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1 General comments

The author proposes to estimate Earth energy imbalances in the sunlit ocean layer (approximately top 200 m) via heat budgets. Heat in the sunlit ocean layer is affected by both radiative forcing and heat transport (e.g. advection and diffusion). In contrast, deeper ocean layers are affected mostly by heat transport and, to a much lesser extent, by geothermal processes. The sunlit ocean layer is important for two main reasons: this is where air-sea interactions take place, and it is where marine life is most abundant. The aim of the paper is relevant: a better understanding of how heat accumulates in the sunlit ocean layer would fit into the current literature about tracking Earth energy imbalance from the ocean perspective and could potentially facilitate a deeper understanding on marine heatwave genesis.

The paper uses a series of state-estimate based datasets to evaluate heat budgets in the sunlit ocean layer. The result section includes: (1) a geographical view of energy imbalances in the sunlit ocean layer, (2) a decomposition of the aforementioned imbalance between so-called *transport* and *forcing*. The author argues that regional heat accumulation depends more on the transport term of the sunlit ocean layer heat imbalance than on the forcing term, hence the title.

As it is now, the paper does not provide enough information about data and methods to assess the scientific results. For instance, is diffusion included in the so-called transport term of the heat budget? This would be important to specify. The lack of structure in the paper does not facilitate the reading and the frequent back and forth between figures and appendix confuses the presentation greatly.

In the “Specific comments” section, I provide some suggestions to help improve on the structure and readability of the paper, per section. In the “Technical comments” section, I highlight some typos and small issues with the form of the article.

2 Specific comments

2.1 Abstract

Overall, the abstract is not clearly structured and therefore not easy to decipher. It would be helpful to re-write the abstract following the structure: background and significance of the work (what is the gap in the current literature), research questions, methodology (including data products), results, and conclusion (with a word on broader impact of the work).

Some other points:

- first 2 sentences of the abstract: I think the abstract should start with a definition of earth energy imbalance, like in the introduction. Read like this it is unclear: we are lead to think ocean heat transport includes radiative forcing and has an impact on marine life. And then in the next sentence we read that below 200 m, there is only ocean heat transport. So the difference in what processes are at play in the two regions and how they contrast with each other is not clear. Starting from the energy imbalance definition and highlighting that the heat budget is: heat transport + radiative forcing in the SOL, and only heat transport (+ some minor geothermal processes) below SOL would make things much clearer for the reader.
- l3: in the abstract, I would add a sentence about the research gap. It is not clear why one needs to introduce the notion of SOL-EI. Something like in the introduction l35-40.
- l15: “OCCA2” is not defined until the appendix, it would be helpful to define it much sooner and clarify why this dataset is more suitable than others for the heat budget analyses.
- l14-18: if this method is the one employed to get the results presented in this article, then it would be helpful to place the sentences about the method *before* the result sentences in the abstract.
- l18: “IAPv4” is not defined.
- l18: I suggest adding a sentence about broader impact of the research at the end of the abstract.

2.2 Introduction

There seem to be results in the introduction, with references to 7 result figures. I would remove those and suggest to focus instead on:

1. introducing the topic and what is the relevance of looking at SOL-EI? (maybe expand on l28-39).
2. explaining the existing literature on energy imbalances: how they are computed (e.g. review by Meyssignac et al. 2019), what is the gap the present research aim to fill within this context.
3. explaining the aim of the present work, what question will be answered, what is the scope of the analyses, what tools will be used (in broad generic terms, for example why choosing state estimates?).
4. a summary of the structure of the article (so that the reader knows where to find what information quickly).

It is really not clear why using OCCA2.2(abcd). It would be very helpful to explain to readers the genesis of those products (referring to their relevant data papers) and why they have been chosen to study energy imbalances in the SOL compared to other products. Some other points:

- l37 “already heightened risk”: not sure what risk is referred to here. I would rephrase with “Warming in the tropical SOL can also boost (increase the frequency/intensity? of) tropical storms and atmospheric rivers” and possibly touch on the socio-economic impact of those.
- Fig. 1 shows different data products, but none of them are defined. It is nice to show several products if they are defined beforehand and the reader understands what is the point of having several products shown in one figure. But here none of those have been introduced beforehand. What is the purpose of this figure? Is it to show an increase in heat content anomaly? Is the figure showing that one product is superior compared to the others in that metric? Does the figure show that the analyses are robust to some technical changes? If the purpose of the figure is to build on the results of Hakuba et al. 2024 by showing the difference between SOL and below-SOL, then I would say this is a result and should go after the methods section.
- Fig 1.: in the caption there is mention of “OCCA2.2 (red)” but in the legend it says “OCCA2.2d”, it would be helpful to harmonize the name of the products to avoid confusion.
- l46: “(see below)”: I suggest to remove it and focus on an article structure that brings things as they are needed by the reader to make sense of the study.
- l47: “SOL-HB” not defined anywhere in the text, a definition is sorely needed.
- l48: refers to the “left-hand-side of the SOL-EI equation” and later “right-hand-side”, it would be more helpful to write the entire budget as a mathematical equation, define each term of the equation *and then* refer to right vs. left side of the equation.
- l47-51 this paragraph refers to 7 figures, including one in the appendix, and briefly covers the budget calculation method. As a reader, it is rather unpleasant to have to scroll through. I think there is no need to refer to result figures in the introduction.
- l54: “innovative estimation method”: if this is innovative, surely it is worth a proper method section.
- l47-62: should be in the method section.

2.3 Methods (absent here)

I insist that a traditional method section is necessary to understand this work. The small method mention in the introduction and the very long (but not so helpful) Appendix are no replacement for a method section located between the introduction and the results. As is it now, it is not possible to assess the scientific results of this work.

Here are some suggestions for what should be helpful in the method section:

1. Data products: Explain the choice of data products (OCCA2, IAPv4, ARGO), why those, why different versions of those, and reference to their respective data papers. Also define properly ECCO4, ERA5, ECCO4-RE2 if they are mentioned. I suggest starting with ECCO4, then OCCA (mentioning and defining Argo), then variants of OCCA(12.2abcd etc.). A table would be helpful here, example of columns: abbreviated name, full name, date, data paper, key features that allow to distinguish them in the context of this paper, etc.
2. Numerical methods: define SOL-EI, SOL-HB, SOL-HC, climate normal state and interdecadal oscillation pattern with mathematical formula (and definition of each terms).

Some notes about the appendix section:

- l333-336: the context provided at l345 should come before any mention of OCCA2(.2abc). Move l333-336 to the end of the context section.
- l337-343: same as above, details about ECCO4 should go into the “context” section.
- l383 and l387: what are intensive and extensive properties?
- l391: reference to A2-A4, but those equations are not explained, what is G prime? why not doing $A2=A3$ and show the full budget? What terms are in “forcing” and “transport”?
- Fig. A1: y-axis 1st row “depth” is missing. I suggest to either put it on all the subplots (with units) or on none at all and add a mention in the caption.
- Fig. A1: not sure why OCCA2.2(abc) since there is one subplot per year/latitude? Are these ensemble means?
- Fig. A1: “middle” is ambiguous in the caption. I suggest to use top/middle/bottom row and columns, instead of just top/middle/bottom. Or put figure handles on each subplot (e.g. (a.), (b.), etc.)
- Fig. A1: the top right subplot shows a snapshot (?) for the year 2022, yet 2022 is not represented in the time series shown in the middle and bottom rows. I suggest to add an explanation for this discrepancy.
- l403: SOL-HB (heat budget?) should really be defined with a mathematical formula.
- Eq A5-6: the transport and forcing terms should be made explicit with a mathematical formula. As they are now, they are much too vague to properly assess the result shown in the paper.
- Eq A8-A9: where does this come from? is it a common way to add weights? the terms are not all defined (e.g. “h”).

2.4 Results

Because there is not enough information about the data and methods employed in this work, the following comments focus on the structure of the article rather than on its scientific content. However, I suspect that the title might be slightly misleading. Based on my limited understanding of the work, it seems that the results show that both so-called “transport” and “forcing” terms have an impact on the geographical distribution of regional energy imbalance in the sunlit ocean layer. There are indeed some regions where the energy imbalance in the sunlit ocean layer seems to be dominated by the “transport” term, but it would be a stretch to say that this is the case for most regions (e.g. see Southwest Indian Ocean, Northwest Atlantic, Drake Passage, etc.). This is why I suspect that the geographical distribution displayed in Fig. 4 is more complex than what the title suggests. It would be fair to say instead that where the heat arrives is not necessarily where it accumulates the most. It would be interesting to document instead which region is dominated by which (properly defined) heat budget term and contrast it to similar studies that have employed heat budgets to diagnose marine heatwaves (see for instance the review from Oliver et al. 2021 <https://doi.org/10.1146/annurev-marine-032720-095144>). That being said, my understanding of the analyses remains limited and thus I cannot evaluate this much further.

About the overall structure of the result section: I would suggest to stick to one subsection/paragraph per figure, and avoid (as much as possible) to refer to previous (or subsequent) figures in the text. This would greatly improve the clarity of the results.

Some other notes:

- l65: if the first result is about the bottom panel in Fig. 2, maybe it would be worth putting this subplot at the top of the figure?
- l74-77: this should go into the method section where a paragraph should be dedicated to explaining why so many data products are considered in this work and what is the purpose of each (a table would be particularly helpful).
- l78 “SOL-EI maximum around 2015 (Fig. 2, bottom)”: I fail to see a maximum at 2015, maybe this refers to another figure?
- Fig. 3 and l98-99: if the area near 40° is the most interesting and referred to in-text, it might make sense to show latitudes 40°N and 40°S in Fig. 3 instead of 35°N and 35°S
- l100: I do not quite agree with the separation of 1980-2007 vs. 2008-2022. From Fig. 3, I see EEI in the northern hemisphere mid-latitudes larger than in the southern hemisphere.
- Fig. 3: since 2007 is a notable separation, at least in the text, it could make sense to add a vertical line on the figures to see where that is?
- Fig. 3 and l113: why speak of 2022 in particular but not putting it in the Hovmöller plots in the middle and bottom rows?

- l115 and l119: in text the temperature unit is Kelvin, but on the figure it is degree Celsius. I suggest to stay consistent with the unit for text and figures, even if they amount to the same thing.
- l118: 2022 is spoken of about the bottom rows of fig. 3 but is not in the time series.
- l121-134: these 2 paragraphs are still about Fig. 3, despite being part of a new section, which is confusing.
- l132: still mention of 2022, while the time series does not cover it.
- l138: what does “jointly” means here?
- l140: why taking the average over 2013:2017? I do not see why based on Fig. 2. Why not go all the way to the most recent periods?
- Fig. 4 and 5: I think Fig. 5 should come before Fig. 4. In Fig. 5 we have elements necessary to understand Fig 4.
- l135-146: if the point here is to show that the method of substracting CNS and IOP from the signal to look at processes at play is reasonable, then I suggest this goes into the method section (together with clear explanation and formulas to understand the data processing and rationale behind decisions, e.g. the time averaging periods). It is also really difficult to read because the text refers to 4 figures, without explaining much what they are, and without clear progression.
- l141 ”vast regions of the Ocean”: it would be helpful to give a short description of those regions?
- it would be useful to clarify what is the difference between the CNS and the anomalies from CNS (or in the method section). It is unclear where is the long-term warming signal here.
- Fig. 5: the mathematical meaning of the tendency, transport, forcings terms are not defined.
- Fig. 5: caption “detail” should be “details”?
- l153-1156: Since this argument is the highlight of the article (based on the choice of title), it might be worth making one single compact figure to display those results, instead of asking the reader to go back and forth two figures.
- l157-161: While I acknowledge a limited understanding of the results presented here due to a lack of information about datasets and methods, I am not fully convinced by the conclusion drawn by the author. Based on Fig. 4 and 5, I cannot quite say confidently that the geographical distribution of energy imbalance is dominated by the so-called “transport” term.
- l170: forgotten parenthesis for the references.

3 Technical comments

Here is a list of more minor details, such as grammar or notation corrections.

- l3, l8, l28, l3, l44, Fig.1 caption and subtitles, l72, l86, etc.: “200m” should be “200 m”, see guidelines here
- l2: “feed back” or “feedback”?
- l385: “seaice” or “sea-ice”?
- l361: the capitalization of all words in the section titles is not necessary
- l210: not sure ocean should be capitalized
- l227: extra space before “:”
- l173-174: SOLEI should be SOL-EI to remain consistent with previous abbreviations.

Abbreviations that need to be defined (in alphabetical order):

- CNS: “Climate Normal SOL-EI” (long-term average of SOL-EI), defined at l425. ! Defined differently at l136: “Climate Normal State”.
- ECCO
- IAPv4
- OCCA: OCEan Comprehensible Atlas, defined l345, should be defined much earlier in the main text.
- OCCA2
- OCCA2.2(a,b,c,d)
- SOL-HB
- SOL-HC