

Earth Syst. Sci. Data Discuss., referee comment RC2
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Comment on essd-2022-239

Anonymous Referee #2

Referee comment on "Heat stored in the Earth system 1960–2020: where does the energy go?" by Karina von Schuckmann et al., Earth Syst. Sci. Data Discuss.,
<https://doi.org/10.5194/essd-2022-239-RC2>, 2022

This study is an update of von Shuckmann et al. (2020) heat inventory . It provides 2 more years of the inventory from 2018 to 2020. There is one innovation compared to von Shuckmann et al. (2020): the new heat inventory includes now estimates of the permafrost thawing, inland freshwater and Antarctic sea ice heat uptake. In this paper, the authors call for a regular update of their heat inventory and for an implementation of the heat inventory in the Paris agreement's global stock take.

This manuscript is dealing with a very important aspect of climate change: the heat uptake of the climate system. The paper is well written and easy to follow. The methods used are sound.

Scientifically speaking, I am disappointed by this manuscript. I find the progress compared to von Schuckmann et al. (2020) is incremental and the results are not new. The uncertainties are not improved compared to von Schuckmann et al. 2020 (not better documented and not reduced in any manner either) and we don't get substantial new knowledge out of the analysis that are proposed.

However, in terms of climate policy and knowledge for action, I think this paper is relevant and support an important position in the community. I definitely agree with the authors that the heat inventory should be implemented in the Paris agreement's global stock take and should be more advertised to the general public. I find this manuscript supports nicely and efficiently this position.

In summary, I find that this paper is more a position paper than a scientific paper. I think the authors should acknowledge that and be clearer on this aspect. I also think the authors should target journals that are more suitable for position papers. By publishing in ESSD they may miss a substantial part of their targeted audience.

Detailed comments:

L139-143: I find this picture of the heat accumulated in the Earth system, which would result from anthropogenic GHG emissions only, too simple and misleading. I think you should acknowledge there is a more complex situation here. At least you should mention the role of other important forcing such as the aerosol forcing and the role of internal variability as well.

L151: you probably mean “confirmed” rather than “revealed”. The long-term heat gain has been revealed a long time ago (ex. Levitus et al. 2001)

L160-161 : indeed the results are closely consistent with the IPCC AR6 and von Shuckmann et al. 2020. I don't see here any significant improvement compared with previous estimates. The improvement only comes from the addition of two more years but the picture of the heat redistribution has not changed. I find this improvement is really incremental compared to von Shuckmann et al. 2020

L185: To my knowledge ice shelf mass discharge has never been attributed to anthropogenic GHG emissions so far (although the attribution is highly probable). This is because attribution needs a thorough understanding and modelling of the processes at play which is not yet available for ice shelf. So I suggest to remove “ice shelf” from this sentence.

L258 : I don't understand why the heat inventory provides a tool for assessing the general status of the GCOS. Can you elaborate ?

L266 : Any other climate indicator or scientific study enables « concerted international and multidisciplinary collaboration ». I don't see a special added value from the heat inventory over other initiatives.

L269-273: I think these lines are the core of this paper. I understand you are calling for a regular monitoring of the heat inventory to support the IPCC solution pathways and to

support regular stock taking of the implementation of the Paris Agreement. So this paper is more a position paper than a scientific paper reporting on recent progress. I think this aspect should be assumed from the beginning and the paper should be presented as a position paper rather than a scientific contribution.

L310-313 : OHC estimates from remote sensing through the global sea-level budget are not merely "possible". They are now mature (See Hakuba et al. 2021, Marti et al. 2022). You should consider these estimates here.

L352: the problem in using the spread as a proxy for uncertainty is that you don't know the sources of uncertainty. Can you tell us more about the uncertainty here? What are the main sources of uncertainty? Which one dominates? What is the temporal structure of the uncertainty? Is it correlated in time? Do you consider this information to compute the tendency?

L402: what does "largely homogeneous criteria" mean? Please be specific

L416 : do you mean « of the corresponding ensemble »?

L430: time correlation in the uncertainty could bias significantly your trend estimate. Have you considered this?

L448: by "below 700m" you mean between 700m and 2000m depth, right? Please be specific

L529: there are typos in the equation. Please correct it

L618 Figure 4: same remark as before. You are using the spread as a proxy for uncertainty. The problem is that you do not know the sources of uncertainty. Can you tell us more about the uncertainty here? What are the main sources of uncertainty? Which one dominates? What is the temporal structure of the uncertainty? Is it correlated in time? Do you consider this information to compute the tendency? What about systematic sources of uncertainty?

L667-670 I agree with the authors, the study here essentially confirms von Shuckmann et al. 2020

L759-760: I understand the much lower uncertainty in ground heat uptake in this study is coming from the new inversion method for the vertical temperature profiles. Why should we trust this new uncertainty estimate rather than von Schuckmann et al. 2020? What makes it superior?

L929: Why attributing the same uncertainty to GIOMAS as to PIOMAS? Is it reasonable? Why so?

L 1032: how do you estimate the rate in EEI and the associated uncertainty . I don't understand how you can get such a small uncertainty in the rate of change of EEI when you have such large uncertainties in the estimate of heat uptake of different components of the Earth system. Please, detail your uncertainty estimate here?

L1044: I disagree. Fig 9 shows that the primary need is to reduce uncertainties rather than to extend the time series. Can you comment on this? Why do you put forward the extension while uncertainties are still so large?

L1088: not "reveal" rather "confirm"

L1100-1127: I agree with this paragraph and I agree this is important to call for implementation of the Earth heat inventory into the global stock take .But I don't think it should be done in a scientific technical paper. It should rather be done in a scientific position paper. In addition, ESSD is probably not the best place to do that.

L1147-1267: How come there are no recommendations on improving/reducing uncertainties? Figure 9 is probably the most advanced scientific result of this paper and it

definitely calls for a reduction of uncertainties. I suggest to put some recommendation along these lines at a high level of priority. If not, we would like to understand why uncertainties are ignored

L1269: Here again, I find the position of the paper is not clear. If this is a scientific paper dedicated to scientists (as ESSD is for) then updating the record is not so important. Reducing uncertainties is probably much more of a priority. But, if this paper is a position paper more oriented toward climate services which calls for the implementation of the heat inventory in the global stock take then yes the priority is probably to update regularly the record. In the latter case the manuscript is probably proposed to the wrong journal and I am afraid you may miss your targeted audience.