

Interactive comment on “Heat stored in the Earth system: Where does the energy go? The GCOS Earth heat inventory team” by Karina von Schuckmann et al.

John Fasullo

fasullo@ucar.edu

Received and published: 21 May 2020

Interactive comment on “Heat stored in the Earth system: Where does the energy go?” by vonSchuckmann and coauthors for consideration in Earth System Science Data

Summary

The manuscript provides a WRCP-SPARC group summary of the status of our understanding of Earth’s Energy Imbalance since 1960 and covers much of the recent progress in documenting the component level evolution of energy uptake, including ocean, atmosphere, cryosphere, and land. An effort is made to quantify some of the

Printer-friendly version

Discussion paper



uncertainties. I find the manuscript to be reasonably well organized and clear in scope. I do have numerous suggestions to improve the writing, which is in instances awkward, but these are generally minor. I also strongly suggest that the tables be reconsidered in favor of a graphical summary such as the one below, which I believe will be much more effective at conveying the key points. I also think additional context should be added, particularly to the section on boreholes, to address the limitations of this data source that I believe go largely unmentioned. I have similar concerns regarding the ocean heat content section. Lastly I also feel some effort should be made to minimize the aliasing of internal variability onto the trends.

Major Issues

- 1) The tables are largely ineffectual at conveying the key points. See suggestions below.
- 2) Basic characteristics of the borehole data go unmentioned and associated caveats in forming an estimate of global-land heat uptake go unaddressed.
- 3) The role of sampling uncertainty in the ocean heat content estimates gets swept under the rug when the multi-estimate average is performed and Figure 1/2 and their discussion gives an overly confident depiction in my view. This issue should be raised and emphasized in my opinion - as it is a major caveat to the summaries.
- 4) No effort is made to account for internal variability in accounting for trends. This is particularly relevant for the atmosphere where the role of ENSO is strong. I suggest some effort be made and a number of reasonable approaches exist.

Minor Issues

78: “internal climate modes can temporarily alter the energy balance for periods of sub-monthly to several decades”; deserves several references

92-93: this statement also deserves several references - e.g. Cheng et al EOS

[Printer-friendly version](#)[Discussion paper](#)

101: there is substantial noise in this equation and this should be acknowledged with an additional term, e.g. epsilon, even in a conceptual model since it is unavoidable.

132: Table 1 would be more useful as a figure such as in the ocean heat uptake summary below as it provides a visual for both period and magnitude/uncertainty:

http://www.realclimate.org/images//oh_uptake2.png

183: I find “change in behavior” to be vague and unclear.

187: I find “Implications of ocean warming. . . , and have in turn impacted the ocean itself” to be both unclear and poorly related to the following sentence where the impacts on the ocean go unmentioned and thus are not examples.

194: “challenges of Ocean Heat Content (OHC) estimates” is poor grammar. what I think is mean is “opportunities and challenges in forming OHC estimates. . .”.

196. Remove “Early” since before 1900 is already stated.

215: “further challenged the global scale ocean heat content estimate” perhaps should read “posed a further challenge for the global scale estimation of ocean heat content”

216: “correct biases”

222: “with satellite-derived. . .”

233: “The opportunity for improved OHC estimation provided by Argo is tremendous”

239: “allow further observing system recommendations” could read “inform further observing system recommendations”

258: 2018).

265: What initiative?

268: We do (present tense?)

271: Is an “ensemble mean and standard deviation of near-global “ an improvement?

Printer-friendly version

Discussion paper



The assumption is that all errors are random and not systematic. But are they? I think the case would need to be made, and I don't think it is largely true.

273: "is needed to include all missing in situ-based products" is not clear what is being referred to? what are the main data sources that have yet to be included (in reanalyses for example)? Figure 1: What exactly is in the sigma? XBT drop rate uncertainties? sampling uncertainties? I sense that the answer is "none" but rather the spread in the derivations of previous products, which themselves have much larger uncertainties and that averaging together does not reduce. This is a point worth emphasizing as Fig 1 seem overconfident as-is. I think that a true "uncertainty" bound be shown somehow (whiskers?). And I suggest the reference to "values are given for the ocean surface" be made clearer as the figure itself obviously doesn't deal with the ocean surface (just the area used for the heating rates). Figure 2: As with Figure 1 there is an underlying sampling/data uncertainty that is not dealt with but is very relevant to the values plotted and should be shown. Ultimately I think this uncertainty creates an important caveat for the results. Also the figure/caption lack units).

311: But this result is already well known?

370: Eq (1) seems off as the surface enthalpy fluxes for P and E are already contained in FS, unless what is meant is the very small term of the sensible heat of rainfall? (other components of enthalpy such as evaporation are already in FS). Given the description later (in lines 388-90) I think sensible heat is the correct term? as enthalpy also entails the latent component.

381: Why "global" evaporation and precipitation?

398: "account for"

406: "neglect"

425: "used" ...

Figure 3: The caption does not adequately distinguish between the terms plotted in

[Printer-friendly version](#)[Discussion paper](#)

the middle and bottom row. Perhaps it could better emphasize that one is the anomaly while the other is the annual tendency (change and trends can be taken as synonymous).

476: An important point is raised here. Clearly El Niño is aliasing on to the trends to some extent. Shouldn't an effort be made to remove such effects? Various approaches have been tried in the literature and I'm not advocating for any single one, but to make no effort seems lazy.

544: There is an odd symbol after "condition."?

555: The entire discussion is very generic here. What is the spatial sampling of boreholes that can be used and what temporal resolution for GSTHs can they provide and at what spatial sampling / accuracy can they be used. How does groundwater corrupt/influence inferred histories? What validation of the reconstructions exist and what does all of this reveal that is both new and robust about "the evolution of the climate system".

565. How is a global estimate inferred from what must be very discrete observations?

Table 2: Again I think Table 2 would be best as a figure such as suggested for Table 1.

639: Remove "in sea-ice extent" as it is redundant.

624: "with ice loss"

652: "which are based on a modeled surface mass balance combined with..."

654: "estimate of floating ..."

740: "needs further evaluation"

761: There is oddly no mention of the need to develop modeling/assimilation capabilities as a priority. The authors may view these as distinct from the observational imperatives however in my view they are not, as they are central to the generation

[Printer-friendly version](#)[Discussion paper](#)

of ocean/atmosphere/cryosphere models that are vital for improving the reanalyses used. The same can be argued for terrestrial system models given the poor coverage of borehole measurements.

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2019-255>, 2020.

Printer-friendly version

Discussion paper



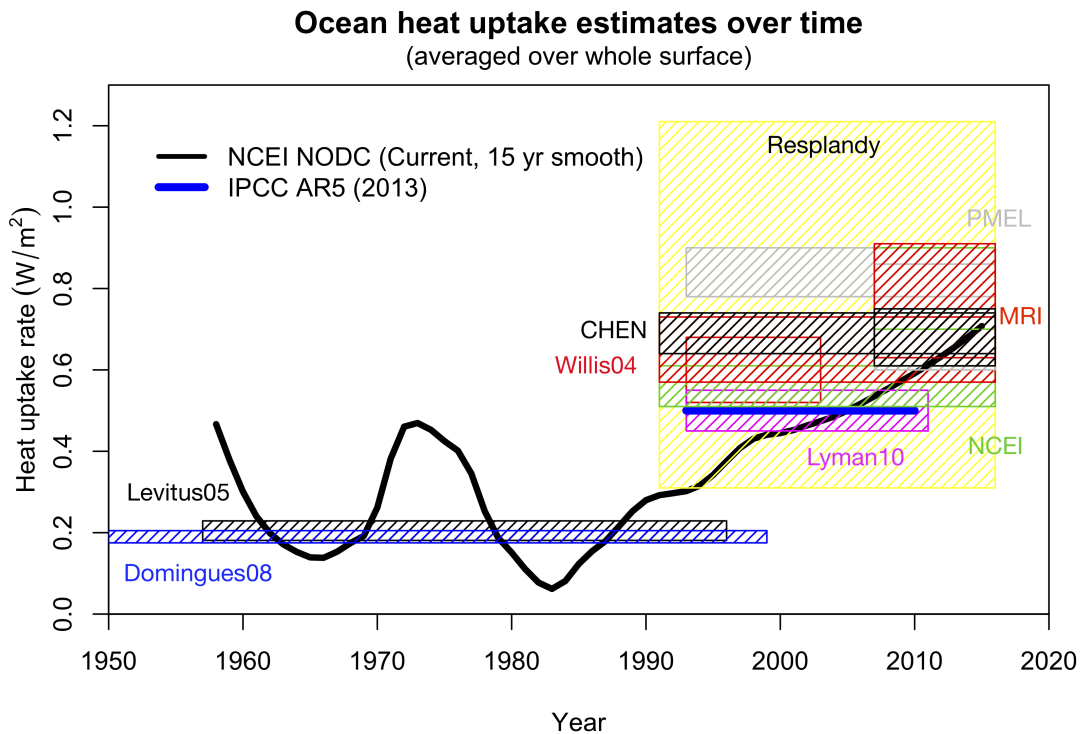


Fig. 1.

Printer-friendly version

Discussion paper

