

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
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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 η 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,

$$dh - v dP = du + P dv = T d\eta + \mu dS_A. \quad (1)$$

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

$$K = \frac{1}{2} \mathbf{u} \cdot \mathbf{u} \quad (2)$$

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}^{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}_θ 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 “2½%” 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, Θ, 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, material 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 κ ? 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 κ 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 Thermostatistics*, 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.

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