Review of revised version of:

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

By Rainer Feistel

Summary and recommendation. I believe that the revised version of this manuscript is much improved and is now closer to be acceptable for publication. However, I believe that the justification for the manuscript, as well as elements of Section 3, still need to be substantially improved before the manuscript can be accepted for publication.

Specific comments

Lines 6-7 Abstract: 'Unpredicted observations [...] are challenging the numerical models' Not sure what that means or what this is based upon. Numerical climate models are generally able to predict both global atmospheric and oceanic warming, although this might be admittedly for the wrong reasons.

Lines 71-74. 'Typically present numerical climate models suffer from an "ocean heat budget problem"' As previously mentioned, I don't understand what that means. Numerical climate models offer an energetically consistent description of the climate system, in the sense that all the energy that comes in either comes out or is absorbed within the climate system. The term 'heat budget problem' suggests that models do not close their heat budget, but it seems to me that they do, so I have no clue what the author is actually talking about. sentence is especially confusing as the references cited relate to observations, not numerical models. The author should make an effort at more clearly articulating the nature of the problem, its origin, and how his proposed approach can help. The author should also carefully distinguish between numerical versus observational issues, errors versus uncertainties, because these distinctions are crucial to identify how to make progress.

More generally, I am quite puzzled by the way the author justifies his work, because climate projections using comprehensive climate models have been able to predict global atmospheric and oceanic warming, as documented by all IPCC reports. Moreover, CMIP have made the outputs of numerous simulations openly available to the community, meaning that it is possible to diagnose from such simulations which processes are responsible for ocean warming in such simulations. A priori, such warming can come from: 1) increased incoming shortwave radiation due to cloud cover redistribution; 2) reduced latent heat release in regions of deep water formation, resulting in less deep water formation; 3) reduced latent heat release; 4) increased incoming long wave radiation due to changes in cloud and cloud cover; 5) increased sensible heat flux. It would be clearer if the author would review the possible different mechanisms by which the ocean can warm, state that this is realistic remains uncertain, and focus on the aspects that potentially can be clarified using rigorous thermodynamic arguments as a way to identify possible shortcomings in coupled climate models.

Section 3 I continue to believe that the approach developed by the author to justify the TEOS-10 definition of heat is heuristic rather than deductive and therefore scientifically unsatisfactory. Of course, heuristics are very common in science, so that this can be perfectly acceptable if this is explicitly acknowledged. My main concern is that this is not really the case here, and that the author present potential enthalpy as more rigorous than it is, without acknowledging the limitations of the approach. I think that the author should try to write this section more objectively and in a more balanced way before the paper can be accepted for publication. As far as I know, potential enthalpy does not solve the problem and should not be presented as if it did. The author should try to identify and discuss what remains to be done to achieve a more satisfactory solution. The following lists a number of places where the discussion can be improved.

Lines 294-296: 'The obsolete hypothesis of heat being a substance is excluded' I think this phrasing is potentially confusing to readers, because 'obsolete' is often understood (especially in its US meaning) as being superseded by something new, but this is not the case, isn't it? As far as I am aware., classical thermodynamics does not define 'heat' as an internal property of the system, but rather tries to limit the use of ` heat' as a mode of heat transfer (Romer, 2001, 'Heat is not a noun', Amer. J. Phys., https://doi.org/10.1119/1.1341254), which I think is what the author is trying to convey in this section. For this reason, I think it would be clearer to say that the view of heat as a substance promoted by the calorimetric theory of heat has been debunked, rather than it is obsolete (even nicer would be to explain the scientific basis for its refutation, as I must admit I never fully understood the arguments). Here, I think it would be helpful to reader if the author could point out that the only accepted sub-forms of energy that appears to be well accepted in classical thermodynamics is the partition of total energy into 'useful' and 'useless' forms of energy, which thermodynamicists refer to as 'exergy' versus 'anergy', or 'free energy' versus 'dead energy', with a review of existing terminology comprehensively reviewed by (Marquet, 1991, on the concept of exergy and available enthalpy: application to atmospheric energetics https://doi.org/10.1002/qj.49711749903 I bring this up, because it seems to me that the concept of 'OHC' used by oceanographers is the counterpart of the concepts of 'anergy' or 'dead energy' of classical thermodynamics. I think that this is relevant because oceanographers often regard 'heat' as the dynamically inactive part of the total energy that is passively transported poleward to remove the excess of energy imparted to the equatorial regions. For instance, Young (2010) and Nycander (2010) both pointed out that defining ` heat' in terms of potential enthalpy implies that the useful part of potential energy should be defined in terms of dynamic enthalpy/effective potential energy. I therefore think that discussing this point would potentially greatly enhance the scientific value of Section 3.

Lines 315-316 `However, this OHC definition has no rigorous thermodynamic justification' I am not sure that I agree with this statement, because if you re-read Bryan (1962) and the ensuing literature, it is apparent that OHC was introduced as a way to separate the total energy transport into a dynamical and thermodynamic component, the latter being assumed to be represented by the non-mechanical energy part of internal energy. The idea was plausible at the time, because kinetic energy and gravitational potential energy have been traditionally assumed as mechanical forms of energy. On this basis, it seemed logical to assume the thermodynamic component of total energy to be related to the non-mechanical part of internal energy, which cp0 theta is meant to approximate. The phrasing suggests that the thermodynamic justification of potential enthalpy is more rigorous, but this is not really the impression one gets from McDougall (2003). Indeed, McDougall redefines the problem of defining heat as the problem of heuristically manipulating one of the expressions for the first law of thermodynamics into an equation for a thermodynamic variable that is as conservative as feasible and whose surface flux matches the neat surface heat flux. Clearly, this way of approaching the problem admits several solutions, so cannot define the concept uniquely. For this reason, I think that it would be more accurate to say that potential enthalpy has a clearer and more transparent thermodynamic justification rather than rigorous, because scientifically, McDougall (2003) does not qualify as ` rigorous' since it is essentially heuristic in its approach. For instance, it does not justify why heat should be defined as a quasi-material function of specific entropy and salinity. Note here that atmosphericists study heat transport in terms of the transport of (moist) static energy, which is also accurately conservative, and whose boundary fluxes coincide exactly with the boundary heat transfer. It also does not discuss the limitations of the approach, or what potential enthalpy is supposed to approximate. For this reason, it would be useful if the author could clarify these or at least comment on these points.

Lines 332-333 – The work required to lift and lower the parcel is balanced. I don't understand this because once the temperature (and salinity) of the parcel lifted to the surface has been modified, the work needed to lower it back to its original position will in general be different than that necessary to lift it up. I therefore don't understand what the author means by it is 'balanced'. Please explain.

Lines 334-336 – The reference state relative to which OHC is measured is arbitrary [...] I don't think that this is generally true. I believe that this is true only for systems whose mass does not change with time but not for systems for which mass changes with time, see Lang et al. (2018) 'poleward energy transport: is the standard definition physically relevant at all time scales? https://doi.org/10.1007/s00382-017-3722-x The latter study suggests that if the mass changes, the relevant reference state should be related to the global mean value of the system.