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Climate of the Past Discussions

Interactive comment on "Variability of the ocean heat content during the last millennium – an assessment with the ECHO-g Model" *by* P. Ortega et al.

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The authors are grateful to the four anonymous referees for their helpful and constructive comments. We think that the new version of the article benefits substantially from the changes that have been proposed. All suggestions were carefully considered and most of them were included in the new manuscript. Some long comments were divided into different sub-points to better organise the response and to ease readability. We have also tried to reply in detail each of the major and minor comments. The full text of the review is reproduced below, with an answer following each comment. A pdf file highlighting changes (deletions/addition) to the previous version of the manuscript

is also sent to the editorial office in order to facilitate a detailed screening of changes. Additional supplementary figures have been included to attend the reviewers concerns.

Reply to Reviewer 1

General Comment: *This manuscript examines some elements of the variability in ocean heat content using a suite of coupled AOGCM simulations with (mostly) realistic forcing. This is a critically important issue and has received insufficient attention to date. Therefore, the present manuscript is welcomed. I did find the manuscript rather heavy going, being difficult to read and some of the diagrams being very difficult to see and identify the important features. However, overall, the manuscript is useful and I would recommend publication after attention to the detailed comments below.*

General reply: The authors appreciate the positive opinion of the reviewer about this manuscript and the interesting comments that follow. We are aware that some parts in the manuscript can be dense and hard to read, and some figures difficult to see and interpret. In this respect, we have reworked the manuscript, trying to provide further information regarding the different methodologies employed (e.g. wavelet coherence analyses, estimation of variance explained by the forcings) and the assumptions made (e.g. correction of permanent drifts, and trends due to initial thermal imbalance), improving the text when discussion was unclear, incomplete or unconvincing (see for example Comments 7, 15, 19 and 20), and modifying the size of the figures to allow better visibility, as well as their captions for a better guidance to the reader.

Finally, following Comment 1.2 of the 4th reviewer the removal of linear trends in CTRL has been reassessed. Changes in the figures are almost imperceptible as the impact of the previous detrending is small in the upper ocean, where the analysis is focused.

SPECIFIC COMMENTS:

<u>Comment 1:</u> (Page 4225, line 5) Add recent Gleckler et al. reference Gleckler, P. J., B. D. Santer, et al. (2012). "Human-induced global ocean warming on multidecadal timescales." Nature Clim. Change 2(7): 524-529.

Reply: The reference has been added.

<u>Comment 2:</u> (Page 4225, Line 14) This flattening is controversial. For example von Schuckmann and Le Traon indicate an ongoing ocean warming. von Schuckmann, K. and P. Y. Le Traon (2011). "How well can we derive Global Ocean Indicators from Argo data?" Ocean Sci. 7(6): 783-791.

Reply: This is now discussed in the text.

Somewhat more debated are observational estimates reporting a flattening of the upper OHC trend since 2003, despite the steady increase in GHG concentrations and the subsequent radiative imbalance at the top of the atmosphere (Trenberth and Fasullo, 2010), which has been challenged by recent Argo data suggesting a continuous ocean warming during the same period (Schuckmann and Traon, 2011). [Starts in Page 3, line 13][1]

<u>Comment 3:</u> (Page 4225, Line 20) While these papers offer additional ways to balance the Earth's energy budget, the magnitude of these two mechanisms is small and they have not demonstrated that they quantitatively can balance the Earth's energy budget.

Reply: A comment on this has been added to the discussion.

However, it is not clear if the proposed mechanisms can quantitatively balance the global energy budget. [Starts in Page 3, line 26]

<u>Comment 4:</u> (Page 4226, Line 2) Should refer to the first detection of these

[1] We will always refer to the manuscript with deletions and additions since page numbering in the clean final version will change after the journal's online edition.

instrumental problems. Gouretski, V. and K. P. Koltermann (2007). "How much is the ocean really warming?" Geophysical Research Letters 34: L01610, doi:01610.01029/02006GL027834..

Reply: The reference appears now in the text.

<u>Comment 5:</u> (Page 4228, Line 6) 2002 in the references.

Reply: References have been corrected.

<u>Comment 6:</u> (Page 4232, line 29) Note the Jevrejeva et al. time series has only 3 gauges to base their global average on before about 1860. A lot of subsequent discussion seems to depend on the accuracy of this record. I think some caution is required.

Reply: Now this is explicitly said in the text to illustrate to what extent the first half of Jevrejeva's estimates is reliable.

Caution is required for its interpretation as a global estimate since only three gauges were included before 1860, and these are biased to Northern Europe (i.e. Amsterdam, Liverpool and Stockholm) [Starts in Page 13, line 12]

<u>Comment 7:</u> (Page 4233) I am not really sure what the meaning of the first two paragraphs on this page is

Reply: The whole section 3.2 has been considerably changed as several reviewers found unconvincing the assumptions and procedure for correcting the effect of the initial thermal imbalance from 1700 onwards, and therefore the final comparison of trends in SL and thermal expansion. We have changed the focus of the section now mainly devoted to discuss the effect of thermal imbalance, simplifying the previous discussion, and removing some parts, like the final comparison of trends. The two paragraphs mentioned above have been also substantially modified. In the current manuscript we just present the two records for SL, highlighting the common aspects like the flattened

SL variations during the 18th century and the gradual sea level rise from 1800 onwards. Finally, we discuss the representation of these features in the simulations. We expect that after these changes the final message is more clear.

A comparison with proxy data is now established (Figure 5a). Given the lack of direct paleo records of past OHC variability, estimates of SL variability are used instead. The longest instrumental SL record is computed from tide gauge observations since 1700 AD (Jevrejeva et al, 2008, hereafter Jev08). Caution is required for its interpretation as a global estimate since only three gauges were included before 1860, and these are biased to Northern Europe (i.e. Amsterdam, Liverpool and Stockholm). The record shows evidence that acceleration of sea level rise began about 200 years ago, and was preceded by a century of comparatively flattened SL variations (brown line in Figure 5a). A local reconstruction from salt-marsh sediments in North Carolina (Kemp et al, 2011, hereafter Kemp11), summarised in an idealised manner by the yellow filled curve in Figure 5a, is in broad agreement with estimates in Jev08, and shows stable SL variability during the same period. However, it should also be noted that the exact timing at which acceleration of SLR starts in the proxy of Kemp11 can be sensitive to the method considered for subsidence adjustment (See Fig. 1 in Grinsted et al, 2011). Given the large temperature trends in the deep ocean even in FOR2 (Figure 4), that would introduce a drift in the simulated thermal expansion, and keeping in mind that none of the other contributions to sea level change, such as the melting of land glaciers or the polar ice caps, is represented in the ECHO-G model, SL estimates are directly compared with the simulated OHC700 (Figure 5a). Despite the misrepresented processes, and the constraint to the upper 700m, simulations and reconstructions show similar variability from 1500 AD onwards. Note in particular the similar onset for the recent upper ocean warming and the observed SLR two centuries ago. The largest discrepancies occur during the first five centuries of the millennium, where none of the forced simulations follow the steady increase of SL in the reconstruction of Kemp11. However, in both simulations, and in particular in FOR2, there is a great degree of coherence between the curves of OHC700 and the total radiative forcing (Figures 5a,b),

which cannot explain the initial trend in SL. This good agreement in FOR2 also suggests that its initial thermal imbalance has a negligible impact at this particular depth. Therefore, from now on, analyses covering the last millennium will only be addressed with FOR2, and will be restrained to the upper 700m of the ocean. FOR1, together with the scenario simulations, will be considered only for analyses centered on the observational period, when the thermal initial instabilities are mostly balanced, as suggested by the great accordance between both forced runs. [Starts in Page 13, line 9]

<u>Comment 8:</u> (Page 4233, line 4) Kemp et al. and Jevrejeva et al. are not in good agreement. Indeed there has been a published exchange between the two groups. Perhaps of importance here is the difference in the time when the acceleration of sea level started.

Reply: As far as we know the exchange between both groups is published in the papers Grinsted et al. (2011) and Kemp et al. (2011b). They mostly focus on the uncertainties related to the subsidence adjustment in the SL record from Kemp et al. (2011a). It seems that these uncertainties, under alternative assumptions for subsidence adjustment, can become considerably large before year 1000AD. In either case, this period is not discussed in our manuscript. Indeed we mainly focus our comparison on the last 300 years. Yet, it is true that the timing for acceleration of sea level rise can also vary under these alternative adjustments (See Fig. 1 in Grinsted et al., 2011), and therefore this caveat is now mentioned in the text.

However, it should also be noted that the exact timing at which acceleration of SLR starts in the reconstruction of Kemp11 can be sensitive to the method considered for subsidence adjustment (See Fig. 1 in Grinsted et al, 2011). [Starts in Page 13, line 19]

<u>Comment 9:</u> (Page 4233, Line 7 and line 10) On line 7 it says 18th century and then on line 10 it says 1800-1900 (i.e. the 19th century). Which is it? See also the Caption to Figure 4..

Reply: The mistake was in line 10, as it is actually the period 1700-1800 (18th century) that was chosen for the detrending. In either case, this part of the discussion has been removed in the rewriting of section 3.2.

Comment 10: (Page 4234, line 7) Since 1955, the models seem to indicate the thermal expansion was about a third of the observed rise.

Reply: This part has been also removed from the discussion, as we are no longer confident in the previous technique for correcting the initial trends due to thermal imbalance.

Comment 11: (Page 4234, Line 9) This 75% seems misleading. You can only arrive at this number if the potential ice sheet contribution is ignored.

Reply: This value was actually taken from the last IPCC report (see http://www.ipcc.ch/publications_and_data/ar4/wg1/en/ch10s10-es-8-sea-level.html). However, as for the two previous comments, this result is not included in the new manuscript.

Comment 12: (Page 4234, Line 13) Suggest "(Fig. 3), we will focus on"

Reply: The suggestion has been included.

Comment 13: (Page 4234, Line 15) Should this be Figure 1?

Reply: Indeed we actually refer to Figure 5a for the OHC and Figure 5b for the net radiative forcing. Both show a good agreement during the last millennium, particularly for FOR2 for which ocean initial conditions are closer to thermal equilibrium. We now specify which are the two panels to be compared.

OHC700 shows a good correspondence with variations in the net radiative forcing (Figures 5a and 5b, respectively). [Starts in Page 16, line 20]

Comment 14: (Page 4234, Line 20) Wasn't El Chichon in 1982?

Reply: The reviewer is right. Date has been corrected.

Comment 15: (Page 4236, Line14-29) Suggest this needs to be rewritten to bring out the most important points.

Reply: The paragraph has been rewritten following the suggestion of the reviewer.

The reasons for both the different latitudinal OHC700 response to the forcing and the larger sensitivity in the extratropics are now explored. Net heat into the ocean will result from the integration of all heat fluxes at the surface, and these can be controlled by changes in cloud coverage. Regression patterns in Figure 7 illustrate how these variables are affected by the total radiative forcing. Interestingly, a general decrease in extratropic cloud coverage (Figure 8a) follows the positive changes in the RF. This same pattern, but with opposite sign, characterizes the changes in shortwave radiation (Figure 8b), as it responds inversely to variations in cloud albedo, thus explaining the larger OHC700 values at midlatitudes in Figure 7. Other important features to note are a remarkable increase of cloud coverage over the Ross Sea and the convection regions of the North Atlantic, that reduces the shortwave incoming radiation at the surface... [Starts in Page 19, line 4]

Comment 16: (Page 4237, Line 17) Delete "Besides"

Reply: "Besides" has been deleted.

Comment 17: (Page 4238, line 22) Delete

Reply: The whole sentence has been deleted.

Comment 18: (Page 4239, Line 1) Proposed by whom?

Reply: This sentence referred to the previous section. It has been rephrased for clarity.

Globally, the impact of internal climate variability on OHC700 appears to be largest at interannual to decadal timescales, especially during periods of low volcanic activity, when the effect of the radiative forcing is mainly observed at lower frequencies (inter-decadal to secular; see Figure 9). [Starts in Page 22, line 5]

Comment 19: *(Page 4240, Lines 1-20) Not sure what this adds at the moment. It is hardly new or surprising that there are regional distributions of heat with climate modes. What this could add would be to more accurately quantify them, and how the global averaged heat content has fluctuated in the model with climate modes. I suggest the material here needs strengthening.*

Reply: The reviewer made a good point. The main purpose of that section was not clear, and some analysis regarding the potential impact of climate indices on global OHC was missing. We have improved the discussion in this respect, including also a new table (Table 3) with the coefficients of determination between the global OHC700 and the respective indices at high and low frequencies (for periods $<$ and $>$ of 5 yr, respectively), thus illustrating how much of the global OHC variance is explained at those frequencies by each index.

The impact of all indices on global OHC700 is assessed at two different frequency ranges: periods above and periods below 5 yrs (Table 3). At the higher frequencies, that mostly accounts for interannual events, only ENSO has a significant impact on OHC700, with R^2 values close to 0.15 both in OBS and FOR1+A2, and slightly lower in the whole FOR2 simulation. This finding is in line with Willis et al (2004) that identified the signal of intense ENSO events in the global OHC integral. At the lowest frequencies, observations show some covariability between the AMO and the total OHC700 (R^2=0.20), although the R^2 coefficient is not significant when the sample size is corrected to take into account the timeseries autocorrelation. In the simulations, only the NAO explains some meaningful part of the global OHC700 at low frequencies, although it could be just a consequence of a delayed NAO response to the forcing that will be later discussed. To further compare the influence of each index in the simulations and

observations,their corresponding regional OHC700 fingerprints are illustrated in the regression patterns of Figure 12. [Starts in Page 23, line 18]

Comment 20: *(Page 4242, Lines 1-3) What is the significance of this. Perhaps it needs to be explained better?*

Reply: Wavelet coherence (WTC) is a powerful tool to identify common aspects of variability and phase relationships between two different series. However no interpretation in terms of causality should be made, as it is rather an exploratory technique to test proposed linking mechanisms with a physical basis (Grinsted et al., 2004). Although Monte Carlo methods are used to assess the statistical significance against red noise backgrounds, there is still the risk of obtaining significant values by simple chance. In our case, we used the WTC to explore the potential frequencies at which both the forcing and the climate indices may have an impact on the OHC, using also information on their phase relationship to test to what extent the possible linkages remain robust throughout time: as long as phase is constant and OHC lags, the physical links can be considered plausible. We admit that in the previous manuscript such warning for the interpretation was not provided, thus leaving the reader with the impression that the linking mechanisms discussed were all statistically significant. Therefore, we have now included a comprehensive discussion on the methodology and its limitations previous to the description of the different results.

A wavelet coherence (WTC) analysis (Torrence and Compo, 1998) is used to investigate the common spectral features between the OHC700 and the different forcings throughout the last millennium (Figures 9 and 10). For all practical purposes, wavelet coherence can be regarded as a localized correlation coefficient but in time frequency space. All plots are generated with the software package provided in Grinsted et al (2004), following their recommendations for the choice of the wavelet transform (i.e. Morlet function) and scale resolution (i.e. 10 scales per octave). Likewise, significance is assessed using a Monte Carlo approach based on an ensemble of 1000 surrogate dataset pairs with the same AR1 coefficients as the input timeseries. Note that wavelet coherence should be used and interpreted with caution. It is mostly an exploratory technique to test proposed linking mechanisms with a physical basis. Herein, WTC is used to explore the potential frequencies at which the forcing may have an impact on the OHC. Yet, no definitive inferences on causality can be drawn. To help in the identification of robust linkages throughout time, phase-relationship is also computed at each time and frequency. The angle between the arrow and the x-axis indicates the phase between the forcing and OHC700. Thus, in-phase relationships are represented by eastward arrows in the wavelet coherence plots. At a particular frequency band, whenever phase remains stable the physical link proposed will remain plausible. [Starts in Page 20, line 11]

Comment 21: (Page 4243, line 25) For the solar variability, there is a 20 year lag, when the oscillation has about a 11 year period. Can you explain please?

Reply: The 20 year lag seems to obey to the accumulated effect of two consecutive 11 yr cycles, as the signal penetrates slowly due to thermal inertia. Indeed, if the analysis is repeated for the period 1000-1700 (new dotted thin lines in Figure 11), when reconstructions of solar variability cease to have the 11 yr period, the 20 yr lag disappears and maximum correlations are observed almost in phase with the OHC changes. These new results are now discussed in the text.

This delayed response points to a gradual accumulation of the energy going into the ocean, the lag timescale being set by the cumulative effect of two consecutive 11 yr cycles and the increasing GHGs, respectively. Indeed, a similar analysis for the period 1000-1700, when neither anthropogenic GHGs nor 11-year solar cycles are included, shows leading times close to zero. [Starts in Page 21, line 21]

Comment 22: (Page 4244, line 14-15) This global signal seems to contradict the last line of the previous page when it says the signals are local. This needs a proper

discussion.

Reply: Following the suggestion in Comment 19, we now discuss in section 5.1 the global impacts of the different indices. And it turns out that indeed ENSO, the NAO and the AMO may potentially impact the global OHC700 at different timescales. Therefore, the statement in lines 14-15 seems to be valid. We have rephrased the line in the previous page to avoid the previous contradictory sense with respect to the global implications.

The fact that the phase of the relationships (arrows in Figure 15) remains stable throughout the whole simulation, with westward arrows in Figure 15b accounting for a North Pacific cooling and eastward arrows in Figure 15c representing a mid-latitude North Atlantic warming, both compatible with the corresponding OHC700 patterns in Figure 13, suggests that a real but intermittent modulation of the OHC700 by both indices may be taking place. [Starts in Page 26, line 5]

Comment 23: (Page 4246, Line 21: the year is 2000 not 200.

Reply: Now corrected in the bibliography

Comment 24: (Page 4251, Table 1) Sorry, I do not understand how the radiative forcing accounts for such a low percentage of variance. Doesn't heat have to be conserved in the climate system.

Reply: Under stationary conditions, the Earth's energy budget should be balanced. In our particular case, the analysis in Table 1 refers to the ocean, and more in particular to its upper 700 m (now explicitly said in the caption). Ocean heat fluxes at the surface will be certainly modulated by radiative forcings as these dominate the incoming short-wave radiation (responding to solar activity and volcanoes) and the incoming long-wave radiation (screened by the presence of GHGs in the atmosphere). However, there are also other terms not directly controlled by the radiative forcing (e.g. sensible and latent heat fluxes). In addition, as the focus is made on the upper 700m of the ocean, some non-negligible part of the surface energy input can be transferred and accumulated into the deeper ocean levels.

Comment 25: (Page 4253, Table 3) The observed thermal expansion coefficient is low compared with recent estimates. The early part of the period has very large uncertainties because of limited data. A weighted fit gives larger numbers.

Reply: As we explained in the response to Comment 7, the comparison and discussion of recent trends in SLR and thermal expansion (and therefore Table 3) have been removed from the manuscript. Yet, we agree that thermal expansion from observational estimates is subject to large uncertainties, in particular during the early years and in the deep ocean, where data coverage is sparse.

Comment 26: (Figure 1) The Mt Agung forcing seems to be small?

Reply: Indeed, estimates of the radiative effect of volcanic aerosols included in the simulations (Crowley, 2000, i.e.) shows a relatively low radiative forcing for Mt Agung as compared to the eruptions of El Chichon and Pinatubo. Note that for that reconstruction, estimates for the period 1960 to 1998 were taken from an updated version of the Northern Hemispheric radiative forcing in Sato et al. (1993), which for Mt Agung's eruption shows a really small amplitude if compared to the Southern Hemisphere. Indeed, a new reconstruction from Gao et al. (2008) and recents updates to the previous reconstruction (Crowley et al., 2008) all show comparable radiative forcings for the three aforementioned volcanoes (Schmidt et al., 2011). Since our simulations are forced with the forcing from Crowley (2000), they can only respond to a weak Mt Agung eruption. However, it is true that OHC700 observations in Fig. 2 seem to be more compatible with a larger Mt Agung eruption, as they show an important cooling during the 60s. This is now discussed in the text.

Note that the remarkable OHC reduction in OBS coinciding with Mt Agung's eruption

in 1963 is not reproduced by either FOR1 or FOR2. This is possibly due to an underestimation in the simulations of the associated volcanic forcing, as two more recent estimates (i.e. Gao et al, 2008; Crowley et al, 2008) suggest a larger magnitude for this eruption, being actually of comparable order to Pinatubo's. These two new reconstructions have helped to highlight important uncertainties in volcanic activity concerning even the instrumental period (Schmidt et al, 2011). [Starts in Page 10, line 6]

Comment 27: (Figures) The maps were indistinct and hard to see.

Reply: We have resized the figures to better fit the page dimensions, thus improving their resolution to help identify the different local aspects in the spatial maps. In figures of wavelet coherency arrows are now bigger, and less dense for a better visibility. In addition, legends have been relocated in all figures (e.g. Fig. 3 and Fig. 12) in which contour values near the Weddell Sea were hidden. Finally, many captions have been changed to provide a clearer description.

References

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