.
- sentence of line 1177: Would suggest moving or repeating sentence further up as some sign-posting for the reader. Thanks. I have now placed a pointer about this at the very beginning of section 13.
- line 1200: Would add comma before brackets and remove the brackets. Yes, good, thanks, done.
- line 1225: Commas surrounding "which depends on the electrical conductivity of seawater" since this is the secondary clause. Thanks, done.
- line 1244-1245: "...very little research, and... as preliminary AND unfinished business". Yes, this wording was wrong. I've changed/softened this now.
- section 14: Echoing Geoff's point, I would have liked to see some key summaries and some call-to-action by providing/highlighting some research outlook directions (e.g. I thought the BGC model coupling to salinity was interesting) the author would like to see in due course. It makes the end a bit less abrupt, and I think that while a review a paper certainly looks to the past, some vision towards the future would be desirable. I have done this now in the final paragraphs of the Summary section 14.
- Figures: Doesn't need to go in the back even for the draft. OK, I will stay with the figures being at the back for now, as I remember reading this in the instructions to authors.
- Figure 2 and 3: If possible, please make the black dots bigger to be consistent with Figure 5. Sorry, I am unable to change this.
- Figure 4: We generally prefer authors to not use jet/rainbow colour schemes now. Would suggest a Red/Blue one (for positive and negative values) or similar. Sorry, I am unable to change this.
- Figure 12: Would prefer not jet/rainbow, but this might be more difficult to achieve. Sorry, I am unable to change this.