Review of:

TEOS-10 and the climatic relevance of ocean-atmosphere interaction

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Summary and recommendation: The main aim of this paper is to present an overview of some of the main historical developments that led to the new TEOS-10 and discuss some of its applications to examine the potential relevance of irreversible evaporation and changes in the cloud condensation level of cumulus for understanding the anomalous ocean heat uptake associated with global warming. Overall, I found the paper a very interesting and stimulating read that should be eventually published. Before that, however, I have several concerns on several aspects of the paper that I think need to be addressed before the paper can be accepted for publication.

Major points

<u>Section 3.</u> Unlike the other sections, this section uses persuasive writing rather than scientific writing to convince the reader of the legitimate and rigorous character of the TEOS10 approach to defining ocean heat content. In essence, this amounts to providing a solution to a question that has not been properly formulated first; as result, the reader is not given the scientific elements necessary to assess the legitimacy of the author's assertions. Moreover, the topic is not properly reviewed or discussed in the context of past research on the issue. As a result, this section does not conform to accepted scientific standards, and therefore should either be significantly improved, or removed from the paper.

Yet, if one looks at the literature, it appears possible to identify the scientific question to be resolved. If one goes back to Bryan (1962), one realises that the problem of how to define heat was originally defined as the problem of how to separate the total energy transport into a dynamical and thermodynamic part. Mathematically, one general way of doing that is by writing the Bernoulli head $B = E_k + \Sigma$, the sum of kinetic energy Ek and static energy $\Sigma + \Phi$

$$E_k + \Sigma = E_k + \Sigma_{dyn} + \Sigma_{heat}$$

Now, the meridional transport of B through some latitude must be balanced by the sum of the wind power input and net heat flux between one pole and the latitude considered. Historically, Bryan (1962) appeared to have considered that the thermodynamic part of the static energy should be defined in terms of the non-elastic part of the internal energy, which is how the quantity $c_{p0}\theta$ is often interpreted (see discussion in Warren 1999). However, while the surface flux of internal energy is the net heat flux Q, this is only accurately the case for $c_{p0}\theta$. If one considers that the goal of the exercise is to define mechanical energy as the quantity absorbing the work transfer by the wind and the heat as the quantity absorbing the heat transfer, then agreed, potential enthalpy more accurately does so than $c_{p0}\theta$. However, if one considers that part of the heat transfer contributes to the production of mechanical energy (which is equivalent to say that the ocean heat engine has a non-zero thermodynamic efficiency, as predicted by the theory of available potential energy or Carnot heat engine theory, e.g., Tailleux (2010)), then clearly, potential enthalpy is less satisfactory, and can only be regarded as some kind of zero-thermodynamic-efficiency limit or approximation of heat, which should be explicitly stated. The physical basis for decomposing total energy into dynamical and thermodynamic components was recently discussed in Tailleux and Dubos (2024), part of it being rooted in the local theory of available potential energy of Tailleux (2018). To achieve consensus, what is still needed is to agree on the objective criteria that one should use to assess the relative merits of different viewpoints.

I believe that the above presentation is more satisfactory than the one given by the author because: 1) it clearly identifies the scientific question to be resolved; 2) it connects the problem to past and recent research on the topic, by re-situating it in the context in which it was originally developed; 3) it gives the reader the necessary scientific elements for assessing the relative merits of different viewpoints on the matter. In contrast, TEOS-10 or the author's section gives the impression that there is only a unique way to address the problem and that there is nothing left to be solved, when this is clearly not the case.

Minor points

<u>Line 65. Typically, present numerical climate models suffer from an "ocean heat budget closure problem" (Josey et al. 1999) and describe the m–2 m–2 ocean-atmosphere heat flux only to within uncertainties between 10 W and 30 W (Josey et al. 2013).</u>
I find this statement confusing because my understanding of the Josey et al papers relate to the `observational' closure problem arising from the technical difficulties of measuring the different heat fluxes component reliably enough and with the desired accuracy. The closure problem in numerical ocean models is a completely different thing. Numerical ocean models will in general exhibit drift depending on many different factors, such as model resolution, and various model errors. The author needs to review the literature more carefully to avoid confusing observational and modelling issues.

Lines 70-72. While that may be the case, countless climate projections have been published that reproduce ocean warming like that observed. Presumably, air-sea interactions in such simulations have been analysed. It would therefore be useful if the author could summarise the state of knowledge on the matter, including discussions of the nature of uncertainties, rather than just speculate on the matter.

Lines 78-80: it would be useful to the reader if the author could translate these numbers in terms of implied change in net evaporation or precipitation, assumes that the two balance on average. May be the author could also discuss the fact that global warming is expected to heat up land area faster than ocean area. As a result, this may decrease relative humidity, with a possible compensating effect over the ocean like the one suggested by the author.

Line 95. About modelling the global heat engine. I agree with the author that improved thermodynamic formulations are useful to that end. Note, however, that a key part of understanding the functioning of a heat engine is to identify the relative fraction of the heat transfer going into driving the dynamics (the thermodynamic efficiency) compared to that passively as heat. It seems to me that while TEOS10 is clearly a success in providing such improved formulations, it is unclear how it can claim to contribute to the understanding of the functioning of the ocean heat engine. Indeed, by assuming that all the heat transfer into the ocean goes into heat, with none contributing to the dynamics, TEOS10 implicitly assumes that that the thermodynamic efficiency of the ocean engine is zero, which is inconsistent with studies such as Tailleux (2010) and many others. Moreover, if atmospheric scientists had a way of defining atmospheric heat as proposed by TEOS-10 in terms of a variable absorbing all heat transfer, then this would also imply a zero thermodynamic efficiency for the atmospheric heat engine, which I am not sure would be very popular.

Figures 3 and 4. Shouldn't credit or copyright for the photo be indicated? Can these be re-used by others?

Lines 129-134. The question is whether the TEOS-10 definition of heat is as rigorous as the author claims, as the definition seems an ad-hoc one to me. TEOS-10 proposes a solution to a question that they never define in the first place. See my comments in the major points section.

Line 200-203. Can the author provide some explanation about why a Helmholtz potential is preferred in that case rather than a Gibbs function? The use of a Gibbs function as the basis for TEOS10 is generally understood from the fact that S, T, and p are variables that are the most easily measured/fixed in practice. We are also told that density is a variable that is very hard to measure in practice, which makes the usefulness of a Helmholtz function hard to understand. So, what are the physical arguments in favour of it?

Line 259-260. Preferred by whom? Many scientists consider that the choice of prognostic variable is a matter of personal preference and essentially subjective. Anybody trained as a physicist will prefer to use a variable that is as close as possible to measured or measurable quantities, which is what most physicists consider to be the best practice. Conservative Temperature may have some desirable features, but it has many undesirable ones as well, as it remains a non-measurable ad-hoc energy-like quantity that does not separate thermal from saline effects as well as potential temperature. It remains a puzzle to me why TEOS-10 found the need to legislate on an essentially subjective matter when potential temperature is clearly advantageous to Conservative Temperature in many important and fundamental ways. Atmospheric scientists retain absolute freedom in using whatever potential temperature variable they want, and many have been developed, the jury being still out on the relative merits of each one. Why should oceanographers have less freedom than atmospheric scientists to choose whatever they consider to be best for their own applications? In this regard, TEOS-10 feels very autocratic.

Line 358, Equation 6: Can you be more specific as to the form of the transfer coefficient Df(u) by providing examples from the literature? I am confused by the author's statement that such a coefficient only depends on u, because my understanding is that such a coefficient also depends on many other things, such as a sea surface roughness, nature of the boundary layer, and so on...

Lines 396-398. This sounds like an important result warranting further attention. However, can the author guarantee that Dq(u) does not depend indirectly on q in a way that would compensate the effect discussed? Change in q may modify the nature of the turbulent boundary layer and the transfer coefficient.

Lines 637. The author only discusses irreversibility associated with non-zero relative humidity under the assumption that the oceans and atmosphere have the same temperature. In reality, the latter may also have different temperatures. Can the author comment as to the implications that this would have for his theory?

Lines 723-725. My understanding is that the Zlcl is to be obtained by integrating the hydrostatic relationship, which can only lead to the author's formula (52) if the entropy and specific humidity are perfectly uniform from the surface to the bottom of the cloud. Is that really the case in reality?

Appendix

Lines 930-934. I am surprised to see the quantities $-pdV$ and $Td\eta$ equated with the work and heat transfers δW and δQ, because this is only true for reversible and quasi-static transfers. As far as I am aware, the exact relations are $T\ d\eta \geq \delta Q$ and $-p\ dV \leq \delta W$. This can be verified for an adiabatic expansion of a piston in a vacuum. In that case, $\delta Q = 0$ yet the entropy increase; moreover, $\delta W = 0$, yet V increases so that $-p\ dV < 0$. Moreover, note that p, V, T and eta relates to internal properties of the fluid, while the concepts of heat and work transfers relate to external properties describing the interactions of the fluid with its environment, so that it is dangerous and confusing to equate internal and external properties without further discussion.

Lines 962-963. I thought that this condition was also true in the presence of gravity. Can the author explain how gravity affects these conditions, given that this is obviously relevant to the oceanic case?

References

Bryan, K. (1962). Measurements of meridional heat transport by ocean currents. JGR, 67, 3403—3414.

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