

Response to the reviews

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We would like to thank both reviewers for their valuable suggestions and comments.

Since both reviewers commented on the application of our findings to a model like E3SM, we combined their relevant quotes in the first section, and addressed them simultaneously in this document and in the manuscript. The second and the third sections contain the rest of the reviewers' comments (one section per reviewer). The black text is from reviews, the blue text is our response.

We are also attaching two pdf files, a modified manuscript and a manuscript that highlights differences between the new and the old versions. All references to the draft below imply the revised draft, unless stated explicitly.

1 E3SM discussion related to both reviews

A quote from Reviewer #1:

... My main suggestion is that the paper arrives at interesting results from the numerical experiments (particularly for System A1), but there isn't much discussion that links this back to expected behaviour in Earth System Models. For instance, the results suggest that System A1's evaporation process is unstable, so if possible it would be helpful to see discussion about how E3SM avoids this. ...

A quote from Reviewer #2:

... After lengthy derivations the reader is left without an interpretation of the results. How do these results impact the simulations of E3SM? I would assume the energetic inconsistencies will introduce a spurious drift but it remains unclear how large this is and what impact it has. This is unsatisfying and I would encourage the authors to explore this more to make the paper more relevant. Ideally, they would conduct model simulations in the spirit of Harrop et al. (2022) to explore the magnitude of the impact of the described energetic inconsistencies. ...

We thank both reviewers for encouraging us to more clearly connect the box-model results to expected behavior in a full Earth system model. Below, and in Conclusions within the modified draft, we expand on how to interpret our results and on why it is not possible to immediately transfer our findings into a model as complex as E3SM.

The primary purpose of our paper is to isolate and diagnose specific thermodynamic inconsistencies in E3SM's moist-energy treatment. Among those are usage of the dry-air specific heat capacity (c_p^d) instead of theoretically established water-species heat capacities in the atmosphere, inconsistencies in thermodynamic potentials across the components (atmosphere and ocean), deficiencies in calculating temperature tendencies during evaporative processes, and, finally, the use of energy fixers to compensate for energy imbalances caused by the above-mentioned thermodynamic inconsistencies. As emphasized in the broader modeling literature (e.g., Harrop et al., 2022), it is important that simulations "work for the right reasons," and in our work we demonstrate how consistent thermodynamics can be introduced in global circulation models so that stability and fidelity do not rely on compensating errors.

Our idealized systems that mimic E3SM thermodynamics, Systems A1 and A2, are designed to show mechanistically how thermodynamic inconsistencies can project onto model behavior. In particular, System A1 demonstrates that when the model attempts to enforce energy closure locally, the thermodynamic inconsistencies can introduce a positive feedback in the evaporation/condensation energetics that manifests as an instability. In the full model (E3SM), energetically inconsistent tendencies can be masked by other interacting components, including energy fixers that act non-locally (or effectively distribute the correction more broadly), as well as the extensive parameter tuning of multiple coupled parameterizations. In the particular case of "A1-like" instability, it is not observed in production E3SM simulations because both energy fixers are applied to the model globally, accounting for both evaporation and condensation at the same time. Since evaporation and condensation rates are close in magnitude and opposite in signs, magnitudes of the fixers are small, and overall the model behaves like System A2. We discuss this in Section 3.4.6 "Three systems and their relation to E3SM."

It would be ideal if a fully consistent Earth system model works without energy fixers (local or global). Our box model, denoted as System I, paves the way for such a goal.

We agree with Reviewer #2 that the most direct way to assess practical impact would be controlled E3SM experiments, in the spirit of Harrop et al. (2022), to quantify the resulting spurious drift and its magnitude. However, implementing a thermodynamically consistent formulation in E3SM cannot be achieved easily, via, say, a simple one-line replacement of constants for specific heat capacities: the biggest obstacle is that the atmosphere is lacking mechanisms of moist energy transfers within the atmospheric column in all of its parameterizations, as described in Sec. 2.2.2 "Current design of E3SM" (which we modified further following Reviewers' suggestions).

A fair comparison of our proposed improvements therefore requires significant development steps – implementation of the concept of moist energy across all components, validation and verification of new components, and retuning of each component and the model as a whole to recover comparable baseline observations. Missing development steps above would result in the model with thermodynamics as crude as the existing one, therefore, invalidating proper comparisons. All of the development stages outlined above are beyond the scope of the present paper. Our results are intended as a necessary intermediate step in this incremental pathway, complementing prior development work of Lauritzen et al. 2022, Harrop et al 2022, Guba et al 2024, and many others. We have added a brief discussion of these points and the resulting path forward below and in the conclusions:

- **Implementing transfers of water vapor energy:** Implementing the correct evaporation flux, given by equation 12, will require moving the energy of the evaporated mass, $c_p^v T \Delta m + (L_v + L_l) \Delta m$ (equation 4), to the atmosphere. However, as discussed in the paper in Sec. 2.2.2, there are no suitable mechanisms within the atmosphere to absorb the c_p^v term in any of parameterizations, including micro- and macro-physics, deep convection, etc. Therefore, we must correctly introduce such mechanisms in the EAM component of E3SM that transport and mix water vapor energy between layers, properly account for it during phase changes, and in turbulence-like parameterizations. Note that even energy-conserving, thermodynamically consistent atmospheric closure schemes by themselves is an underdeveloped field.
- **Implementing transfers of energy of other water forms:** A similar task will need to be accomplished for the energy of falling hydrometeors, cloud water, and cloud ice. While this is an active area of research, several fundamental processes require finalization.
- **Proper validation and verification of new moist-physics packages:** The microphysics, macrophysics, convection, etc. packages will need to be modified to account for consistent and unapproximated thermodynamics. These would likely be developed first as standalone codes with implementation on idealized and isolated test cases featuring only the relevant physics targeted by the respective package, perhaps in a similar manner to their original development [Morrison and Milbrandt, 2015, Morrison et al., 2015], before being coupled to operational models like E3SM.
- **Tuning E3SM with the new packages:** Tuning models as complex as E3SM towards observations remains a significant and necessary step due to uncertainties in Earth system modeling. The new consistent thermodynamics would reduce many of those uncertainties, but would not eliminate all of them. We expect that tuning will remain a big part of the Earth system model development even with consistent moist thermodynamics.

2 Review #1

2.1 Minor comments

1. Page 4, Lines 103-105. The authors mention that for the processes relevant to this work, conservation of energy can be reduced to conservation of enthalpy. Although the authors provide citations for this, I think the paper would still benefit from a brief explanation of this.

Thank you, an explanation was added to Sec. 2.2.1, 4th paragraph.

2. Page 4, Line 106-109. Could the authors clarify that their L_v and L_l terms are constant, and so are the “reference” latent heats, rather than the standard latent heats (which should be temperature-dependent when the heat capacities of different phases of moisture are different).

Thank you for this question. We added a clarifying statement about L constants to not to be confused with *latent heats* (Sec. 2.2.1, 7th paragraph), which are defined as differences of enthalpies of phases of water a few lines below (line 114 of the old draft). Also, following this comment and Reviewer #2 comments we reworded/added the corresponding text (Sec. 2.2.1, paragraphs 5-7) and added and references in that subsection. Please, note that constants L_v and L_l are not time-dependent in case of variable latent heats. Since, for example, the definition of a latent heat of evaporation is the difference $h^v - h^l$, the choice of specific heat capacities for water forms defines whether variable or constant latent heats are used. The case of variable latent heats corresponds to our System I formulation with unapproximated thermodynamics, and Systems A1 and A2 (E3SM/EAM-like thermodynamics) use constant latent heats formulation.

3. Page 5, around Eqn (1): could you clarify that m_l is the amount of water in the ocean after evaporation has occurred?

Thank you, it was added (Sec. 2.2.2, 3rd paragraph).

4. Page 5: Lines 141-143: Could the authors expand on why the atmosphere only receives an energy of $(L_v + L_l)dm$, and why the pressure adjustment process being energy-conserving is the cause of this? Should this process not conserve energy?

Yes, thank you, one can argue that the pressure adjustment process should have never been 'fixed' with the energy fixer. Note that EAM/E3SM inherited this design from CAM/CESM. The design is that each process in the atmosphere is energy conserving, including the process of the pressure adjustment. Such a design has its great advantages, it allows one to easier control and troubleshoot separate processes in the atmosphere. Also, when it was originally introduced, models' resolutions were so coarse that only transfers of L terms were considered for the moist energy transfers. But once this design is in place, it requires a significant effort to step away from it and to start modeling transfers of moist energy more accurately. Simply allowing the pressure adjustment to not be energy-conserving would not help the model to be more accurate. This is because while the pressure adjustment 'inserts' or 'removes' energy terms like $c_p^v T dV$ and $c_l T dK$ from the model, it does not do it accurately – there are many places in the model (in physics packages) where water forms change location and phases, and if the physics packages do not operate with energy of water during transport, mixing, or phase changes, simply avoiding pressure adjustment being fixed will not make the model any better.

Following your comment, we expanded the corresponding section by adding text on the pressure adjustment (paragraphs 5-7 and 10 in Sec. 2.2.2 in the new draft).

5. Page 6, Line 157. I would remove the word “possibly”, it seems clear to me that your framework is improved! Thank you, done.

6. Page 6, Eqn (4). It might be helpful to clarify what the temperature is in this equation. Thank you, we added an explanation that T is to be defined below.

7. Page 6, Lines 172-179. I found this paragraph really useful in understanding Section 2.2, and wondered if the authors might consider moving some of it to be earlier, which might improve the readability of this section. Thanks, we reworded the 1st paragraph of Sec. 2.1

8. Page 7, Table 1: I found this table helpful. Can I just clarify whether the top left box (“energy flux received by atmosphere, current”) should just be $(Lv + Ll)dm$? Is it correct that it also includes $clTdm$, as I felt this didn't agree with Line 143.

Please, note that inclusion of the c_l term is discussed in the last paragraph of Sec. 2.2.2. The term is brought in by another fixer, IEFLX. We relate to your confusion, it is very challenging to understand and to track these details in the current design. The convoluted design of the current model provides another motivation for our work, to outline a new design without fixers and with explicit energy transfers.

9. Page 12, Line 282-283: “as discussed later in Sec. 3.4.1”. Apologies if I simply missed it, but I couldn't find this discussion.

Thank you for catching this, the text got lost in previous revisions. We added back a paragraph (the 3rd paragraph in Sec. 3.4.1) on temperature ambiguity in Sec. 3.4.1 and reworded text in parentheses on those lines.

10. Page 12, Eqn before Line 294. I had wondered here if the denominator should be $c_l(m_l^o + dV)$?

Thank you for pointing to this issue. The code does calculate the tendency in the way you suggested. The difference between the two formulas is of a second order term, as we discuss in Sec. 3.4.1 (which we amended with an explanation on 2nd order terms following your comment #9). However, following your comments, we see that it is better to keep the discussion going with the formula based on $c_l(m_l^o + \Delta m)$ to avoid confusion. We reworded Sections 2.2.2 and 2.2.3; we also fixed a similar typo (there, it was only a typo) in Section 3.2.2.

11. Page 12, Eqn (25). I think it would be helpful in this equation to clarify that m_l is m_o^l to keep the notation consistent.

Thank you, fixed.

12. Page 19, Line 450, “In System I, latent heat release causes T_a to decrease towards the neutral curve”. I actually struggled to spot this decrease from the plot as the line seems almost flat, so it could be worth clarifying the description here.

Thank you for noticing this. We have now added a description that final T_a is only slightly lower than its initial value. However, the broader point is an unrealistic increase in T_a for system A1, that has been expanded with a more direct connection to the relevant equations.

13. Page 23, Conclusions: It would be really good to expand the conclusions, or the discussion in the previous section, to link the numerical results to the behaviour we might see from ESMs that follow the approaches of Systems A1 and A2, and why we might not see the instability that the numerical results seem to indicate. It would also be good to have a comment on how the authors intend to follow-up on the analysis from this work.

While originally we tried to address this in Section 3.4.6 “Three systems and their relation to E3SM”, following your comment we amended it. There, we explain that System A2, that we built upon System A1, is quantitatively most close to the current ESM models, including E3SM. Both System A1 and A2 are important for our presentation: System A1 is derived by carefully following concepts of E3SM, while in System A2 we remove effects of energy fixers because of their small local values in E3SM. This hopefully answers your first question – ESMs have System-A2-like behavior.

In the new draft, we also addressed your comment, potential follow ups, and what our work means for development of models like E3SM in the rewritten conclusions. Please, also see our response in Section 1.

2.2 Typographical or Style Points

1. Page 3, Line 74: There is a space missing between “e.g.,” and the citation
Done.
2. Page 4, Line 98: “boththe”, space missing
Done.
3. Page 4, Line 104: “practive” should be “practice”
Done.
4. Page 5, Line 131: “Assume ocean” is missing “the”
Done.
5. Page 5, Lines 144 and 146, and Page 8 Line 218: there are forward apostrophes that should be backwards apostrophes.
Done.
6. Page 6, Line 178: is there a rogue space after the forward slash in “deficit/ excess”?
Fixed.
7. Page 17-18, Lines 415-419. This was a very long sentence! Could you break it up?
Thank you for bringing this to our attention. We have re-written this text for clarity.

3 Review #2

This manuscript deals with the details of energy changes associated with water mass exchanges between the ocean and atmosphere. The authors derive energy conservation equations associated with evaporation and precipitation and outline simplifications made in a numerical model (E3SM), and how energy/mass fixers account for those. Instead of investigating the effects of the simplifications in model simulations they go on with exploring the behaviour of the ODEs arising from their different conservation equations. It turns out that the equations underlying E3SM imply unrealistic temperature drifts to reach equilibrium, but they stop there. There is no discussion of implications of their results for the model and the climate it simulates. This makes the manuscript a not so satisfying read as it stands.

Thank you for your comments. We did not explain this sufficiently in the original draft, but System A1, which we believe the Reviewer refers to in “... the equations underlying E3SM imply unrealistic temperature drifts to reach equilibrium”, does not adequately represent the E3SM model in this case. This is why we also introduced System A2 – while it is not energy-conserving, one could argue that qualitatively it represents E3SM much better. This in turn is because the global fixers applied in E3SM, the ‘dycore’ fixer and IEFLX, are small due to presence of both evaporation and precipitation at the same time.

Following your comment, we amended Section 3.4.6 (paragraph 4), which discusses differences between Systems A1 and A2 and highlights the overall advantages of the unapproximated System I compared to the other two. We also significantly expanded Conclusions, which now address issues raised above.

Please, also see Section 1 of the Response for the response to the last sentence in the Reviewer’s paragraph above.

Major point #1: (please, see Section 1 for the response)

Major point #2: Despite the lengthy derivations important specifications are missing. In section 2.2.1, there needs to be a discussion on reference temperature. It seems the authors are employing C scale as otherwise their equations would not work. But they should keep in mind that a meteorologist would expect K scale. There also is no discussion of the assumed reference state of water: it appears it is ice at 0 deg C. Lauritzen discussed the equivalence of different choices for the reference state in detail, but here it needs to be specified at least. Furthermore, in line 109 not even L_v and L_f are identified as latent heats of evaporation and fusion. Moreover, it is not said that L_v and L_f are taken as their constant values at 0 deg C (I think) rather than being temperature-dependent. Note also that Mayer et al. (2017) described in some detail the energy changes associated with phase changes as well as the formulation with constant vs variable latent heats.

Thank you for this important observation. Compared to the original draft, we reformulated the definitions of h^l and h^v (unapproximated) in Sec. 2.2.1 to be consistent with the one using the ice reference state and T_{00} from Lauritzen et al.. We then combined terms with T_{00} and $L_{s,00}$ and $L_{f,00}$. This led to small changes in constants L_v and L_l as defined in the 5th paragraph of Sec. 2.2.1. These modifications did not affect numerical results in Sec. 4 qualitatively or quantitatively, up to a few minor changes. We retained the formulation of E3SM-like thermodynamics as in the original draft: The E3SM implementation does not use T_{00} , and we kept values of L constants consistent to the E3SM code, too.

Thank you for your question about latent heats. We added a clarifying statement about L constants to not to be confused with *latent heats*, which are defined as differences of enthalpies of different phases of water. We reworded and expanded the corresponding text, added a clarifying paragraph, and references in that subsection (Sec. 2.2.1, paragraph 7). Please, note that L_v and L_l are not time-dependent in case of variable latent heats. Since, for example,

the definition of a latent heat of evaporation is the difference $h^v - h^l$, the choice of specific heat capacities for water forms defines whether variable or constant latent heats are used (citing Mayer2017, "The temperature dependency of /paper notation for latent heat/ is due to the difference in specific heats of liquid water and vapor..."). The case of variable latent heats corresponds to our System I formulation with unapproximated thermodynamics, and Systems A1 and A2 (E3SM/EAM-like thermodynamics) use constant latent heats formulation.

We added Chris Eldred, a thermodynamics expert, as a co-author of this work since he provided his expertise in navigating this and other questions.

We are not sure what Reviewer meant by "... the authors are employing C scale as otherwise their equations would not work." – hopefully, their concerns were addressed above. The manuscript uses only Kelvin scale.

3.1 Minor points

Line 42: What do the authors mean by "climatological energy trends"?

Thank you, we added 'long time scale' to the sentence as clarification.

Line 97: "both the" – space missing

Thank you, fixed.

Table 1: in the current model, shouldn't the energy fluxes received by the atmosphere be scaled by $cpdry$?

Thank you for this question. The fluxes should not be scaled by c_p^d , because the total energy received by the atmosphere does not contain a c_p^d term. It is removed from the atmosphere by the 'dycore' energy fixer, which fixes both the dycore and the pressure adjustment. We agree that this part of the model's design is very confusing. Similar questions were raised by the other reviewer in Section 2 (items 4 and 8). Following both Reviewers' comments, we added more clarification on this question and the pressure adjustment in Section 2.2.2 (paragraphs 5 and 6). We highlight that it is very challenging to understand and to track these details in the current design of EAM. The convoluted design of the current model provides another motivation for our work – to make a new design without fixers and with explicit energy transfers.

Line 221: Why do the authors choose ΔK for condensation rate and not ΔC ?

Thank you, we have a slight preference to use K symbols. This is because the draft uses a lot of c notations for specific heat capacities, but also because the capital C is universally used to define an unspecified constant, $C = const$.

Line 296: the authors seem quite confident that eq. (12) is "more correct" than eq. (25), where the difference lies in the use of different specific heats for the evaporated water mass. They seem to assume that the energy required to make evaporation happen is taken equally from the main water mass and the evaporated water mass ΔV (when ΔV is already vaporous). The described processes in nature happen at the same time, and so it is not entirely clear why the sequence of events as insinuated by eq. (25) is the correct one? Can the authors elaborate why this is the more appropriate view on the ongoing processes?

Thank you for this question. In our approach for unapproximated thermodynamics, we follow physical principles as closely as possible. The difference between Eqns (12) (unapproximated thermodynamics) and (25) (E3SM thermodynamics) is that Eqn (12) uses specific heat capacity of vapor, c_p^v , for vapor mass, while (25) uses specific heat capacity for liquid, c_l , for it. Our adopted approach in unapproximated thermodynamics is to use specific heat capacity of vapor for vapor, and not specific heat capacity of liquid, like in Eqn (25), for it. Like the community uses $L_v + L_l$ for the vapor mass instead of using L_l (that is used for liquid only), in the same spirit that in our unapproximated approach we use all of the established thermodynamics of water vapor for water vapor.

Yes, the process of evaporation is established in the literature (for example, [Feynman, 1963-1965 (Online updates 2006, 2013, 2022) or [Niiler, 1993]), and it is known that the liquid cools as it evaporates. Therefore, in (12) and in (25) the ocean is assigned new, cooler, temperature. Note that the temperature assignment for the evaporated mass is a second-order effect, as we discuss in Sec. 3.4.1 (we amended Sec. 3.4.1 following a comment from Reviewer #1, Item 9.). Separately, there are different time scales in the Earth System Models, and one could point that mixing with air could also play a role in deriving (12). However, as stated in ["Climate System Modeling", editor K. Trenberth, Chapter 8], "The mixing of tracers by turbulent processes is much greater than by molecular diffusive processes." In this work potential effects from molecular diffusion with air is ignored in (12).

Line 378: in- \dot{z} is; also replace "." with "," later on that line.

Thank you, fixed.

Line 391: typo in "globally"

Thank you, fixed.

Line 397: a similar ref to Harrop et al. has already been made further above.

Yes, we cited it twice because of different contexts – one mention is to emphasize poor performance of local fixers, and the other one is in the context that models with local fixers are similar to System A1.

References

- Richard Feynman. The Feynman Lectures on Physics, Chapter 1 Atoms in Motion, 1963-1965 (Online updates 2006, 2013, 2022). URL https://www.feynmanlectures.caltech.edu/I_01.html.
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- P. P. Niiler. The ocean circulation. In K. E. Trenberth, editor, Climate system modeling. Cambridge University Press, 1993.