

Earth Syst. Dynam. Discuss., referee comment RC3
<https://doi.org/10.5194/esd-2022-32-RC3>, 2022
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Comment on esd-2022-32

Anonymous Referee #3

Referee comment on "Continental heat storage: Contributions from ground, inland waters, and permafrost thawing" by Francisco José Cuesta-Valero et al., Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2022-32-RC3>, 2022

In this manuscript the authors evaluate the continental heat uptake since 1960. It is an update of their previous work published in von Schuckmann et al. 2020. Compared to von Schuckmann et al. they made 3 changes in their estimate: 1) they changed their method to estimate the ground heat uptake from subsurface temperature profiles 2) they added an estimate of the permafrost heat uptake with a permafrost model and 3) they added an estimate of the lake, reservoir and river heat uptake with a global lake model forced by historical simulations of Earth System Models (ESM). The authors find a total continental heat storage of 23.9 ± 0.4 ZJ since 1960 which is consistent with von Schuckmann et al. estimate of 24 ZJ since 1960. But this consistency is by chance. Indeed, the authors actually find a ground heat uptake that is significantly smaller than von Schuckmann et al. 2020 by $12 \text{ mW} \cdot \text{m}^{-2}$ and the difference in ground heat uptake is compensated by the addition of the permafrost heat uptake and the inland water heat uptake estimates.

The manuscript is clear and well written. It deals with an important question which is the distribution among reservoirs of the excess of heat gained by the climate system in response to greenhouse gases emissions. The distribution of heat among the Earth reservoirs at interannual and longer time scales is driven by the different heat capacities of the different reservoirs. The heat capacities of the reservoirs show very marginal changes with climate change thus the distribution of heat in the climate system is the same over time as it warms. It is important to estimate the distribution of heat among the Earth reservoirs to determine where the heat is actually located and what are the places of the world that are the most impacted by global warming. This is also a good indicator of current global warming and its distribution. As such, it is useful to derive the land heat uptake in order to raise public awareness. The work in this manuscript helps in this objective. In particular I find interesting the tentative estimate of the permafrost heat uptake. Permafrost heat uptake is an important indicator of the changes in a key place for the future of the climate system. It could definitely be an interesting indicator to raise public awareness.

Concerning the results of this paper, I find they are disappointing for three main reasons

First the authors find a ground heat uptake that is not consistent with their previous estimate in von Shuckmann et al. 2020 although they have used the same subsurface temperature profiles and the same inversion method. The significant difference between their previous estimate and the current estimate comes from the aggregation technique. But the confidence in the aggregation technique is not evaluated in the manuscript. So, we don't know why the estimate of ground heat uptake is so sensitive to the aggregation technique and what should be done to tame down this high sensitivity. We don't know either which aggregation technique should be trusted and thus which estimate of the ground heat uptake should be trusted: the one that is proposed in this manuscript or the previous one from von Shuckmann et al. 2020? More analysis are needed here to determine the confidence in the ground heat uptake estimate and explain the causes for the differences among the different estimates

Second, the authors estimate the inland water heat uptake from models only. They do not use any observations or reanalysis (even the forcing of the global lake model is coming from ESMs). If the objective of continental heat uptake estimates is to "inform about future warming and climate change as well as to understand the future consequences for society and ecosystems associated to continental heat gains » as the authors claim, then it does not make sense. Climate model projections are not informed by their own simulations of the historical period. They are informed by comparison against independent observations retrieved from the real world. So, to support their objectives the authors should provide an estimate of the inland water heat uptake that is derived from observations in a way or another (using forcing from reanalysis for example?)

Third I find that the uncertainty estimates are in general largely overlooked over the whole paper. In the case of the ground heat uptake, we are left at the end of the paper with a new estimate of the ground heat uptake with a very low uncertainty range ($\pm 0.8 \text{ mW}\cdot\text{m}^{-2}$). This small uncertainty range only accounts for errors in the thermal diffusivity, errors in the thermal conductivity and errors in the reference profile (through the bootstrap approach). But it does not account for any sources of systematic uncertainty

such as the poor and inhomogeneous distribution of the subsurface temperature profiles. Given the high sensitivity of the ground heat uptake estimate to the aggregation technique, the poor distribution of subsurface data is certainly the dominant factor of uncertainty here. Thus the very small uncertainty range of $\pm 0.8 \text{ mW.m}^{-2}$ is dubious. In the case of the permafrost heat uptake the uncertainty range does not account for many sources of systematic uncertainties as well. In particular the estimate is done with a unique permafrost model. Permafrost models show very large differences. At least, the use of another (or several) model would give insights on the level of this potentially large source of systematic uncertainties. In the case of the inland water heat uptake there is simply no information on how the uncertainty is derived.

For these reasons I think the paper is not ready for publication as it is. I think it needs a substantial amount of work to answer the important points I raised before.

I add below a list of additional comments

L46: what do you mean by "consistently"

L53: "high latent heat of fusion": high compared to what?

L95: the Xibalba logs are poorly and in-homogeneously distributed. Have you estimated the biases that could be caused by this in-homogeneous distribution? This is probably a leading source of uncertainty. You should at least estimate the order of magnitude of this source of systematic uncertainty and acknowledge it in the paper.

L102: the reference period for the calculation of the quasi equilibrium is precisely during the little ice age when land heat uptake was probably negative. This is potentially an issue for the inversion as it may bias high the anomalies with respect to the quasi equilibrium (since the quasi equilibrium you chose was a cold transient response to the little ice age rather than an equilibrium). Have you analyzed this possibility? Do you have an idea of the potential error induced by the fact that the reference period is during the little ice age rather than during an equilibrated period?

L130: same remark as for I 95: the bootstrap approach quantifies the uncertainty due to errors in the thermal diffusivity, errors in the thermal conductivity and errors in the reference profile. But what about the systematic errors coming from the in-homogeneous and poor distribution of profiles? This source of uncertainty probably dominates over the others. Can you elaborate on this? Provide a first estimate of this systematic error?

L145: the permafrost heat storage is derived from a model. But to which extent can we trust this model to represent the actual Permafrost? You do not provide any information on the validation of the model against observations. What confidence do we have in such a model?

L160: you are using a unique permafrost model. What about comparing against other independent models to get insight on the amplitude of potential sources of systematic uncertainty related to your model?

L172: why not using a forcing from reanalysis rather than ESM? This would be much closer to the real world. You claim further that the heat uptake estimate is important to inform projections of the future climate. If so, you need to get observational estimates of the heat uptake rather than model estimates. I don't understand the rationale here to use ESM forcing rather than reanalyses forcing

L199: How do you account for river depth?

L199: how do you compute the uncertainty of your inland water heat uptake estimate?

L204: the new uncertainty range is one order of magnitude smaller!!! This is huge! Especially for uncertainty. How do you explain that?

L204: the very small uncertainty of the present study is such that your result is inconsistent with your previous estimate in von Shuckmann et al. How do you explain that? The inconsistency between both results means that one or the other or both estimates are wrong!! Which one is wrong then? The present study estimate or your previous study estimate? The paragraph L202 to L212 recall the method used in von Shuckmann et al. 2020 and the method used here to aggregate the data. But it is inconclusive on which aggregation method should be trusted. Since the two methods yield inconsistent results, we need to understand where the problem is, which number should

be trusted and why we should trust it rather than the other.

L215: I find dubious that the different inversion technique and the different number of vertical profiles are enough to explain a change of the ground heat uptake by a factor 2 between Beltrami 2002 and this study. Either there is a misunderstanding of the real causes for the difference between both estimates or it means that land heat uptake is highly sensitive to the number of vertical profiles. It brings me back to my previous question: is there an important bias due to the poor and in-homogeneous sampling of the vertical profiles. A good test would be to take the same profiles as Beltrami 2002 and re estimate the land heat uptake with the inversion developed here and check whether you find the same result

L262-264 your new result agrees with your old result but for wrong reasons!! It is because you were biased in the ground heat estimate and here the bias is compensated by the new reservoirs you are adding in (permafrost and lakes). The right conclusion is that you find a ground heat uptake that is significantly different from the previous one . You should acknowledge that clearly and explain why. Can you elaborate on that?

L270 paragraph 4: I don't understand the point of this paragraph. Indeed we know that land heat uptake has numerous impacts on society and ecosystem. But it does not mean we need to estimate the land heat uptake to anticipate those impacts. In practice impacts on society and ecosystem are not derived from estimates of the land heat uptake. They are rather estimated from the output of climate models which use as input CO₂ concentrations and which tune their model against surface temperature and global EEI at TOA. So, in which way estimating land heat uptake will help to improve climate models and anticipate impacts on society or ecosystems. We should rather focus on improving the land surface models that are embedded in climate models, shouldn't we?

The only interest I see in estimating land heat uptake or permafrost heat uptake is to derive indicators for public awareness. Is that what you want to do? If so, you should state it clearly

L322 : I have the same remark: I don't see how the magnitude of land heat uptake inform on future warming and climate change. Future warming and climate change are given by

climate models and climate model just don't work with land heat uptake. So please elaborate to explain what you mean here

L325: An interest I see in estimating land heat uptake is to derive an observational benchmark against which climate model could be validated. But in this case you would need to derive observation only estimates of land heat uptake. That would be probably more suitable to the objective of informing projections of future warming