RF: Thanks for careful reading and detailed discussing.

1. **OHC**

Review 2: „Section 3. 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.

… 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 …

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."

RF: Entropy (here, "*N*") was originally discovered and defined by Clausius describing heat exchange in the form of dN = dQ/T. Using Clausius entropy, a problem of defining heat does not exist: entropy is formally defined for the first time in terms of heat whatever "heat" may actually be. Meanwhile, there are various alternative definitions of entropies in the subsequent literature, more or less related to heat, but here empirical Clausius entropy is used in the same sense as by numerous textbooks from Gibbs to Prigogine.

Section 3 explains OHC in terms of surface entropy flux. This is no balance of the total ocean energy, nor is it a description of the real heat exchange between ocean and atmosphere. Section 3 proposes a fictitious thermodynamic process by which proper heat exchange is formally related to thermodynamic state properties.

Definitions are neither right nor wrong per se; they may be more or less useful for a certain purpose. OHC is a matter of definition. The main intention of Section 3 is placing emphasis on the physical arbitrariness of any OHC definition. It is up to the oceanographic community to adopt one option as a standard to ensure comparability of reported figures. Note that Section 3 had already substantially been modified in response to Reviewer 1.

As a measurable physical quantity, "heat" is defined only as a heat exchange between two bodies rather than any "heat substance" contained in a volume. It is clearly said that the common term "OHC" is thermodynamically sloppy and ambiguous; Section 3 suggests one option of defining a heat flux and a reference state consistent with the TEOS-10 OHC definition given by McDougall et al. (2021). This simple option does not require any details of complex energy transformation processes within the real ocean. However, a sentence hinting on more complex analyses of the OHC problem has been added to Section 3:

"OHC as a part of the total energy balance of the ocean is analysed by Tailleux (2010, 2018) and Tailleux and Dubos (2024)".

2. **Line 65.**

Review 2: "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).

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."

RF: I do not see the need for such a distinction in the context of my paper. Numerical models can hardly describe ocean surface heat fluxes more precisely than observations by which they were tuned. If those models have numerical problems even larger than 10 W and 30 W per m-2, they lack significance in explaining the discussed effects of 1 W per m-2.

3. **Lines 70-72.**

Review 2: "… 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."

RF: I myself do neither run nor assess climate models or similar products; I just report published statements of renowned experts. All analyses I am aware of do conclude that the uncertainty in estimating the global mean air-sea heat flux is hardly any better than 10 W m-2, if at all. Contributions to this uncertainty are various and complicated, and are certainly not the topic of this paper. What does matter here, however, is the plausible conclusion that a model with uncertainty larger than 10 W m-2, see e.g. Fig. 5.10 in Josey et al. (2013), cannot reliably distinguish and explain effects of the magnitude of 1 W m-2. Attempts to blindly do this may well be considered as speculative.

"Despite a certain success it must still be stated that the modellers among my colleagues are not yet aware of the severity of the problem. As a rule, they work with energy-balance equations whose coefficients had been verified by measurements, while their latent-heat fluxes are mostly determined as the remaining left-over term, which therefore received the residual as an additive. This has implications for the surface temperature that is important for most of the models, and in particular for the water-vapour flux" (Daniela Kracher et al. 2009, doi:10.1127/0941-2948/2009/0412). "The global water cycle and the exchange of freshwater between the atmosphere and ocean is poorly understood" (Penny Holliday et al., 2011). "For most products, it is not possible to close the [global ocean] heat budget to within 10 W m-2 and in some cases the bias is of the order of 30 W m-2" (Simon Josey et al. 2013: p. 128). "The drivers of a larger Earth energy imbalance in the 2000s than [before] are still unclear. … Future studies are needed to further explain the drivers" (Karina von Schuckmann 2023). "Climate models struggle to explain why planetary temperatures spiked suddenly. … No year has confounded climate scientists' predictive capabilities more than 2023" (Gavin Schmidt, Nature, 21 March 2024).

4. **Lines 78-80.**

Review 2: "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."

RF: A global mean evaporation / precipitation of about 1000 mm corresponds to an oceanic latent heat flux of roughly 100 W m-2. RH uncertainty of 1 – 5 %rh corresponds to 5 – 25 W m-2 latent heat flux uncertainty, or 50 - 250 mm of annual precipitation. Related text inserted:

“Unfortunately, marine RH is observed only with uncertainties between 1 and 5 %rh (Lovell-Smith et al. 2016), or, accordingly, between 5 and 25 W m–2 of latent heat flux, which is roughly corresponding to unknown variations ranging up to 50 … 250 mm evaporation.”

Review 2: “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.”

RF: Such a compensation effect is speculative. Dominating 85% of global evaporation occur at the ocean. Global warming on land is rather different from that at sea, and does not belong to “Ocean-Atmosphere Interaction” of this paper. For details see e.g.: Blunden, J., Boyer, T., and Bartow-Gillies, E. (eds.): State of the Climate in 2022, Bull. Amer. Meteor. Soc. 104, S60–S61, https://doi.org/10.1175/2023BAMSStateoftheClimate.1, 2023

5. **Line 95.**

Review 2: “…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… “

RF: TEOS-10 does not describe the “heat engine” dynamics of the climate system; TEOS-10 only provides the most accurate, comprehensive and mutually consistent thermodynamic tools for use in climate research. This paper explains that and just offers some simple tutorial examples for the use of TEOS-10 in the climate context.

6. **Figures 3 and 4.**

Review 2: “Shouldn’t credit or copyright for the photo be indicated? Can these be re-used by others?”

RF: Ocean Science is publishing under the Creative Commons Attribution 4.0 License.

7. **Lines 129-134.**

Review 2: “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.”

RF: Neither TEOS-10 nor this paper have ever attempted to define “heat”. Clausius’ original definition of entropy is in terms of heat exchange. “We have … two of the fundamental ideas of the science of heat – the idea of temperature … and the idea of heat as a measurable quantity, which may be transferred from hotter bodies to colder one” (James Clerk Maxwell 1888, Theory of Heat, Longmans & Green: p. 9). “The variation in entropy during an infinitesimal reversible transformation is obtained by dividing the amount of heat absorbed by the system by the temperature of the system” (Enrico Fermi 1937, Thermodynamics. Prentice-Hall: p. 52).

8. **Line 200-203**.

Review 2: “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?”

RF: In statistical physics, a canonical ensemble with the partition function Z determines the Helmholtz energy by $F = - kT \ln Z$. The function Z depends on the particle number N, the temperature T and the volume V, and is a functional of the microscopic particle interaction energy, entirely independently of the macroscopic phase the substance may actually take. For a given substance or mixture, this formula $F(N, T, V)$ is single-valued and universally valid, be it a gas, a liquid, or any solid phase, such as for water around its critical point and for each of the various ice phases. By contrast to F, the Gibbs energy $G(N, T, p)$ is multi-valued in the T-p vicinity of phase transitions, where each phase is represented by its own separate "leaf" of G that intersects the leaf of the other phase. TEOS-10 includes a single Helmholtz function jointly for liquid water and water vapour, but two different Gibbs functions. References to related textbooks introducing that matter have been added to this paper:

"For theoretical reasons (namely, the statistical so-called *canonical ensemble*, Landau and Lifschitz 1966: §31; Kittel 1969: Ch. 18), …"

9. **Line 259-260.**

Review 2: Conservative Temperature "Preferred by whom?"

RF: Preferred by ocean modellers who started using TEOS-10, see e.g. Almeida et al. (2018), as far as I know this. See also Young (2010), https://doi.org/10.1175/2009JPO4294.1; https://en.wikipedia.org/wiki/Conservative_temperature; or Pawlowicz, R. (2013) Key Physical Variables in the Ocean: Temperature, Salinity, and Density. Nature Education Knowledge 4(4):13

10. **Line 358, Equation 6:**

Review 2: "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…"

RF: In fact there is a wealth of different definitions of the transfer coefficient Dq(u) of latent heat in oceanography, meteorology and hydrology. In this paper reference is only made to the recent definitions given by Josey (2013: eq. 5.1), Stewart (2008: eq. 5.10c) or Pinker et al. (2014, doi:10.1002/2013JC009386: eq. 1) who parameterise this coefficient simply as a linear function of wind speed, independent of the various other surface properties. To my knowledge, there is no suggestion available yet from the literature for the functional form of Df(u).

11. **Lines 396-398.**

Review 2: "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."

RF: Available from TEOS-10 for the first time, relative fugacity is the proper irreversible thermodynamic driving force for the water transport across the air-sea interface. Properties of the turbulent boundary layer may certainly depend on additional properties beyond those provided by TEOS-10.

12. **Lines 637.**

Review 2: "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?"

RF: The thermal "skin effect" of the air-sea interface has been studied by several authors such as Peter Saunders (1967) or Kristina Katsaros (1980). The sensible heat flux affected by this effect is generally small compared to the latent heat flux. In the sense of Onsager linear irreversible thermodynamics, the heat flux driven by the temperature difference, and the evaporation flux driven by the different chemical potentials (that is, relative fugacity) will also have a cross effect of evaporation driven by the temperature gradient and, symmetrically, of sensible heat flux driven by the relative fugacity. However, the magnitude of the cross effect is unclear and has so far been assumed to be negligible.

13. **Lines 723-725.**

Review 2: "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?"

RF: In the LCL model presented here, it is assumed that the uplift of air occurs at constant entropy and specific humidity in order to compute the LCL pressure. The same assumption is also applied for computing the LCL height. Of course, this is an idealised model of reality. Figure 20 in Feistel et al. (2010a) shows measured radiosonde profiles of those quantities with an approximately isentropic surface layer over the tropical Atlantic.

14. **Lines 930-934.**

Review 2: "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, δQ=0 yet the entropy increase; moreover, δW=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."

RF: It is true that TEOS-10 describes equilibrium thermodynamics of seawater, ice and humid air. In classical thermodynamics all exchange processes are idealised as reversible and quasi-static. For application of TEOS-10 to geophysical processes, TEOS-10 may be generalised under the assumption of local equilibrium, see e.g. Feistel and Hellmuth (2024a) and the discussion of entropy production in Section 5.3 of this paper.

The discussion on OHC in this paper is explicitly focussed on the fact that heat is an exchange quantity rather than a state quantity. "We have … a right to speak of heat as a *measurable quantity*, … however, … we have no right to treat heat as a *substance*" (J. C. Maxwell, 1888, Theory of Heat, p. 7).

As an aside, the gas expansion into vacuum violates the condition of local equilibrium (namely, the existence of a local Maxwell distribution of particle velocities) so that neither temperature nor entropy may properly be defined in that case.

15. **Lines 962-963.**

Review 2: "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?"

RF: Under gravity, the equilibrium condition of equal (molar) chemical potentials μ is replaced by the condition that for each species the form ($\mu + M\,\phi$) must take equal values across a volume, where M is the molar mass and ϕ is the gravity potential (Guggenheim 1949: chapter XI).