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

==========

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 $\tilde{h}_{S_A S_A}$ 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, $\hat{h}_{S_A S_A}$ 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 Θ 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 ∇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 $\ln\rho$'" 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 α and β, then this ratio would just be a constant; but isn't the linear combination that uses α_p and β_p totally different from that using α and β? Thanks. I think it’s OK. The heave affects Θ_z and S_z in both N^2 and $\left(\alpha_P\Theta_z - \beta_P S_{A_z}\right)$.

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 ω 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.