Review Referee #2

First of all, we would like to thank the reviewer for its relevant questions and comments that will help improve the manuscript, both in terms of content and form.

Marti et al. provide an update on the global ocean heat content change since 1993 based on a geodetic approach. The new time series shows a positive trend of OHC-based EEI, suggesting an acceleration of global heating. This geodetic-approach-based estimate shows a reasonably good agreement with other in situ observations. The paper is good and well-written, and the geodetic approach provides a promising method to estimate OHC and EEI, which should appear in the state of the ocean report. I have several comments on a couple of things and hope to improve this paper:

Abstract: a trend of 0.75 Wm-2 does not suggest an acceleration of ocean warming, instead, it is “ocean warming”. The number you derived later: 0.29 [0.04;0.56] W m-2 decade-1 for LEGOS-Magellium over 1993-2022 is the “acceleration”. please rewrite the abstract.

In the abstract we want to focus on the acceleration of the EEI, not the global OHC one.

→We will correct the abstract to display the warming acceleration (ie EEI trend).

Abstract: The last sentence “This study highlights the importance of rigorously estimating uncertainties based on space geodetic data to robustly assess EEI changes.”. This seems not a key point of this study. The uncertainty quantification is always referring to other papers and not thoroughly introduced in this paper. I would say something to highlight the importance of a combination of multiple lines of evidence to depict ocean warming and its acceleration, because clearly, the geodetic approach shows the value and could be a nice addition to the traditional approaches.

→ the end of the abstract will be reviewed to focus on the importance of analysing various estimates and their uncertainties, and of the added value of the space geodetic method, to eventually robustly assess the EEI changes.

Introduction, page-1, line 20-24: please give a balanced and more inclusive citation for each group of methods.

For the in situ approach, we eventually only kept von Schuckmann et al., 2022 as it is the latest publication regarding this topic that aims at presenting results from the scientific community. Stammer et al., 2016 provides a review of data assimilation issues and challenges for ocean reanalysis for climate applications and seems relevant enough to illustrate the method.

→ Hakuba et al, 2021 will be added for the space geodetic approach.

Page-2, line 48-50: this paragraph seems out of place, I suggest putting it after page-3, line 74, i.e. the derivation of GOHC should come after the regional OHC is derived. And, another thing is: please also clarify how you make regional patterns before GRACE (you used the individual contributions to manometric sea level from Greenland, Antarctica, mountain glaciers and from terrestrial water storage before 2002, are you using regional fingerprints or just global time series?)

- The details given on line 48-50 were located here to highlight the difference in the method compared to Marti et al., 2022 based on the progress made in Rousseau et

al., under review, and to show that GOHC change is no longer estimated directly from global steric level change as in Marti et al. 2022, but as the sum of regional OHC changes. In this manner, it helped us to explain the calculation of variables (SSL, IEEH) at regional scales.

→ so as not to interrupt the thread in the method between the description of the input data and the calculation of the SSL, we propose to slightly reword the lines 48-50 and move them higher up, before the description of the input data.

- Regarding the barystatic component of sea level beyond the GRACE(-FO) period, we relied on the work performed in the frame of the ESA-CCI program about the sea level budget closure (Horwath et al. 2022). It is estimated by summing several contributions:
  - The contribution from global glacier mass changes assessed by a global glacier model
  - The contribution from Greenland Ice Sheet mass changes assessed by satellite radar altimetry and by GRACE
  - The contribution from Antarctic Ice Sheet mass changes assessed by satellite radar altimetry and by GRACE
  - The contribution from terrestrial water storage anomalies assessed by the global hydrological model WaterGAP (Water Global Assessment and Prognosis)

Time series is provided at global scale, and it is then considered homogeneous over the global ocean. This approximation is motivated by the fact that the contribution of the mass to spatial variations of sea level change is only significant in closed seas and very high latitudes (Piecuch and Ponte, 2011; Piecuch et al., 2013) which are not present in our study. Moreover a sensitivity test was conducted over the GRACE era to assess the impact on the GOHC and EEI of considering homogeneous manometric sea level maps instead of regional patterns. The results showed a difference of around 1% in the GOHC trend and no more than 3% difference in the interannual variations of the EEI.

→ we will specify in the manuscript that the SLBC data are defined at global scale.

Page-2, line 51: Is the salinity effect only neglected in GOHC calculation, not for regional OHC calculation?

We neglected the salinity effect in the OHC, and therefore in the GOHC calculation, due to the lack of in situ based halosteric sea level change data which is both available over the study period and corrected for the drift due to recent anomalies on conductivity sensors. Moreover, the study concentrates on the OHC on a global scale, to which the halosteric contribution to sea level is negligible compared with the 2 other contributions.

→ eventually we might opt to remove the sentences related to recent instrumental drifts because it brings more confusion than clarifications to the reader.

Page-3, line 85-86: I don't understand why performing the filter step at regional scales is low on GOHC estimate but more on the EEI estimate. What does exactly this mean? Isn't EEI is simply derived from GOHC according to Eq.(1)?

The Lanczos filter that we are using is not perfectly linear (two lobes). The effect of this non-linearity is accentuated when we differentiate the GOHC into EEI.

→ For ease of reading we will reformulate to simplify this portion related to the filtering step.

Page-4, line 100-119: the presentation and introduction of these datasets are a bit of chaos and not organized: different data products should be better grouped according to the data/methodology difference, so the readers can better understand their differences. Please see Cheng et al. 2022 (https://doi.org/10.1038/s43017-022-00345-1) for reference.

Page-4, line 111-115: Not all five data centers are “Argo centers”. They are fundamentally different from “Argo resulting GOHC change estimates” because ISAS, EN4 and NOAA used all available in situ data, but JAMSTEC and SIO are purely Argo. They should be better grouped and introduced.

→ The data for comparisons will be better described. We will first introduce GOHC computed by ourselves from gridded fields based on in situ data, starting with Argo-based ones. We will then introduce the 2 OMI CORA and CORA-2011 also based on in situ data (in the next version of the manuscript, we decided to simplify it and remove ARMOR3D dataset as it results from a mixed method). GCOS ensemble, gathering most of those datasets. Finally, we will introduce another space geodetic GOHC (Hakuba et al., 2021) .

When they are not appropriate, references to “Argo-based data” in the section 2 (IEEH description) and in the section 3-Results will also be removed.

Page-4, line 111-115: all five data products are used in the GCOS estimate, so they are not independent from the GCOS estimate. Please discuss this issue.

→ This information and associated comment will be given (currently only CORA and CORA2011 inclusion within the GCOS ensemble is mentioned on line 110-111).

Page-4, line 119-121: I don’t understand this, many of the data products listed above are monthly data (ISAS, EN4, NOAA, JAMSTEC, SIO, CORA, ARMOR-3D), why should you need to interpolate to monthly time series??

Page-4, lines 116-117 indicate that we chose to subsample all the GOHC monthly datasets to fit with the annual time-series from GCOS and CMEMS-OMIs (particularly relevant for this publication in the OSR).

→ “ both GCOS ensemble and OMIs are made up of yearly .. so that the derivative is made on a monthly time scale”: this portion needs to be clarified. We will explain that GOHC annual time series are used. They are then linearly interpolated to monthly time steps before applying the same method as applied for our product to compute the EEI.

Page-5, line 136: “Over their respective area of data availability”: please clarify which domain you are referring to?

By “their respective area of data availability”, we meant that the LEGOS-Magellium GOHC was computed considering the maximum spatial extent our method can reach at the moment (~86%), while for GCOS the dataset has its own and fixed spatial extent reference (~76%) - for GCOS, we do not have control on the spatial extent, the GOHC change time series associated with von Schuckman et al., 2023 publication was used)

→ In order to facilitate the comparison between GCOS and Legos-Magellium GOHC trends in Figure 1, we will update Figure 1 to plot the LEGOS-Magellium GOHC change obtained over the same spatial extent as the GCOS dataset. We will also revise the comment.

→ we will update Figure 2 and divide it into 2 parts as shown on the figure below. On the left, GOHC is estimated considering 86% of the ocean surface (the maximum spatial extent that can be taken into account with our approach currently), while for results on the right data was considered over the GCOS mask.

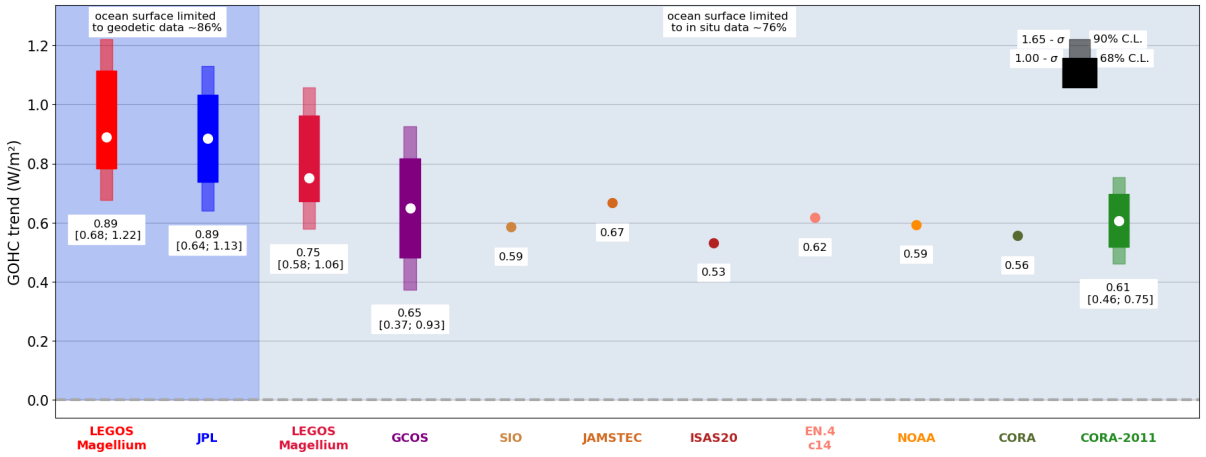

Figure Ré-1: next version of the Figure 2 of the manuscript

Page-5, line 146-148: it is not correct: different GOHC estimates used in this study has different ocean-land mask, so their areas are different. And there is no “Argo mask” because different Argo data center also has different masks. Please clarify what is this referring to.

We called “Argo mask”, the mask defined in Marti et al. 2022 (figure 1). It corresponds to the common availability area of 11 in situ gridded T/S datasets and covers 79% of the total ocean surface.

Apart from GOHC changes estimates on which we do not have control (OMIs, GCOS), we can ensure that the area considered to compute the different GOHC change estimates is homogeneous using a common mask. Even if the ocean-land border used by the different in situ data centers are different, in such a way we are able to discard the main discrepancies on GOHC estimates related to spatial extent of their input data.

→ In the next version of the manuscript we will no longer refer to the Argo mask when discussing the different GOHC trends because it brings confusion. Only two masks will be mentioned: see figure above R2-1.

Page-5, line 149-154: please also discuss the variability in these time series: are the inter-annual variation meaningful (i.e. ENSO) or just noises?

Some signals can result from errors, but most of them are eventually not noise as there is correlation between them. These variations can be related to the main ocean modes (PDO, ENSO) or the interannual variability of the aerosol content in the atmosphere (volcanic eruptions), and are not well known and documented.

→ We will add a sentence in the next manuscript to discuss this aspect.

Table 1: I don’t know what is “Argo mask” you are referring to: mask of which Argo product?

See above, “Argo mask” is an internal definition that we will no longer use