Dear Reviewer,

We thank you for your thorough and constructive feedback. This file provides a complete documentation of the changes made in response to each of your comments. Reviewer's comments are shown in normal text, author responses are shown in bold, italic, blue text.

Reviewer 3

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 12mW.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 propsoed 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

Please, note that the bootstrapping aggregation method has been extensively described and analysed in Cuesta-Valero et al. (2022), including a comparison with the aggregation method used in von Schuckmann et al. (2020). This reference was a preprint at the time of submitting our manuscript, but now it has been peer-reviewed and it is fully published.

Of course, we consider that the ground heat storage estimate in this paper is better than the one published in von Schuckmann et al. (2020) because of the reasons outlined here and in Cuesta-Valero et al. (2022).

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?)

We agree with this point. Nevertheless, we have no alternative right now. ISIMIP currently is the only source of multi-lake-model estimates available globally. The reason for using forcing from climate models instead of reanalysis is that in ISIMIP2 such reanalysis-driven runs simply have not been conducted. Moreover, if they would have been conducted, they would have stopped in the recent past (2015 in ISIMIP2a and 2019 in ISIMIP3a), thus even then we would be missing the most recent years. Furthermore, while the climate models do not capture natural variability, they have been bias-adjusted, and they are designed to capture long term meteorological trends (that

drive inland changes in water conditions).

As discussed on the manuscript, this is an aspect of the study to be improved in the future, but we think there is no immediate solution.

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 (±0.8mW.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 ±0.8mW.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.

Previous works have concluded that the distribution of subsurface temperature profiles is enough to provide with hemispheric and global averages considering profiles (Beltrami et al., 2004; Pollack et al., 2004), pseudo-proxy experiments using climate simulations (e.g., Beltrami et al., 2006; González-Rouco et al., 2006; García-García et al., 2016; Melo-Aguilar et al., 2020), and CRU TS data (Cuesta-Valero et al., 2021). Therefore, we consider the incomplete distribution of profiles to be a minor source of uncertainty. Regarding the width of the uncertainty range for ground heat flux, please see again Cuesta-Valero et al. (2022) and our answer to the first comment. We have added a sentence explaining why we think the spatial distribution of subsurface temperature profiles is not a significant limitation in the new version of the paper.

In the case of permafrost, uncertainties arise from the different stratigraphies and snow cover characteristics considered, trying to address all the poorly known, or directly unknown, configurations of ground ice, water and snow in the Arctic. Indeed, considering different permafrost models would allow us to include the uncertainty due to the different model physics. Nevertheless, our results already present a wide uncertainty range that is unlikely to be exceeded using further models. Please see Sections 2.2 and 5 for more details.

Uncertainties in inland waters heat storage are estimated from the multimodel ensemble analysed here. Concretely, the best estimate consists in the average of all sixteen lake model simulations, with the uncertainty range being defined by the standard deviation. Since the ensemble consist of four different global lake models, each driven by four bias-corrected GCM simulations, providing the climate forcing, the estimates include both the uncertainties related to structural differences in the lake models, and to the different climate trajectories simulated by the driving GCMs. We have included a line in Section 2.3 explaining this point in the new version of the text.

Standard error propagation methods are applied to drive the uncertainty of the continental heat storage from the different uncertainty estimates of the three components. Nevertheless, we recognize that this is not a robust approach given the differences in the factors contributing to the uncertainties in the three components of the continental heat storage. We have included a discussion about this in the text.

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.

We respectfully disagree with the reviewer. We are well aware of the limitations of the analysis, we detail them in the manuscript, and provide a plan for reducing current limitations in future iterations of the analysis.

Arguably, the main limitation of the analysis is the extensive use of models to provide estimates for subsystems without adequate in-situ measurements. Unfortunately, the lack of adequate observations is not going to disappear in the short term, but we think that model simulations can be used in the meantime as best-guesses. This is not the ideal situation but a sort-term solution to inform the scientific community about the thermal state of the surface and subsurface of the continental landmasses, as well as to estimate the Earth heat inventory as precisely as possible.

We have added a new paragraph in the Conclusions section clearly stating that the current estimates of continental heat storage should be improved in future iterations of this analysis, and we have included a discussion about the different uncertainties in the estimates of continental heat storage. We think that these two additions enhance the clarity of the current analysis and provide with a clear path for future iterations of these project.

I add below a list of additional comments

L46: what do you mean by “consistently”

We mean that the land term of the Earth heat inventory has been the second largest term after the ocean in previous analyses. We have changed this sentence to improve the clarity.

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

We refer here to the high energy required to melt a certain amount of water in comparison to the energy required to warm it. We have clarified this point in the new version of the manuscript.

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.

Please, check our answer to the third comment above. Several works have shown before that the global distribution of subsurface temperature profiles is complete enough to represent the long-term evolution of surface conditions at the global scale.

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?

Indeed, the period of reference for borehole inversions include part of the Little Ice Age (LIA, 1300-1850). Nevertheless, we consider the potential effect of LIA in our estimates of ground heat storage to be small. As indicated in the manuscript, we estimate heat storage as accumulated heat flux since 1960, that is, in our estimates flux is 0 in 1960, thus the LIA signature in the estimated heat storage should be irrelevant. There should be an effect on the flux histories retrieved from logs containing LIA signatures, but since LIA was not a spatially homogeneous process, not all profiles will include this signature (e.g., Beltrami et al., 2003).

Furthermore, the number of eigenvalues retained in the solution also limits the presence of the LIA signature in the inversions (Melo-Aguilar et al., 2020). Since we used the two highest eigenvalues in our inversions, the effect of LIA in the inversions will probably be attenuated. Therefore, we conclude that LIA has not a relevant role in our results.

Another issue with a possible change of depths to estimate the quasi-equilibrium profiles is the fact

that the number of logs quickly decreases with depth requirements. That is, a change in the depth range to provide estimates relative to a period before the LIA would result in a markedly lower number of profiles. And a change towards shallower depths risks including the transient signal in ground temperatures due to the beginning of the industrialization, biasing the determination of the quasi-equilibrium profile.

For these reasons, we consider the effect of LIA to be small, and the current depth range to estimate the quasi-equilibrium profiles of 200-300 m as the one that avoids the transient signal of the Industrial Revolution on the profiles while maximizing the number of logs contributing to the global ground heat storage.

L130: same remark as for L95: 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?

Please, check our answers to previous comments.

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?

The model has been compared to ground surface temperature measurements at 82 different permafrost stations from the Global Terrestrial Network for Permafrost (GTN-P) covering the period 2007-2016 (Langer et al., 2022). The root mean squared error between the simulation and the measured temperatures is reported to be $2.2\ \mathrm{K}$, ***with a warm bias of*** $0.6\ \mathrm{K}$, ***a performance comparable or better than other model analyses. For a detailed evaluation of the model, please check the indicated reference. We have also included a couple of lines with these results in the new version of the text.***

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?

Please, check our answers to the previous comments. We know that this is an important issue that shall be mitigated in the next iteration of this analysis.

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

The rationale here is as follows: we want to have an ensemble of multiple lake models to remove the effect of structural lake model bias. The ensemble is produced by a community of lake modellers usually after years of discussion and planning, thus it takes a substantial amount of time to implement design changes in ISIMIP protocols. The point raised by the reviewer is a good argument for pushing for reanalysis-driven simulations in the next iteration of ISIMIP, ISIMIP3a. But not all modellers would be interested in this type of simulations, and in any case ISIMIP3a will cover until 2019, so the produced experiments would not cover the recent past.

Unfortunately, there is no immediate alternative, as ISIMIP currently is the only source of multi-lake-model estimates available globally.

L199: How do you account for river depth?

River depth is dealt with within both global hydrological models (WaterGAP2 and MATSIRO) in their simulation of the total water stored in rivers. In WaterGAP2 for example, river water storage of a grid cell is calculated based on the hydraulic radius, which is based on the actual discharge and empirical relationships between river depth and width at bankfull conditions (Müller Schmied et al., 2014).

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

Please, check out answer to previous comments regarding this point.

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

In short, previous works considering several subsurface temperature profiles overestimated the 95 % *confidence interval, as their aggregation methods were not correct from a statistical point of view. For a detailed analysis and a comparison between the new bootstrapping aggregation technique and previous methods, check Cuesta-Valero et al. (2022). We have reinforced this explanation in the new version of the manuscript too.*

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.

Please, check our answer to the previous comments. The new estimate is the better one because of the reasons outlined above.

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 Beltarmi 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

Redoing the analysis of (Beltrami et al., 2002) (BE02 hereinafter) is not completely possible, as Xibalbá profiles are truncated at $300\ \mathrm{m}$***, and profiles in BE02 were used with full depth. Nevertheless, we can try to mimic the methodology of this analysis within certain limitations. There are 910 Xibalbá profiles measured before 2002, in comparison to the 616 logs reported in BE02. Inverting these 910 selected profiles with a step change of 50 years for the solutions (as in BE02) leads to a ground heat flux of*** $58\ \mathrm{mW\,m^{-2}}$ ***for the period 1950-2000, which is more similar to the*** $\sim 40\ \mathrm{mW\,m^{-2}}$ ***in BE02 than to the*** $\sim 85\ \mathrm{mW\,m^{-2}}$ ***reported for 1960-2020 on the text. The remaining difference can be explained by I) the almost 300 additional logs in comparison with BE02, and II) the fact that logs were not truncated to the same depth in BE02, thus each log had a different reference for estimating the quasi-equilibrium temperature profile. These results are consistent with what we indicated in the previous version of the paper.***

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?

Indeed, we obtain a different ground heat storage than in von Schuckmann et al. (2020), as we have pointed out in the manuscript. Also, please check the detailed analysis in the reference Cuesta-Valero et al. (2022) indicated on the text. We have changed the text in order to improve the clarity of this point.

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?

In the mentioned paragraph, we did not mean that estimates of ground heat storage are required to quantify or to anticipate the impacts, but that climate simulations suggest that ground heat storage is going to keep increasing in the near future, even in low emission scenarios, as well as the impacts associated to heat storage. We have rewritten the conflicting sentences to make that clear.

Also, quantifying ground heat storage helps to identify shortcomings in climate models, particularly in land surface model components. For example, previous works have shown that climate simulations underestimate ground heat storage because their land surface models are too shallow, and therefore the represented volume of the continental subsurface is insufficient to store the right amount of heat. This bias in the thermal state of the subsurface also affects the simulated subsurface temperatures, as well as the represented amount of permafrost in the model. There is a discussion about this point in the Conclusions section of the manuscript.

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

We respectfully disagree with the reviewer about this point. We consider that estimating the heat stored in all climate subsystems is also important to quantify the evolution of the Earth energy imbalance at top-of-the-atmosphere, as explained in von Schuckmann et al. (2020) or Chapter 7 of the sixth assessment report of the IPCC (Forster et al., 2021). Of course, the ocean accounts for ~ 90 % ***of the total heat storage but ignoring the rest of the subsystems imply to add a bias of*** ~ 10 % ***to the estimate, a bias that can be avoided.***

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

Indeed, a better wording was possible for this sentence. We have rewritten it in the new version of the manuscript.

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

We completely agree with the reviewer. This is the beginning of a collaboration to provide with such estimate, but while we investigate the way of obtaining such observation-based estimates, we are forced to relay in reanalysis and models. As there seems to be a confusion regarding the long-term goal of this collaboration, we have provided with a new final paragraph clearly stating our plans for the future.

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