Review #2

General comments

The presented study provides estimates of ocean heat uptake and the Earth's energy imbalance, mainly focusing on the space geodetic retrieval (from radar altimetry and GRACE gravimetry). The topic is timely and important, and the material in the paper provides a good basis for further scientific discussions and explorations. Emphasis is laid on the more exhaustive error analysis compared to previous research of the same topic.

IMHO, the paper still contains areas for improvement before a publication is justified. I'll highlight these below.

recommendation to editor: Major revision

Main points:

* How does the choice of averaging region affect the estimates? I realize that the inclination and Argo coverage prohibit high latitude estimates, but the consequence is that the reported errors do not take into account the omission of the high latitude signals. Since the main selling point of this paper is the computation of the errors, it would actually be an added value if the authors could try to quantify this high latitude omission error in a (stochastic, i.e. from models) way. I realize this adds extra work, but I think it would be very fitting in this paper.

Our uncertainty estimates indeed do not account for the sensitivity of the method to the averaging and collocation of spatial geodetic data, or to the filtering of the GOHC time series. We focused at this stage on the uncertainty only related to the propagation of the measurement errors from space geodetic measurements to OHC and EEI times series assuming that the method or the space data sampling (in space and in time) is not a source of error. The same approach was done for the sea-level rise uncertainty calculation at global and regional scales by Ablain et al. (2019) and Prandi et al. (2021) where only altimetry measurement errors are considered. However, it would be relevant in the future to measure the sensitivity of the method (e.g. averaging, filtering), but also the fact that the EEH does not represent the deep ocean or is not given at the very near coast, or the fact that the geodetic space measurements do not measure the very high latitudes. This is what we have proposed in the perspective of this study.

* Related to this, is that the restoring of the ocean/atmosphere products in GRACE and consequently subtracting the estimates from IB corrected altimetry should be described in more detail as it can falsely introduce atmospheric signals in the averages of global ocean mass. Are there still ocean averaged atmospheric components in the GRACE data? Or have these been corrected before comparing with altimetry?

GRACE solutions ensemble is corrected from atmospheric effects using the spatial mean of the GAD product. Therefore, the GRACE data provide an estimate of the ocean mass variations, theoretically free from atmospheric effects. However, the correction applied is only as accurate as the model used. If there were significant atmospheric effects not accounted for by the GAD, they would remain in the estimated ocean mass changes. These sources of error may not be adequately described in our ensemble, which may require attention in future studies. Details are available in the Appendix A - l487.

* Uncertainties of EEH. The method to estimate the expansion efficiency of heat comes from a paper which I don't (yet) have access to, so more clarifications may be needed. For example, it is claimed that the EEH is most sensitive to salinity and at the same time it is claimed that the error bars are reduced because of the Argo data. Argo data is known to have considerable biases and errors in the salinity estimates so I wonder whether the authors could better clarify why they think the errors are now considerable smaller.

For the calculation of EEH at global scale, monthly 3D in situ temperature and salinity fields from various 11 Argo solutions were used to compute the ratio between GMTSL change and GOHC change. These monthly ratios are averaged over time, then averaged together to provide a global EEH estimate of 0.145 ± 0.001 m YJ−1 representative of the 0–2000 m ocean column for the period 2005-2015, excluding marginal seas and areas located above 66° N and 66° S. This regional extent corresponds to the spatial extent that is regularly sampled by the in situ Argo network. The global EEH estimated here is in good agreement with previous estimates of 0.12 ± 0.01 m YJ−1 (equivalent to 0.52 W m−2/mm yr−1) representative of the 0–2000 m ocean column over 1955–2010 from in situ observations (Levitus et al., 2012) and 0.15 ± 0.03 m YJ−1 for the full ocean depth over 1972–2008 (Church et al. 2011). Its uncertainty is however much smaller than in previous studies because our EEH computation is based on the Argo network that has a precise estimate of ocean temperature and salinity down to 2000 m depth and our estimate relies only on effective measurements that were processed homogeneously (eg. interpolated data are excluded, the same horizontal and vertical mask is used). Previous studies from Levitus et al. (2012) and Church et al. (2011) used an ensemble of temperature and salinity products that covered the whole ocean over the past decades with in-filled data where measurements are lacking. The differences in the in-filled data explain the large uncertainty in Levitus et al. (2012) and Church et al. (2011). Here we restricted the study to the region and the time span covered by Argo. We expect estimates of EEH to be very precise when the calculation is restricted over the sampled region because EEH accuracy depends only on T,S measurement accuracy and the TEOS10 equation accuracy (and both are very accurate at levels below 0.1%).

Note that our accurate estimate of EEH does not prevent it to be biased by systematic effect not accounted for. In particular the systematic effect of the sampling of Argo which is not fully global (measurements are sparser above 66° latitude and below 2000m depth). Because of this effect our estimate of the global EEH is likely biased by a few percent. It is likely biased high because the bottom layer, below 2000m depth, is less salty than upper layers which would result in a slightly lower global EEH estimate if it was accounted for in the computation. We dedicated a whole paragraph in the new version of the manuscript to explain this in detail. In particular we identify clearly that our estimate of EEH is precise but potentially biased high. See section 3.3.

Concerning the dependence of EEH to salinity, when we claim that "that the EEH is most sensitive to salinity" we mean that EEH is relatively more sensitive to salinity than to temperature. However, at global scale, salinity changes are very small, so the EEH changes as well.

In addition, to avoid Argo issues on salinity measurements, we used salinity until 2015 only. It prevents from the recent drifts in the Argo record of salinity (see for example Ponte et al. 2021).

→ The sentence dealing with the EEH uncertainty was completed (section 3.3)

* The authors may be aware that in parallel to this paper a similar one has come out: Hakuba et al 2021 (https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2021GL093624) They actually find a larger EEI (0.9 W/m2). This paper would in fact be a nice opportunity to put these numbers in perspective (e.g. why do they arrive at a higher number?). Since the GIA

correction on GRACE has such a large effect I indeed do wonder whether the GIA corrections can be one of the culprits.

Over a similar period (2005-2015), results obtained by Hakuba et al., 2021 (trends_global.xlsx provided with the article) are in agreement with ours:

- Hakuba et al., 2021: EEI mean: 0.77 W/m² [-0.51 W/m²- 1.05 W/m²] (2005-2015)
- Marti et al., 2021: 0.77+ 0.24 W/m² (January 2005 - December 2015)

However, major differences are noted with regards to the input data. First, the value of the global EEH differs from one article to the other and the impact of this value is strong. Then, GRACE data used in Hakuba et al., 2021 is a mascon solution (JPL) corrected for the GIA using 5000 solutions from Caron et al. 2018. Our spherical harmonic solutions ensemble relies on two different GIA corrections: Caron et al., 2018 and ICE6G-D. This might indeed explain the difference between the ocean mass trends (over 2005-2015, 2.41 mm/yr [1.98 - 2.75] for Hakuba et al. 2021 and 1.80 +/- 0.21 mm/yr for our ensemble solution). It further stresses the fact that estimating uncertainties due to post-processing choices in GRACE solution is necessary to be able to compare different products.

→ The results from Hakuba et al., 2021 are now mentioned in section 6.

Minor remarks:

Intro l33 "play a minor role" is the other 10% meant or something else? Yes , it is. The other reservoirs storing 10% of the energy excess have a low contribution to the EEI variations at the timescales of interest.

l72: "innovative algorithms" I don't want to temper your enthousiasm, but maybe "new" or "original" is better here (let the reader judge themselves whether these are innovative). The main novelty of the present work is the implementation of a robust algorithm to propagate the uncertainties.

l89 Neglible Or assumed to be neglible? In the former case maybe provide an estimate from the cited papers.

Several authors indeed conclude that the salinity variations are negligible in the computation of the global mean sea level change, contributing by about 1 % to the global mean sea level change.

-> We have added this order of magnitude and an additional reference.

l172-l173 "It is however .. content" I don't understand this sentence The section was reformulated.

l190 "if included here" -> if it would be allowed to absorb a fraction of the ocean heat uptake Rephrased.

l245: What kind of numeric differencing scheme is used here? Forward differencing scheme (numpy.diff) → We have specified this in the text.

"256: "is implicitly accounted for in the local EEH coefficients". I'm trying to get my head around this, and understand how the salinity effect would be implicitly accounted for. Please clarify (maybe add a formula for to explain this)

The integrated expansion efficiency of heat is computed with the data of 11 Argo products over 2005-2015. (We start in 2005 because the coverage of Argo is global in 2005. We end in 2015 because salinity issues in the Argo products start at the end of 2015 see for example Ponte et al. 2021). We use the mean salinity field of the 11 Argo products over 2005-2015 to estimate the IEEH (instead of the reference salinity at 35psu). So the IEEH is computed taking into account the density of sea water at the level of the 2005-2015 mean salinity. It means that the density effect (i.e. the halosteric effect) of the mean salinity over 2005-2015 is accounted for in IEEH. However any halosteric effect induced by a change of salinity wrt to the mean over 2005-2015 is not accounted for. So the map, that is computed as a mean over 2005-2015 accounts for salinity but any other map, computed over a different period, would miss a small halosteric contribution from salinity anomaly wrt to the mean salinity over 2005-2015.

→ The explanations were reformulated and completed.

l289-299 "Thus on the overall ... we neglect it here". I don't really understand the word "correlation" in this context, and why it would be an argument not to apply a GIA correction to altimetry. I suggest to address this together with the comment of the other reviewer on geocentric sea level rise

There is a misunderstanding, because the altimetry is actually corrected from GIA as explained in the dedicated section about the sea-level calculation. In this section we only speak about the correlation of the errors of the GIA and ITRF in the altimetry and gravimetry datasets. We explain that we do the sum of the GMSL and GMOM covariance matrices assuming the errors on GMSL and GMOM are independent of each other. We simply discuss that this assumption is also made for the contribution of GIA and ITRF errors in the sea level and ocean mass estimates. We indeed explain that the level of these errors in each dataset (GMSL and GMOM) is not of the same order and therefore are very poorly correlated.

→ In order to clarify this section, we slightly reformulated the sentence in line 289 without changing the main idea of this paragraph.

l409 power plant -> power plants corrected