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Climate
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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.

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

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

Reply to Reviewer 3

General Comment: *This paper addresses an important topic in the broader arena of global climate change: namely ocean heat content and particularly through the past 1000 yrs. It is an area that we need more contributions, and to this end this work can make a contribution. However, I found the paper to be lacking in a couple of areas that need to be revised (some major and then also some more minor) before being suitable for publication. My primary concern with this paper lies in using a model (ECHO-G) that has applied heat and freshwater flux adjustments based on current observations (Levitus) and on a very short time scale (30 days) - at least according to Ortega et al, 2012 (OR12 in the paper) to 1100 yr runs that simulate the Last Millennium and 100 yrs in the future. The flux adjustments to late-20th Century and on short time scales suggest that changes and variability in ocean heat content due to forcings of the past 1000 yrs will be strongly constrained by this rather than sufficiently allowed to vary in a manner that can truly increase our understanding of ocean heat content during this time frame. Without discussion, explanation (or a clearer one than is presented?) and analysis of the effects of the constraints on ocean heat and ocean heat variability (and substantial drifts!) from the heat and freshwater flux adjustments to modern times (which appear to be SST and salinity relaxations?) on longer simulations, it is not possible to adequately evaluate the scientific results of this paper. Another, albeit smaller, concern are inconsistencies w.r.t. choices of which simulations to use for comparison, which ones to extend, how long to extend, etc. For example, observations from the 20th century from 1955-2010 are used, but both runs over the historical period only go to 1990 whereupon"future scenarios" are used. Then FOR1 is extended to 2000 (why not 2010?) - and yet it is FOR2 that is used for analysis of the Last Millennium (so* *why not extend FOR2 for historical time frame too)? And what indeed do the scenarios A2 and B2 represent? These are relatively minor (compared to above issue), however they are confusing and an explanation of why the choices were made (and of the scenarios A2 and B2 so that one doesn't have to go to references to find out what they represent) would help.*

General reply: The authors understand the main concerns of the reviewer. It is not easy to quantify the particular impact of flux adjustments on the validity of our results. Yet, their influence could be important. We have tried to shed light on their potential effect by producing Sup. Figure 2 to compare the actual heat flux adjustments with the net climatological fluxes. Flux corrections remain generally small with respect to the actual fluxes, in particular over the regions where the effect of local indices was analysed, thus pointing to a minor impact of the flux-corrections on heat uptake. We have also improved the final discussion, in which we account for the well-known biases inherent to this methodology, the main model shortcomings, and also we put our simulation in the context of other recent AOGCMs, most of them without flux adjustments. Further discussion on these aspects can be also found in the response to the general comment of the second reviewer. In the new manuscript we also discuss in larger extent the problems related with the initial thermal imbalance in the whole set of experiments. Regarding the experimental setup, we want to clarify that the commented inconsistencies arise from the fact that none of the simulations has been specifically designed for this analysis. They were originally conceived to provide a first evaluation of last millennium climate variability with an AOGCM (González-Rouco et al., 2003b), and have been widely analysed ever since (González-Rouco et al., 2003a, 2006, 2009a, 2011; von Storch et al., 2004; Zorita et al., 2003, 2005; Beltrami et al., 2006; Gouirand et al., 2007a,b; Stevens et al., 2007; Fernández-Donado et al., 2012). As currently we are unable to compute new simulations, or extend the existing ones, we had to find the better way to combine them in our analysis. The main limitation that we found is that both FOR1 and FOR2 finish in 1990, 20 years before present. That explains why we had to make use of the scenario simulations A2 and B2. In FOR1, we have values covering up to year 2000AD as that particular simulation was extended under the frame of the EU project SO&P. Although still insufficient, we have included it in the analysis as it helps to increase the overlap period with observations. We understand that the final experiment configuration is somewhat confusing and probably not the most adequate for the analysis. However, it still provides a valuable framework for the study of the last millennium. The description of the experiments has been now improved in the manuscript to help understand the reasons for the particular choices (see new Section 2.2).

Finally, please note that most figures in the manuscript have been redone after realizing that the removal of the linear trends in CTRL is no longer valid (See Comment 1.2 of the 4th reviewer). Fortunately, changes in the figures are almost imperceptible as the impact of this incorrect detrending is really small in the upper ocean, where the analysis is focused.

SPECIFIC COMMENTS:

Comment 1.1: (p. 4227 Line 7) does the HOPE-G ocean model include an ice model? How is sea ice simulated? Or is it?

Reply: HOPE-G incorporates a dynamic-thermodynamic routine that uses atmospheric fluxes to estimate sea ice and snow cover changes (Legutke and Voss, 1999). Sea ice is computed as a prognostic variable, that therefore can respond to changes in the climate system. This is now mentioned in the model description.

New text: The ocean component incorporates a dynamic-thermodynamic sea-ice module that uses atmospheric fluxes to estimate sea ice and snow cover changes (Legutke and Voss, 1999). Sea ice is computed as a prognostic variable, that can therefore respond to changes in the climate system. [It starts in Page 5, line 12][1]

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[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.

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Comment 1.2: (p. 4227, Lines 7-14) What effects do the model resolutions have on heat and freshwater fluxes as well as heat content (or mixing and sub-grid scale parameterizations)?

The coarse model resolution translates into the fact that an important number of (subgrid scale) processes are not resolved. These must hence be parameterised (see below). In particular, horizontal mixing includes both the shear-dependent and harmonic contributions. The shear-dependent eddy diffusivity is important only in the upper ocean and only in regions with very strong velocity shear. Elsewhere it remains more than one order of magnitude smaller than the harmonic diffusivity. Vertical mixing uses constant background diffusion and a Richardson-number dependent scheme similar to that proposed in Pacanowski and Philander (1981). All parameterisations of diffusion conserve heat and salt. The total coefficients for eddy diffusivity, which will pace the ocean heat uptake and downward transport, tend to be in good agreement with the low values estimated in the open ocean away from rough topography, but higher within the mixed layer as a result of wind stirring and unstable surface buoyancy forcing, especially in winter (Sup. Fig. 5). This could partially explain the low heat uptake efficiency in ECHO-G as compared to other more diffusive ocean models (See General comment from the second reviewer). Also applied are a mixed-layer treatment with additional surface vertical diffusion for weak surface stratification, and a parameterisation of sunlight penetration following an exponential decay and a maximum depth of 100 m. In addition, in cases of unstable stratification, convective adjustment must be applied. Each pair of vertically adjacent unstably stratified grid boxes is mixed, with heat and salt being conserved. Note that a summary of these parameterisations has been included in the description of the model (Section 2.1). Certain large-scale mechanisms may not be well represented by the model, like the western boundary currents and the location for its departure from the coast. Non-resolved dynamics lead to an underestimation of the strength of the subpolar gyre (15 Sv, not shown), clearly below the range of observations (25 to 40 Sv; e.g. Clarke, 1984). All these misrepresentations have a direct impact on the simulated heat and freshwater fluxes. Indeed, the maps of flux adjustments in Sup. Fig. 2 (bottom row) show important corrections in the convection regions of Labrador, the Norwegian and the Weddell Seas, as well as across the Gulfstream. Finally, orography is also poorly represented in some regions where specific processes can be important for the large-scale climate, such as deep water formation. The potential impact of these misrepresentations on OHC variability, and the implications to our findings are now partly discussed in the Section of conclusions.

New text: Furthermore, due to the coarse model resolution several sub-grid scale processes with a potential influence on the OHC cannot be resolved and must be therefore parameterised. In particular, horizontal mixing includes both the shear-dependent and harmonic contributions, with the first only important in the upper ocean and the regions with very strong velocity shear. Elsewhere it remains more than one order of magnitude smaller than the harmonic diffusivity. Vertical mixing uses constant background diffusion and a Richardson-number dependent scheme similar to that proposed in Pacanowski and Philander (1981). Note that all parameterisations of diffusion conserve heat and salt. Also, the total coefficients for eddy diffusivity, which will pace the ocean heat uptake and downward transport, tend to be in good agreement with the low values estimated in the open ocean away from rough topography, but higher within the mixed layer as a result of wind stirring and unstable surface buoyancy forcing, especially in winter (Wolff et al, 1997). Finally, in cases of unstable stratification, convective adjustment must be applied. Each pair of vertically adjacent unstably stratified grid boxes is mixed, with heat and salt being conserved. [It starts in Page 5, line 21]

New text 2: The major constraint relies on the use of flux adjustments (and more in particular heat flux corrections), since they can largely affect the ocean heat content and its regional patterns. However, a comparison of these terms with the actual fluxes at the surface (Sup. Table 1 and Sup. Fig. 2) shows that in overall, and in particular in the centers of action of ENSO, the AMO and the NAO, the magnitude of heat adjustments remains considerably smaller, thus suggesting that their probable impact on ocean heat uptake is also small (Sausen et al, 1988). In contrast, freshwater

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adjustments remain comparably important, particularly across the Gulf stream, thus evidencing that some ocean dynamics are misrepresented by the model, and thereby, casting doubt on the validity of our results for regions like the North Atlantic. [It starts in Page 28, line 28]

Comment 2: (pp. 4227-4228, Lines 25-Lines 7) The forcings over the Last Millennium and the next century are sufficiently important to this paper to put in a figure so that the reader can refer to them immediately rather than look them up in another paper. If, due to constraints on space, etc. require that a figure on the forcings is not included, referring to them as shown in Figures 8-9 as well as a sentence or two describing the basic premise of the scenarios (A2 and B2) will help (e.g."high emissions" or"low emissions" or some description giving at least an idea of what these scenarios represent). This is particularly important as the simulations using current conditions stop at 1990 (21 yrs ago) and it's not at all clear which scenario (A2 or B2) most closely resemble a continuation at least of historical conditions.

Reply: We understand the inconvenience of not including at the beginning a figure compiling all the individual forcings, as these are essential to understand the OHC changes throughout the last millennium. Such a figure appears, as noted by the reviewer, in several previous articles (González-Rouco et al., 2003b, 2009b; Ortega et al., 2012). In the previous manuscript we preferred not to included it to avoid the repetition, as all these forcings appeared later on in the wavelet coherence plots (old Figure 9). However, following the suggestion of the reviewer, it has been now included at the beginning of the new manuscript. We also admit that some extra information is necessary in Section 2 for a proper description of the forcings and the scenarios. All these problems have been fixed in the new version of the manuscript.

New text: ... two future scenario simulations (A2 and B2) that extend FOR1 until 2100 AD and corresponds respectively to projections of high and moderate GHGs emissions (Nakicenovic et al, 2000). [It starts in Page 6, line 15]

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<u>*Comment 3:*</u> *(p. 4230 Lines 17-20) How exactly where the % of variance explained by forcings calculated? Perhaps easily explained in a"methods" section?.*

Reply: What we actually compute is the coefficient of determination (denoted by R^2, and corresponding to the square of the correlation coefficient R). For a linear regression model R^2 represents the percentage of the total variance of the original data explained by the predictor. In our case, we are trying to understand the changes in global and basin-wide OHC (i.e. our predictand) in terms of the total radiative forcing (i.e. our predictor). The text and the caption in Table 1 have been rephrased to refer directly to this coefficient.

Rephrased version: Coefficients of determination (R^2) are compared separately before and after 1990 (Table 1). The OHC700 variance explained by the forcing, both for the simulations and observations, is found to increase dramatically from below 30% for 1955-1990 to about 70% in the subsequent 20 years. [It starts in Page 10, line 17]

Rephrased caption: Coefficient of determination R^2 between the OHC700 and the total equivalent radiative forcing during the periods 1955-2010, 1955-1990 and 1991-2010. In this particular case, the R^2 coefficient represents the fraction of OHC700 variance explained by the forcing, and is calculated for the whole ocean and also individually for the three major basins. [In Page 36]

<u>*Comment 4:*</u> *(p. 4231 Lines 2-15) Linear trends in OHC estimated from observations (discussed here and shown in Figure 2/ new Fig. 3) have stronger zonal gradients than simulations. Any ideas why? Likewise, there appear to be large discrepancies between obs and simulated OHC trends in the North Atlantic in regions of deep water formation - add a bit of discussion here (why, etc.) - not just AMO. The 1990s, in particular, have a large increase in AMOC in observations, decreases in temperature in the subpolar gyre, and corresponding changes in circulation (e.g. Levitus, 1990; M. K. Flatau, L. Talley and P. N. Niiler,2003, J. Climate 16; Hakkinen and Rhines, 2004, Science 23). These may be part of natural variability rather than trends... so some*

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discussion please.

Reply: It is true that some regions (like the tropical Pacific, or the North Atlantic) seem to show stronger zonal gradients for the linear trends in the observed OHC. The main problem, as also evidenced by the reviewer, is that during the two periods analysed with instrumental data (e.g. 1955-1990 and 1991-2010), the influence of internal processes (like ENSO, the AMO, the ocean circulation...) seems crucial to explain the different OHC700 trends. And these aspects of internal variability (e.g. the phases of the different modes of variability) are not necessarily similar in the simulations and observations.
Finally, we have reoriented the discussion, to make more emphasis on the role of these internal processes, and to discuss additional mechanisms as those suggested by the reviewer in this comment (subpolar gyre, AMOC,...).

Rephrased version: The spatial distribution of recent trends is now explored to identify robust features on the response to the forcing, and also local modulations by internal modes of variability. In the period 1955-1990 (Figure 3, left column), agreement between observations and simulations is limited, suggesting an important role of internal variability. From 1991 to 2010 (Figure 3, middle column), common features can be highlighted both in the observed and simulated trends, with larger tendencies in the North Atlantic and western Pacific, and a rather uniform warming in the Indian basin. Both simulations also show a patch of ocean heat loss to the North of the Ross Sea, which relates to a local decrease in convection over the region that will be further discussed in Section 4.2. Note that observations now show opposite trends with respect to those from 1955 to 1990 in both the Atlantic and Pacific oceans. This change in trends could be compatible with shifts in the phases of the PDO and the AMO indices among both periods. These contributions of natural variability will be further analysed in Section 5. Other possible shifts in natural processes that can influence the observed OHC700 trends during the last 20 years are the recent tendency to more negative NAO phases, a weakening of the SST gradients in the North Atlantic Current and the

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Irminger Basin (Flatau et al, 2003), and a decline in the strength of the North Atlantic subpolar gyre during the 1990s (Hakkinen and Rhines, 2004) and of the Atlantic meridional overturning circulation (AMOC) in the last decade (Send et al, 2011). [It starts in Page 11, line 5]

Comment 5: (p. 4235, line 24) Figure 6 (new Fig. 7) shows insignificant cooling in Weddell Sea but large and significant warming(cooling) in Amundsen (Ross) seas (respectively).

Reply: The authors are sorry for the mistake in the location. As the reviewer says it is actually the Ross Sea and not the Weddell Sea that presents a significant cooling. This has been corrected in the new version of the manuscript.

Comment 6: (p. 4237, lines 1-3) largest values in the Southern Ocean are in the Ross Sea region, NOT the Weddell Sea (generally thought of as an area of deep water formation). Where in the ECHO-G model in the Southern Ocean is deep water formed/ convection? Also, only net surface ocean cooling that appears to be significant (from the figures) in the Southern Ocean is in very small geographic areas near the Ross sea.

Reply: The regions of convection in the three long simulations are illustrated in Sup. Fig. 6. In the Southern Ocean, the model simulates deep convection in the Weddell and the Ross Seas. Shallower convection is also observed in the Amundsen Sea. Regarding the particular statement in the paragraph, we now mention explicitly which are the two particular convection regions where the large positive OHC anomalies are observed. The small significant patch with net upper ocean cooling seems to be responding to a localized reduction in cloud cover, whose exact origin is unknown to us.

Comment 7: (p. 4239, lines 19-24) Is the AMO detrended but not the PDO? Do you apply a linear detrending to the AMO after removing global signal (and thus really *detrend 2x - which doesn't make sense particularly over a short - 55 yr - time frame)*

Reply: The reviewer is completely right. The linear detrending on the AMO definition is redundant after the removal of the global signal is performed. We have recalculated the AMO now only applying the substraction of the global mean, and corrected accordingly the details on the text, and the corresponding figures. No large changes with respect to the previous results have been found, as the final AMO timeseries remains rather stable and compares well with the previous version with the double detrending .

Rephrased version: Likewise, the AMO is defined as the regional average of Atlantic SST anomalies north of the Equator. Unlike in the AMO definition of Enfield et al (2001) no previous detrending is applied to the SST data. Instead, to only preserve the signal of internal natural variability unrelated to the forcing, the three previous indices are computed with SST anomalies calculated with respect to the global SST mean, thus filtering out the influence of the global warming signal (Zhang et al, 1997). [It starts in Page 22, line 26]

Comment 8.1: There is no discussion about teleconnections between ENSO and Southern Hemisphere surface temperatures, sea ice, etc. (and the classic dipole pattern is partly observed in Figure 11a (New Fig. 12a) but either not present or obscured by the"obs" label in the bottom right of the plot) suggested in obs. OHC (and suggested by Figure 11a / New Fig. 12a) but either not reproduced in simulations (Fig 11b / New Fig. 12b) or possibly suggested but a bit too far north (Figure 12a/ New Fig. 13a). These teleconnections between ENSO and Southern Ocean appear robust in obs - some discussion is merited here, including an overview of the models ENSO performance.

Reply: We understand that by classic dipole the reviewer refers to the opposing impact of ENSO on sea ice concentration and surface temperature between the Ross/Amundsen and the Bellingshausen/Weddell sectors (e.g. Liu et al., 2004), with positive ENSO phases leading to less sea ice and warmer temperatures in the Amundsen sea and the opposite changes in the vicinity of the Antarctic peninsula. From Figure 12a, it is not so obvious that this pattern occurs for the observations. Please, note that labels have been changed in all figures to avoid a partial screening of the regression coefficients. It is true that there is a clear warming northeast of the Ross Sea, but its negative counterpart seems to be too weak and remains insignificant. This apparent misrepresentation might be just a consequence of not looking directly at the changes in surface temperature. As the OHC is integrated in the upper 700 m, the signal of ENSO might be just attenuated due to the contribution of subsurface waters. Regarding the simulations, none of them seems to reproduce the main aspects of this teleconnection. Indeed, no significant impacts of ENSO on Southern Ocean surface temperature were found in a previous evaluation of ENSO performance in a control ECHO-G simulation (Min et al., 2005b). Other reported biases relate to the simulated amplitude and frequency of ENSO, which turns out to be too strong and regular. Despite these systematic errors, ECHO-G performance is good when compared to other AOGCMs and observations, as it has a well-defined Walker circulation, it simulates an enhanced warming from the tropical central Pacific to the west coast of South America, and it also reproduces well the associated rainfall in the tropical Pacific (AchutaRao and Sperber, 2002). A comment on the teleconnection ENSO/Southern ocean temperatures has been also included.

New text: It is worth mentioning that ENSO and NAO variability has been already evaluated in another control simulation with the ECHO-G model (Min et al, 2005b) through a comparison with observations and other simulations. The overall model's performance is good, although it presents some systematic errors like an overestimation of the NAO impact over the Pacific sector, and stronger than observed ENSO amplitudes, which tends to be also too frequent and regular. Regarding ENSO, the model resolves properly the Walker circulation, the associated rainfall over the tropical Pacific, and the anomalous warming from central Pacific to the coast of South America. Likewise, the simulated NAO reproduces well the dipolar SLP structure over the North Atlantic, as well as the corresponding quadrupole in surface temperature, and the opposed precipitation pattern over the North and the South of Europe. [It starts in Page 23, line 7]

New text 2: Some aspects of the ENSO teleconnection with surface temperature in the Southern Ocean (e.g. Liu et al, 2004) can be identified in the observations, but not in the simulations. In particular, observations show a significant warming north of the Amundsen sea, with opposing changes in the Bellingshausen and Weddell Seas, although these latter remain insignificant. [It starts in Page 22, line 9]

Comment 8.2 (p. 4241-4242, line 29-line3) This is an important result and should be supported by a Figure in which one can actually tell which way the arrows are pointing, or another way of showing this (see comment below on Figures 8, 9 14).

Reply: We have increased the size of the arrows in the 3 figures to help visibility.

Comment 9: (p. 4224, Line 7)“later” not“latter” and delete“on”; (p. 4226, Line 19) remove“Besides” (doesn’t make sense)

Reply: All changes have been made.

Comment 10: (p. 4229 line 19) verb tense -“concern” not“concerning”

Reply: The sentence has been rephrased

New text: This has led to the publication of somewhat different OHC estimations by different groups and institutions (Domingues et al, 2008; Ishii and Kimoto, 2009; Levitus et al, 2009), which mainly differ in the representation of interannual OHC variability (Lyman et al, 2010). [It starts in Page 7, line 13]

Comment 11 and 12: (p. 4237, line 17)“Besides” doesn’t make sense...”In addition” or remove
(p. 4240, line 24) Eliminate“besides”

Reply: In both cases “besides” has been removed.

Comment 13: *(Fig. 8 / New Fig. 9) What“horizontal dotted line”? I can’t see one or find one...*

Reply: The horizontal dotted line was in an initial version of the plot but was removed for the final version of the previous manuscript. The caption has been now corrected.

Comment 14: *(Figures 8, 9, 14 / New Figs. 9, 10, 15) The“arrows” are very difiňĄcult to see and in some regions impossible to tell the direction they point. I suggest either eliminating them entirely (the do not clearly support conclusions as they are not visible or understandable.*

Reply: As previously said in the response to comment 8.2, the arrows have been increased for better visibility. We prefer this to eliminating them entirely from the figures of wavelet coherence as they provide useful information on the phase relationship among the different time series, which is discussed throughout the text (see below).

In the text: Regarding the PDO and AMO, both indices show alternating periods of good and poor coherence with the local OHC700 at multidecadal timescales. 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 suggest that a real but intermittent modulation of the OHC700 by both indices may be taking place [It starts in Page 26, line 3]

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