

RF: Thanks for indicating the various minor issues. They have been fixed.

1. Recent warming 2023

Review 1: „There is an emphasis in the Abstract and Introduction on the alarming and unexplained global warming of 2023-2024. In the context of an academic review paper on ocean thermodynamics I think that this might be overdone and might make the review seem a bit dated when read in say 10 years' time. I recommend playing this down somewhat by delaying its discussion so that, while this issue is still there to gain the reader's attention, it does not distract from what is essentially a scholarly work.”

RF: The 2023 warming is taken as a current example for the urgency and importance of the questions raised. This “teaser” is presented as just a single quotation. To make this more obvious, the section has been split into 2 paragraphs:

“...the reported ocean's average warming rate amounts to 1.3 W m^{-2} , and is apparently even increasing.

The currently observed *ocean heat content* (OHC) represents a merely transient maximum after a decade-long systematic warming process in the past, see Fig. 18 in **Section 6**, which may proceed to even higher values in the future. In **Section 3**, thermodynamic aspects of related OHC definitions will be considered. Regarding the long-term period since 1971, “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 of this change” (von Schuckmann et al. 2023; p. 1694). Laterally, the observed heat excess is unevenly distributed over the world ocean (Fig. 1), in contrast to what naively may be expected from rising atmospheric CO_2 concentrations. Rather, warming seems to be most pronounced in the austral and boreal west-wind belts. Selected thermodynamic relations between OHC and cloudiness are briefly discussed in **Section 6**.”

2. OHC

Review 1: „I identify one small part of this manuscript that I think is wrong and should be deleted, namely the part around Eq. (5).”

RF: I have described my intention behind eq. (5) in more detail now:

“The process depicted in Fig. 6 measures the total heat flux $\int dh = \int Td\eta$ which changes the entropy of the given sample from the current value, η , to some arbitrary reference value, η_{ref} , and this way, the process also changes the parcel's enthalpy from $h^{\text{SW}}(S, \eta, p_0)$ to $h^{\text{SW}}(S, \eta_{\text{ref}}, p_0)$. Integration over all ocean samples results in an OHC value of

$$\text{OHC}^* = \int [h^{\text{SW}}(S, \eta, p_0) - h^{\text{SW}}(S, \eta_{\text{ref}}, p_0)] \rho^{\text{SW}}(S, \eta, p) dV. \quad (5)$$

While the choice of the OHC reference state is - in principle - entirely arbitrary, such as simply putting $\eta_{\text{ref}} = 0$, it is reasonable to better adapt this selection to the purpose of the OHC definition. The main purpose of estimating OHC is keeping track of the ocean's long-term energy balance, in particular of the ocean's share of global warming. Three conditions appear immediately plausible in order to achieve this goal,

- (i) *The OHC definition should ensure that OHC differences represent a suitable spatial integral over the heat fluxes crossing the ocean's boundaries. As discussed in more detail in Section 5.3, production of entropy, $d_i\eta$, caused by irreversible processes between different parcels within the ocean, does not affect the ocean's total enthalpy budget. This is quite in contrast to entropy exchange, $d_e\eta$, of the given sample in the form of*

reversible heat flux across its boundary. Such irreversible processes affect the ocean's total potential enthalpy much less than its total entropy (McDougall et al. 2021). For this reason the OHC reference state should explicitly be defined in terms of potential enthalpy, $h^{SW}(S, \eta_{ref}, p_0)$, and this way only implicitly in terms of entropy by specifying $\eta_{ref}(S)$.

- (ii) *Provided that the ocean's mass remains the same between any two ocean states (1) and (2), the difference OHC(1) – OHC(2) should depend only on the surface heat flux balance during the time in between.* For this reason, the OHC reference value should be independent of changes occurring in the density distribution, $\rho^{SW}(S, \eta, p)$. This can be achieved by assigning to each ocean parcel the same reference potential enthalpy, $h^{SW}(S, \eta_{ref}, p_0) = \text{const}$, even though such a state may hardly ever be observed in the real ocean.
- (iii) *Quantitatively, OHC values estimated at different times or places should be mutually comparable without estimation bias resulting from possibly changing methods of OHC calculation.* For this reason, resulting OHC values should be independent of the inevitable arbitrary, physically irrelevant reference-state conditions imposed on energy and entropy, such as eqs. (1)-(3). This can be achieved by assigning to each ocean parcel the same standard-ocean enthalpy as its reference potential enthalpy, $h^{SW}(S, \eta_{ref}, p_0) = h_{SO}$. In the special case of TEOS-10 enthalpy, this value is defined by eq. (2), $h_{SO} = 0$. This choice is implicitly made by the definition (4) but needed to be considered explicitly as soon as alternative equations for seawater enthalpy or entropy are employed, such as those of Millero and Leung (1976) and Millero (1982, 1983)."

3. Bible quotation

Review 1: "I discourage the inclusion of religion in scientific papers. First, the bible is not a scientific book, nor is it scientifically correct since its discussion of the arrival of humans on earth (in its first chapter) contradicts the known science of evolution. Second, scientific papers should be able to be read by authors of all religions without them encountering quotes which somehow endorses the basic textbook of one religion. Hence, I think that biblical quotes, just like quotes from the textbooks of any religion, should not be allowed in Ocean Science. Please delete these lines."

RF: I agree that religious arguments should be excluded from scientific papers. However, more than 2000 years ago, there was no science apart from religion; modern science and modern religion have common roots. They diverged when religious branches turned into frozen dogmatic prescriptions to be used as instruments of political power, while science remained open for change, correction and evolution. Regardless of that, both religion and science still are – even if very distinct – mental models for the structure and the causal functioning of the perceived world.

With respect to the hydrological cycle, the Bible quotations in the paper are the oldest documented observations of nature that I could find. Similar other "holy books", such as the *Popol Vuh* of the Maya, or the *Teaching of Buddha*, are almost exclusively focused on human life and history, rather than observations of natural phenomena (although the scientific term *hurricane* is borrowed from the Maya god *Huracan*). People of the past had noticed that all rivers discharge into the sea whose level did not rise though, and that clouds may release vast amounts of water while floating virtually weightlessly across the sky. They had no plausible causal explanations for such mysteries and credited those to divine intervention.

The history of understanding the hydrological cycle has ancient roots and is, remarkably, not finished yet. This paper addresses relevant pending problems of modelling that cycle, and in so far it seems scientifically appropriate to refer to the exceptionally few written cases of revealing the poor very beginning of this understanding.

4. SMOW

Review 1: “Surely the water which IAPWS-95 describes is not Standard Mean Ocean Seawater”

RF: IAPWS-95 describes IAEA Standard Mean Ocean Water (SMOW) which is the solvent of IAPSO Standard Seawater.

5. Absolute Salinity

Review 1: “Line 201 and in hundreds of places throughout the paper, Absolute Salinity and Conservative Temperature are used without their upper-case letters. This goes against what IOC et al. (2010) and Valladares, J., Fennel, W., and Morozov, E.G (2011) and Spall et al (2013) [see below] dictate. I think the field should stick with the upper-case letters, simply because there are many different possible definitions of absolute salinity and of conservative temperature, but there is only one definition (each) of Absolute Salinity and Conservative Temperature.”

RF: In section 2, the paragraph introducing salinity has been edited to read:

“... The variable S , at which a subscript A is omitted here for simplicity, is the specific or **Absolute Salinity**, the mass fraction of dissolved salt in seawater, which differs from **Practical Salinity**, S_P , measured by **present-day** oceanographic instruments, **as well as from various other obsolete salinity scales** (Millero et al. 2008). **Throughout this paper, the term “salinity” is short hand exclusively for TEOS-10 Absolute Salinity. ...**”

Lower-case conservative temperature appears only once in the text; fixed.

6. Internal energy E or U

Review 1: “Replace E with U in this and subsequent equations. IOC et al (2010) has used U and u for internal energy (extensive and intensive), and this review paper should do the same.”

RF: The use of E rather U has now been justified below eq. (B.1):

“Note that IOC et al. (2010) uses the symbol U for the *internal energy* rather than E in eq. (B.1). This replacement is done here for denoting with u the wind speed, eq. (6), rather than specific internal energy, which is defined here by $e = E/m$, eqs. (1) and (B.3). The symbols E and e are frequently used for internal energy in the thermodynamic literature, for example by Gibbs (1873a) or Landau and Lifschitz (1966).”

Appendix B is just about thermodynamics in general without special emphasis on oceanography.

7. Minus sign of eq. (B.10)

Review 1: “equation (B.10) has a sign error. The last term should be added, not subtracted.”

RF: Consider the seawater case $n = 2$, $w_0 = 1 - S$, $w_1 = S$. We get from (B.10)

$$\mu_S = \mu_1 = g + \left(\frac{\partial g}{\partial w_1}\right)_{T,p} - \sum_{j=1}^1 w_j \left(\frac{\partial g}{\partial w_j}\right)_{T,p} = g + (1 - S) \left(\frac{\partial g}{\partial S}\right)_{T,p}$$

in agreement with eq. (2.9.5) of the Manual. No sign error.

Despite this, for clarity, the sentence above (B.10) has been changed to

“Because the Gibbs function depends only on the independent intensive variables, $g(w_1, \dots, w_{n-1}, T, p)$, the solutes’ chemical potentials, $i > 0$, are”

8. Partial derivative of (B.11)

Review 1: “The things that are held constant during the differentiation in the last term in this equation are not correct. They should be the same as the corresponding term in the next equation. Eq. (B.11).”

RF: Yes, but (B.11) is to be adjusted to (B.10). Corrected to

$$\mu_0 = \left(\frac{\partial G}{\partial m_0} \right)_{T,p,m_j>0} = \left(\frac{\partial(m.g)}{\partial m_0} \right)_{T,p,m_j>0} = g - \sum_{j=1}^{n-1} w_j \left(\frac{\partial g}{\partial w_j} \right)_{T,p,w_k \neq j} \quad (\text{B.11})$$