

We thank the anonymous referee#1 for the comments on our manuscript submitted to *ocean science*. We appreciate the thoughtful and constructive feedback on the paper. We have addressed all concerns in the revised manuscript, as documented below in our point-by-point responses (in blue) to reviewer comments (in black)

Review of the Manuscript “Observed and simulated full-depth ocean heat content changes for 1970-2005” by Lijing Cheng et al.

This is an interesting paper, which along with other recently published and in-press publications stress the importance of the ocean heat content calculations for the climate monitoring and provides an update of both the observation- and model-based ocean heat content estimates.

General comments

1) I believe the paper should more strongly underline the conclusion that the existing (CMIP5) models are characterized by the extremely large spread in their estimates of the total OHC. This spread exceeds by far the estimated observational uncertainty in the OHC change even for the upper 0-700m where the model drift is expected to be less important compared to the deeper layer. This finding poses the question about the ability of the existing models to simulate the OHC change.

Reply: We agree that the paper should more strongly underline the large spread of CMIP5 models. Therefore, we made the following revisions:

(1). In the abstract, a sentence is added to highlight this issue:” *The CMIP5 models show large spread in OHC changes, suggesting that some models are not state-of-the-art and require further improvements. However, the ensemble median has excellent agreement with our observational estimate*”

(2). In Page10Lines1-3. This paragraph is to highlight the large uncertainty of climate models. One more sentence is added “*The spread of CMIP5 models far exceeds the estimated observational uncertainty in the OHC changes even for the upper 0-700m where the model drift is expected to be less important compared to the deeper layer.*”

We think it is better not to “**poses the question about the ability of the existing models to simulate the OHC change**” as suggested by Reviewer#1. Because some of the models are not good according to our assessments but some others are consistent with observations, so a more precise conclusion is preferred.

2) There is a small discussion in the manuscript regarding the OHC decrease after the major volcano eruptions (page 6). The OHC time series shows several other OHC decrease events of similar magnitude (1976 2001, 2004, 2007) which are obviously not connected to any big volcanic eruption. Consequently, the interpretation of the OHC-decrease events near 1983 and 1993 as being most probably due to the volcanic eruptions allows alternative explanations as well.

Reply: This is a good point. We can't rule out the possibility that the OHC-decrease events in 1983/1993 are due to other forces, based on the observation-based analyses because all of the signals are mixed together.

A discussion is added in Page6Line30-Page7Line6:” *But our observational analyses can not exclude the possibility that the unforced ocean variability (such as ENSO) and the insufficiency of data coverage (which could induce spurious inter-annual OHC change) are fully or partly responsible for the values calculated above, which requires more careful model-based studies in the future. Moreover, it is also suggested that volcanic eruptions can trigger an El Niño like response in the ocean, which is another possible explanation (Mann et al., 2005).*”

Minor comments

P.1 Lines 25ff: “Numerous efforts have been done to detect the historical OHC change“. The list of references which follows in parenthesis is incomplete: for instance the earlier estimates done by the Levitus group are missing, as well as the estimates provided by Gouretski&Koltermann, 2007. The reader thus gets a wrong impression that the cited studies are the only available in the literature where the OHC estimates have been reported. Please, extend the list of relevant studies.

Reply: We added two more references in the revised manuscript (*Levitus et al., 2005; Gouretski and Koltermann, 2007*), and added “for example” to note that we only listed parts of but the latest references (since it is not a review paper).

P.2, lines 16ff. Please, reformulate the sentence: for instance, the following piece of text “to construct the climatology based on data with near-global data coverage” definitely needs improvement.

Reply: This sentence is modified to “*Because the choice of reference climatology to compute anomalies can lead to errors due to the sparseness and inhomogeneity of the historical ocean sampling (Lyman and Johnson, 2014; Cheng and Zhu, 2015), it is preferable to use the climatology which is constructed based on data with near-global data coverage (Cheng and Zhu, 2015), i.e., during the recent years in the Argo period.*”

P.2, line 30: “The gridded method”. Though this term has already been used by the authors in the earlier paper, I suggest to change the name to, say, “multiple grid method”, or “flexible grid method”, because otherwise the name of the method states that the method itself is subjected to gridding, whereas it is the data what is gridded.

Reply: Thanks for the comment. The gridded method is changed to “Flexible-grid Method” in the revised manuscript.

Data and Method section: there is no mention here about the usage (or not usage) of the Mechanical bathythermograph data. This is a large data set, with the data being biased. Gouretski&Reseghetti 2010 provided a correction scheme for the MBT data, which successfully reduces the overall bias.

Reply: We have corrected the MBT bias by using the method provided in *Ishii and Kimoto, (2009)*. We mentioned this point in Data and Method section (Page3Line19):” *MBT bias is corrected using the method provided in Ishii and Kimoto, (2009).*”.

Page 4, line 3: change to “less land and no boundary currents”

Reply: Done

Page 4, line 14ff: “The five ensemble members ... sample plausible uncertainties” - 1) how the uncertainties can be sampled and 2) what is the method to decide if they are plausible???

Reply: This sentence might be misleading. Now it is revised to “*The five ensemble members in ORAS4 approximately represent the uncertainties in the wind forcing, observation coverage, and the deep ocean.*”

Page 4 line 21: change “from choice” to “from the choice”

Reply: Done

Page 4, line 29ff: Is the assumption about the proportional increase in heat uptake in the deep ocean justified?

Reply: It is very difficult to provide a full justification for this assumption. Because (1). There is no sufficient observations pre-1990 below 2000m (2). The models have large discrepancies about the deep ocean changes below 2000m. So how much the deep ocean changed is unknown and it is difficult to have a direct estimate.

We simply provided two justifications: (1) The upper 7000m oceans are mostly controlled by the wind, while the deep oceans (700m-bottom) are mostly controlled by the meridional overturning circulation. So the OHC changes at 700-2000m and 2000-bottom may share some similarities. (2) And we provided the uncertainties involved, suggesting that this assumption will not significantly impact our key conclusion.” *We show that the difference of this lower and upper bound of the 700m-bottom OHC change is equal to ~13% (~10%) of the full-depth OHC change during 1970-1991 (1970-2005), which indicates the maximum error induced by this assumption.”*

Page 5, line 9: “i.g. spurious long-term trends arising the slow model adjustment....” - are the words “due to” missing here??

Reply: Modified to “*spurious long-term trends arising due to the slow model adjustment to the initial conditions and/or imperfect representation of the energy budget*”

Page 6 line 10: “The total OHC change ... has increased ...” - please, indicate the time period here.

Reply: The time period “*from 1970 to 2005*” is indicated here.

Page 9, line 24: change “is shown to small” to “is shown to be small”

Reply: Done