

Earth Syst. Sci. Data Discuss., referee comment RC1  
<https://doi.org/10.5194/essd-2021-220-RC1>, 2021  
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## Comment on **essd-2021-220**

Anonymous Referee #1

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Referee comment on "Monitoring the ocean heat content change and the Earth energy imbalance from space altimetry and space gravimetry" by Florence Marti et al., Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2021-220-RC1>, 2021

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Review of ***Monitoring the ocean heat content change and the Earth energy imbalance from space altimetry and space gravimetry*** by Marti et al.

The presented dataset and article are highly relevant for in-depth research on Earth's energy imbalance (EEI), its uncertainty, and causes for (internal) variability. The authors derive ocean heat storage from geodetic satellite observations, namely altimetry and gravimetric observations that lend themselves to derive the ocean's thermal expansion as their residual – at least in the global mean. While this approach has been successful in previous studies to constrain EEI – which by definition is a global metric – the authors go so far as to also present a regional ocean heat storage dataset based on the geodetic data. The gridded heat content/storage data is flawed as the authors admit, but may inspire the community to help improve and produce higher quality products of similar sort.

While the paper is important, well written and places rigor on uncertainty quantification, I have identified a few major weaknesses that I would like to see addressed before publication. Editorial comments are at the end of this review.

- **Expansion efficiency of heat:**

- I agree that gridded Argo data are useful to derive the EEH, assuming that the 0-2000m and full column efficiencies are very similar, at least in the global mean. However, by using Argo data in the form of EEH to derive GOHC from the geodetic space observations, your geodetic GOHC estimate is not fully independent from Argo data. How does that affect the validation results against Argo GOHC and EEI? On line 444 and 474 you state the independent nature of the datasets, but this is not truly the case – if I understand correctly, an EEH derived from ocean observations will always be needed to translate satellite-based steric changes to GOHC.
- Your mean EEH value amounts to 0.145 m/YJ (or  $0.43 \text{ Wm}^{-2}/\text{mmyr}^{-1}$ ) and is therefore larger than the Levitus (0.12 m/YJ or  $0.52 \text{ Wm}^{-2}/\text{mmyr}^{-1}$ ) estimate for the 0-2000m ocean column. What is the reason for this? Dealing with such small

numbers, I find this discrepancy rather large. If we assume a steric sea level change of  $1.6 \text{ mmyr}^{-1}$ , your EEH yields an ocean heat storage of  $0.69 \text{ Wm}^{-2}$ ; but using the Levitus number, the heat storage becomes  $0.83 \text{ Wm}^{-2}$ . I find this difference quite dramatic. Furthermore, the tiny EEH uncertainty derived here does not acknowledge that a smaller EEH of  $0.12 \text{ m/YJ}$  might be possible.

- How is the EEH uncertainty derived? With  $\pm 0.001 \text{ m/YJ}$  it is one order of magnitude smaller than the uncertainties by Church et al. and Levitus et al. Why is that? Even if the temporal variability in EEH is small as the authors state, I would expect a certain degree of variability, which might impact (increase?) the EEH uncertainty? Calculating the EEH on a monthly basis, I would expect a lot of noise, it might be helpful to see a figure of the EEH timeseries as part of the appendix, and to use this timeseries to derive the uncertainty (in case that is not what the authors already do).
  - Figure 1, spatial distribution of EEH: Something might be wrong with this figure. EEH increases with temperature and salinity. This means EEH must be high in the tropics/sub tropics and low near the poles. At the moment, the map depicts a pattern that is opposite to expectations.
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- **Calculations & geophysical corrections:**
    - I would appreciate a table that summarizes the values for geocentric GMSL, GMSL (after correction), GOML, EEI etc. Without the table, it has been difficult to follow your calculations. For example, the value for the GMTSL is not provided in the paper.
    - I backed out the GMTSL value using the authors'  $\text{EEH} = 0.43 \text{ Wm}^{-2}/\text{mmyr}^{-1}$  and  $\text{GHOC} (2002-2016) = 0.7 \text{ Wm}^{-2}$ . The GMTSL should be  $1.63 \text{ mmyr}^{-1}$ . With this and the GMOM trend of  $1.83 \text{ mmyr}^{-2}$ , I arrive at an GMSL trend of  $3.46 \text{ mmyr}^{-1}$ . For the same period, the authors report a GMSL trend of  $3.57 \text{ mmyr}^{-2}$ . Please rectify the  $0.1 \text{ mmyr}^{-1}$
    - The authors are not reporting the geocentric GMSL change. Adding to their reported (relative) GMSL trend ( $3.57 \text{ mmyr}^{-2}$ ) the GIA and ocean bottom deformation - which should sum up to  $-0.38 \text{ mmyr}^{-1}$  - the geocentric GMSL must be near  $3.19 \text{ mmyr}^{-1}$ . This seems very low. I would expect a trend of  $\sim 3.5 \text{ mmyr}^{-1}$  according to the AVISO data. I suspect the authors might have confused the corrections. The sign of the correction for ocean floor deformation should be negative ( $-0.1 \text{ mmyr}^{-1}$ ) so that:  
 $\text{GMSL} = \text{geocentric GMSL} - \text{GIA} - \text{GRD} = \text{geocentric GMSL} + 0.38 \text{ mmyr}^{-1}$
    - By the way, GMTSL is not spelled out, but I believe it refers to Global Mean Total Steric sea Level?
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- **EEI uncertainties:**
    - I appreciate the authors thinking about the impact high frequencies in GOHC variability have on EEI derivation. However, the authors do not justify the low-pass filter cut-off period of 3 years sufficiently. Lines 100-103 would need a reference and maybe even a demonstration of what the filter does to the GOHC time series. Furthermore, I wonder what slightly different filters or cut-off periods may look like? Did the authors do an assessment of different filters? How does EEI look like if no filters are applied? I understand such an assessment might be beyond the scope of this article, but I think this matter is important and needs to be addressed, at least briefly.
    - On line 248, the authors state the high frequency (to be filtered out) is only visible in the altimetry and not the gravimetry data. Can this be shown in a figure in the

- appendix? Would it be better to only filter GMSL before combining with GMOM?
- By applying the smoothing filter to GOHC before deriving EEI, the authors had to empirically estimate EEI uncertainty. What would the EEI uncertainty be if no filter was applied and the errors could be propagated as for GOHC? I expect the uncertainty is sensitive to the filter applied, which means the error due to the filtering needs to be included? Perhaps, this could be based on a sensitivity analysis (using different filters/cut-offs). For this article, a discussion about the impact of filtering GOHC on the EEI uncertainty estimate would be appreciated.
  - Compared to Meyssignac et al. (2019), the EEI uncertainty seems to have almost halved. I do not agree that this is solely due to the decreased EEH uncertainty - and even if, I suspect the EEH uncertainty is underestimated I think the role of the low-pass filter needs to be unveiled. I recommend to discuss and summarize potential reasons as to why the uncertainty range has decreased.
- **Regional OHC:**
- As the authors state correctly, the regional OHC estimates from geodetic data require a correction for regional halosteric effects. My question is then, instead of comparing OHC between geodetic and in-situ data, why not compare the total steric changes instead? I understand there is an issue with salinity drift in some or most of the in-situ datasets. But there are some, e.g., the SIO dataset, which do not show a global drift in halosteric sea level change. I think using steric changes would be a clearer and less convoluted comparison between the geodetic and in-situ retrievals.
  - Line 253: I do not understand how the salinity effect is accounted for by the EEH map over 2005-2015. The EEH is defined as the ratio of thermosteric (sea level rise due to thermal expansion) over OHC changes. You would need thermosteric sea level change to back out OHC using these EEH estimates, but the geodetic data provide you with total steric changes. No matter the time period, you would need information on the regional halosteric changes.
  - I do not believe the regional/gridded OHC as it stands is useful for the community to study OHC variability. This article and the public datasets require a disclaimer on the use of the gridded OHC derived from space geodesy. Or the authors could provide the total steric changes instead or in addition to the OHC map.
  - Besides, I doubt the EEH map (Figure 1) is correct, which affects the gridded OHC. By providing the steric changes instead (or separately with the EEH map), users could use their own EEH maps and assumptions to derive OHC.
  - Line 447: I do not remember seeing a quantification of the high correlation of the OHC map with climate mode fingerprints in models or with the Argo data. Has this been assessed? If not, I would weaken the statement by saying the patterns look/appear similar qualitatively. At the very least, this article should show the Argo-based map of OHC change to be able to qualitatively compare the geodetic approach against, which it does not in its present form.

#### **Editorial and Minor comments:**

- line 16: "... estimating the ocean heat content (OHC) change provides an accurate proxy of the EEI."
- line 29: "It is challenging to estimate the EEI from TOA radiation fluxes since it is two orders of magnitude smaller..."

- line 37: delete "counted"
- line 43: "The direct measurement approach relies..."
- line 48: "The ocean net flux approach assesses the radiative ..."
- line 54: "except for the polar regions"
- line 56-59: This sentence is long and convoluted, please edit.
- line 68: "Reducing uncertainty as much as possible." Can you explain/provide examples of how to reduce uncertainties? I'm a bit taken aback by this statement. I hope the message is not to make the uncertainty look smaller than it is by favoring certain approach over others?
- line 68: What do the authors mean by "extending the spatial and temporal coverage of OHC change? What exactly is being extended and how?"
- line 71: change not changes
- line 112: referred to as
- line 117: "for" not "from"
- line 121: - 1 mmyr<sup>-1</sup>
- line 160: How do you make the post-processing choices? Do you account for the probability of the choices? How?
- line 163: What are these state-of-the-art estimates? References?
- line 194: Please indicate here that the regional OHC derived does not include a correction for regional halosteric effects and should be used with caution.
- Line 242: "taking" not "making the difference"
- Line 244: please state the surface area in numbers
- Line 250: "Monthly steric sea level changes are directly..." delete "grids"
- Line 262: I was under the impression EEH was calculated as the ratio of thermo-steric sea level change and OHC change. How exactly do you incorporate salinity climatologies?
- Line 264: What is meant by "counterpart" – do you mean "downside"?
- Line 274: delete "consisting in"
- Line 276: first step consists of
- Line 291: Please spell out GMTSL
- Line 316 "the final operation applies the formulation...":
- Line 344: What about comparing the GMOM trend against independent estimates of land ice melt and land water storage change? Independent melt estimates for the Greenland and Antarctic ice sheets as well as glacier do exist in the literature, e.g., the IMBIE project. I expect sea level budget assessments like for example by the WCRP 2018 provide estimates of these melt rates and should be able to indicate closure of the ocean mass budget.
- Line 408: "integrated over". Delete "For information,"
- Line 409: power plants:
- Line 415: I do not understand how this conclusion can be drawn. Monthly variability has been smoothed out and the monthly variations are not shown. If this is discussed here, a figure would help. And how do you conclude that an uncertainty of 0.8 to 1 is large? Compared to what? CERES? Please clarify.
- Line 428: "... important signals that sign at global scale." I do not follow, please edit.
- Line 439: I do not fully agree that this is the first study of its kind. There has been work by Llovel et al. (2014), Dieng et al. (2015), and others making use of geodetic datasets to estimate the oceans' thermal expansion.
- Lines 473-478: I would delete the sentence "It enables independent comparisons which is the unique method to robustly check and validate the final EEI estimates". It does not add much.