Point by point response to reviewer comments – round #2

Reviewer 1: Submitted on 12 Aug 2024

R1: The revised manuscript is ready for publication, in my opinion. There is just one sentence that I think should be deleted. It is at lines 332-333 of the revised manuscript where it says "The work required to lift and lower the parcel is balanced." This is not correct. The raising happens at one specific volume and the lowering at another, so the two works are different. I suggest deleting the sentence.

RF: It is important to understand here that the fictitious excursion of the parcel to the surface and back must not alter the energy balance of the ocean. For this reason the parcel's heat exported reversibly at the surface is imported reversibly again so that the parcel is returned to its original state before it is lowered back to its depth. After the parcels excursion for the purpose of heat exchange and heat measurement, the ocean's state is considered to be exactly the same as before. The ocean should not gain or lose any energy as a result of the excursion. I have tried to clarify this:

"The work required to lift and lower the parcel is balanced because the parcel's thermodynamic state is exactly the same before and after the balanced reversible heat exchange across the surface. The "heat content" defined this way for a single parcel is added up then over all ocean parcels to result in its total OHC value."

R1: One of the reviewers was concerned with the arbitrary nature of the reference states of TEOS-10. This has been addressed in many previous publications, and the additional text between lines 243 and 254 reviews this material for the benefit of that reviewer and for the general reader.

RF: Thanks for this support. I can only hope that these arguments make those things clear enough.

Reviewer 2: Submitted on 01 Sep 2024

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

R2: 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.

RF: I cannot add quotations to the Abstract, but I can add a couple here to explain what I mean and upon which statements it is based. I trust in the correctness of such expert statements (the first quotation has been added to the text):

„Climate models struggle to explain why planetary temperatures spiked suddenly. … No year has confounded climate scientists' predictive capabilities more than 2023. … This sudden heat spike greatly exceeds predictions made by statistical climate models."
[Schmidt, G. (2024): Climate models can't explain 2023's huge heat anomaly - we could be in uncharted territory. Nature 627, 467. https://doi.org/10.1038/d41586-024-00816-z]

"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."

[von Schuckmann, K. et al. (2023): Heat stored in the Earth system 1960–2020: where does the energy go? Earth System Science Data 15, 1675–1709. https://doi.org/10.5194/essd-15-1675-2023]

R2: 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.

RF: The phrase "ocean heat budget problem" is a literal quotation from Josey et al. (1999, 2013), as cited in my text. There, I find statements like "the ocean heat budget closure problem that remains a major unresolved issue in the field" (2013: p. 115). Please find detailed explanations there. I do not run such models and do only rely on such published assessments. It is not the task of my paper to go into those details reported elsewhere.

R2: 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.

RF: If the ocean surface heat budget is uncertain to 10 – 30 W/m², and unclear flux anomalies amount to about 1 W/m², this does not seem to me a proper "closure of the heat budget problem".

To my knowledge, ALL numerical circulation models fail to strictly obey energy conservation:

"Unfortunately, it is not always possible to maintain the exact conservation laws and symmetries in the [discretized model] equations."
[Griffies, S.M., Adcroft, A.J. (2008): Formulating the Equations of Ocean Models. In: Hecht, M.W., Hasumi, H. (eds.): Ocean Modeling in an Eddying Regime, Geophysical Monograph Series 177, American Geophysical Union, pp. 281-317. doi:10.1029/177GM18]

Certainly, TEOS-10 can and will not resolve any such problems, but it may improve model details such as parameterizations of the evaporation flux by using chemical potentials of water rather than specific humidities, as current models still do following Dalton. This is the aim of my paper.

R2: 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.

RF: Again, I do not run any climate models, I only refer to publications of climate modellers.

„Climate models struggle to explain why planetary temperatures spiked suddenly. … No year has confounded climate scientists' predictive capabilities more than 2023."
[Schmidt, G. (2024): Climate models can't explain 2023's huge heat anomaly - we could be in uncharted territory. Nature 627, 467. https://doi.org/10.1038/d41586-024-00816-z]

R2: 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.

RF: Again, analysing deficiencies of climate models and suggesting specific remedies is far beyond the scope of this paper. I only refer to such analyses published by experts:

"Most CMIP6 models fail to provide as much heat into the ocean as observed."
[Weller, R.A.; Lukas, R.; Potemra, J.; Plueddemann, A.J.; Fairall, C.; Bigorre, S. Ocean Reference Stations: Long-Term, Open-Ocean Observations of Surface Meteorology and Air–Sea Fluxes Are Essential Benchmarks. Cover. Bull. Am. Meteorol. Soc. 2022, 103, E1968–E1990. https://doi.org/10.1175/BAMS-D-21-0084.1. p. E1968]

"The drivers of a larger EEI in the 2000s than in the long-term period since 1971 are still unclear, and several mechanisms are discussed in literature. For example, Loeb et al. (2021) argue for a decreased reflection of energy back into space by clouds (including aerosol cloud interactions) and sea ice and increases in well-mixed greenhouse gases (GHG) and water vapor to account for this increase in EEI. Kramer et al. (2021) refer to a combination of rising concentrations of well-mixed GHG and recent reductions in aerosol emissions to be accounting for the increase, and Liu et al. (2020) address changes in surface heat flux together with planetary heat redistribution and changes in ocean heat storage."
[Von Schuckmann, K. et al. (2023): Heat stored in the Earth system 1960–2020: Where does the energy go? Earth Syst. Sci. Data 15, 1675–1709, https://doi.org/10.5194/essd-15-1675-2023]

I am just recommending TEOS-10 to be used in climate models and provide some tutorial examples for doing this, but the ultimate net improvement from using TEOS-10 can only be concluded from future practical implementations in global models which are missing yet, unfortunately, even a decade after the TEOS-10 adoption by IUGG.

R2: 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.

RF: There is no "TEOS-10 definition of heat" in this paper. Rather, the reader is reminded of the conventional definition of heat by Clausius, Maxwell and others, and the century-old textbook knowledge that there does not exist any state quantity exactly representing "heat". After a cyclic process, when a system has returned to its previous state, all state quantities take the same values as before, but the heat exchange does not need to be balanced during that cycle.

Heat is a measurable exchange quantity rather than a state quantity (Maxwell 1888). However, there are state quantities whose difference matches the particular heat exchange associated with a specific process. Different heat exchange processes may require different such state quantities which in that case represent the related quantity of transferred heat.

McDougall et al. (2021) proposed the state quantity “potential enthalpy” as a measure of the ocean heat content. This constitutes an exact measure of “heat” only if it is associated with a suitably designed heat exchange process. Here, such a conceptual process is proposed. This attempt does not need to present the only suitable process. Defining such a process along with the OHC formula would give the OHC definition an improved thermodynamic justification. This “remains to be done to achieve a more satisfactory solution”.

Advantages and deficiencies of the use of potential enthalpy are in detail discussed in the TEOS-10 Manual and several related articles. There is no need to repeat these aspects in the current paper.

R2: 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 convein 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.

RF: James Clarke Maxwell and Arnold Sommerfeld are renowned experts in thermodynamics, and I have included their literal quotations for their rigour. To understand why heat “is not a substance” (an historical obsolete idea known as caloricum or phlogiston) it is sufficient to read the explanations of Clausius, Maxwell or many other textbook authors. The main argument is that after an excursion, a system can precisely return to its former state while having consumed (or lost) a non-zero amount of heat during that cycle. Any “substance”, however, had to return to its original amount, gaining zero during a cycle. All I need and want to do here is to hint the reader on classical thermodynamic textbooks. There is nothing new to say.

Thanks for the Romer reference; I have added the Romer quotation: “Heat is not a substance! More formally: Heat is not a thermodynamic function of state” (Romer 2001: p. 107). Readers in doubt may look it up.

I do not see the need here to review the various forms of energy introduced in geophysics for special purposes. I only want to draw attention to the relation between OHC and proper thermodynamic "heat", appreciating novel definitions that have become available through TEOS-10.

R2: 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.

RF: To be clearer, this phrase has been changed to:

"However, in representing a kind of "heat substance", this OHC definition has no rigorous thermodynamic justification"

R2: 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 conceptuniquely. 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.

RF: From my perspective, McDougall has thoroughly explained his motivation to define potential enthalpy, and has done a lot of mathematics to derive or estimate the properties of this quantity. Definitions are a matter of usefulness, not a matter of right or wrong. My only task in this context is an attempt to relate the (path-independent) state quantity "potential enthalpy" to the conventional thermodynamic (path-dependent) exchange quantity "heat". The OHC part of this paper goes already into the relation between heat and potential enthalpy.

R2: 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.

RF: The lifting and lowering of the parcel is assumed to perform an exact thermodynamic cycle, so that the ocean's state before that excursion is the same as afterwards. The parcel's salinity remains the same all the time, and its entropy at arrival at the surface is restored by putting back the exchanged heat before the parcel is lowered again. I have added text to emphasise this.

“The work required to lift and lower the parcel is balanced because the parcel’s thermodynamic state is exactly the same before and after the balanced reversible heat exchange across the surface. The “heat content” defined this way for a single parcel is added up then over all ocean parcels to result in its total OHC value.”

R2: 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.

RF: As part of an arbitrary definition, reference states may always be defined arbitrarily. Of course, such a state definition may be relevant or not, useful or not, possess certain advantages or disadvantages. Reasonable conditions for the choice of the OHC reference state are discussed in points (ii) and (iii) below eq. (5).

The related sentence has been modified as:
“The **reference state** relative to which OHC is measured may freely be specified at will, but beneficially be chosen with respect to its convenience or usefulness.”