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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.

P. Ortega et al.

pablo.ortega@lsce.ipsl.fr

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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 Figures and tables have been included in a supplementary file to attend the reviewers concerns.

Reply to Reviewer 4

General Comment: *This paper covers interesting material that is within the scope of Climate of the Past. The characterizations of the roles of external forcings and internal climate modes for the ECHO-G model are interesting and would enrich the community's understanding of the ability of climate models to represent these physical phenomena. The scientific methods appear reasonable, but I find not enough information is given about how they were applied in order for a reader to assess how their use or understand the limitations of the results. Similarly, none of the trend values were provided with uncertainties. There are problems with some of the datasets used by the authors, but they have applied reasonable limitations in their use in order to address these issues. However, I am concerned that in a number of different contexts, global trends are subtracted from spatial data before regional analyses are performed. I would like to see evidence that that hasn't introduced spurious trends in those regional analyses. Overall, the paper is clear and organized in a logical way.*

General reply: The authors appreciate the comments and suggestions. They have all been answered in detail. Special effort has been put to strengthen the manuscript to meet the main reviewer's concerns as, for instance, the initial disequilibrium due to too short a spin-down period (Comment 1.1), the use of different methodologies (e.g. Comments 5, 6, 11 and 26) or the uncertainties inherent to some of our results (Comment 12). Also, as suggested in Comment 1.2, we have checked the validity of the removal of linear trends in CTRL, realizing that contrary to what we had assumed before, trends are not steady and in consequence the detrending is no longer valid. Fortunately, the impact of this incorrect detrending is negligible in the upper ocean, where the analysis is focused. Most figures have been redone now using undetrended data, but no significant changes have been observed.

SPECIFIC COMMENTS:

<u>Comment 1.1:</u> *(p. 4227) Preparation of Simulations I am concerned about the equilibration process and drift correction used on FOR1 and FOR2. According to OR12, FOR1 was initialized from year 17 of the present-day control and equilibrated to year 1000 conditions for only 100 years. FOR2 was initialized similarly, starting from year 1700 of FOR1. As Ortega et al state on p. 4232 ln 1, and in my experience, properly equilibrating simulations using coupled atmosphere-ocean models for millennium simulations from even late preindustrial runs takes more than 1000 simulation years. I think choosing to only examine FOR1 over the observational period is appropriate, but I wonder why in OR12, they exclude the first two centuries of FOR2 due to disequilibrium and not in this paper. I also think it is important in ln 23 that they make it clear that the initial conditions of FOR1 are not just "anomalously warm," which might happen if the modes of variability happened to be in a warm state, but that the climate had not equilibrated to cooler conditions from a present-day control run.*

Reply: The authors recognise that there is an equilibration problem in the configuration of the millennial forced simulations. As the reviewer points out, the spin-down period of 100 years turned out to be clearly insufficient to bring the ocean model (starting in year 17 of CTRL for FOR1, and in year 1700AD of FOR1 for FOR2) to a thermal state compatible with the external forcing conditions in year 1000AD, in particular for the deep ocean layers. We agree that the fact that FOR1 appears to be "anomalously warm" is just a consequence of this insufficiently long equilibration period. As suggested by the reviewer, this is now stated in the corresponding line in the text. Also, we believe that the reasons that led us excluding the first two centuries of FOR2 for the analysis in OR12 do not necessary apply for this study. We took a conservative decision common to both FOR1 and FOR2 to get rid of the effect of large initial trends in the deep ocean,

especially for FOR1, that could have an important impact on the Atlantic meridional overturning circulation, as this quantity is integrated for the whole ocean depth. In this current study, OHC is analysed in the upper 700 m, where FOR2, unlike FOR1, shows no remarkable trends (Fig 5a, and bottom panel in Fig. 4b), thus suggesting that the upper ocean initial state in FOR2 is likely equilibrated to the external conditions.

New text: FOR1 is initiated from year 17 in CTRL and driven during 100 years to the forcing conditions of year 1000 AD. The resulting starting conditions are anomalously warm when compared to other model simulations (Goosse et al, 2005; Osborn et al, 2006), making evident that the spin down period was too short to allow the model to reach equilibrium in surface temperatures. [It starts in Page 6, line 4][1]

<u>Comment 1.2:</u> *(p. 4232) Ortega et al state that they remove CTRL trends at each ocean level from FOR2, since there is an apparent drift. Although removing global trends from spatial data has been done before, I find it problematic here for two reasons. 1) It presumes these trends are globally uniform. This isn't a problem when the authors perform analyses of global data, but Ortega et al also perform regional analyses in this paper. Can they demonstrate that they haven't actually introduced trends to their regional data? Are the ocean trends consistent between their regions of interest in CTRL? 2) don't think the authors have demonstrated that the trends in CTRL are due to climate drift and not disequilibration, in which case CTRL is not similar enough to their transient runs in order to use it as a basis for correcting trends. OR12 states that the model already uses ïňĆux corrections based on the spin-down run for CTRL in order to avoid climate drift. Furthermore, it appears in Fig. 3 (New Fig. 4) that the control run is not drifting at a constant rate, but is slowly adjusting to present-day conditions. If this is the case, then there is no reason to expect a similar trend in the transient runs, which are not aligned in time with CTRL (FOR1 starting at year 17 and after 100 further simulation years, and FOR2 after 1700 years of FOR1 plus another*

[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.

100 years) and are equilibrating to entirely different conditions than CTRL. Fortunately, most of the analyses have been performed with data from the top 700 m only, which show little trend for CTRL in Fig. 3.

Reply: The reviewer is completely right. Unlike what we thought, trends in CTRL are not stationary. We have recalculated the depth profile of trends in Figure 4b for the first and second half of the CTRL simulation (light blue and light green curves, respectively). It is evident that trends are particularly strong at the beginning and later on they tend to fade away as the ocean reaches more stable thermal conditions. Hence, there is again a clear issue of initial thermal imbalance, just like in FOR1 and FOR2, and therefore the correction applied to remove the trends in CTRL is no longer valid. Fortunately for the main findings in our analysis, the focus is placed on the upper ocean, where trends are visibly smaller. All figures and tables have been redone now using the raw data (without any trend correction), and no significant changes have been found (actually they tend to be almost imperceptible). We believe that this responds to two main reasons. First, as already said before, that CTRL trends are particularly small in the upper levels and thereby they only produced a minor change in the long-term trends. And second, that all interannual to multidecadal and centennial variably remains unchanged thus contributing to produce similar results.

<u>Comment 2:</u> *(p. 4229) Observations of Ocean Heat Content: Ortega et al state that global estimates of ocean heat content are sensitive to both the datasets included and the methodologies used. As a consequence, they focus on long-term trends and decadal variability. Nevertheless, in Fig. 1 (New Fig. 2), they present data with annual resolution. How was this data generated?*

Reply: For both the simulations and observations the OHC700 fields are integrated from yearly three dimensional temperature fields. No subsampling is applied as NODC-OCL observations (Levitus et al., 2012) are interpolated and provided in a gridded field. Other observational estimates will certainly present different variability, in particular at

interannual timescales (Lyman et al., 2010), thus justifying our focus on the longer timescales. But that does not prevent us to present the different OHC700 curves at their actual resolution.

<u>Comment 3:</u> *(p. 4231) General warming over 1955-1990? The authors state that there is "weak general warming" in all three datasets over this period, but this is not clear in Fig. 2 (New Fig. 3). What is the source of this discrepancy? How weak is weak?.*

Reply: We could say that there is a general warming as linear trends in Table 2 support a general OHC increase. In either case we agree that this is not so evident in the Figure. Therefore, we have decided not to mention this warming it in the text.

<u>Comment 4:</u> *(p. 4258) Total rad. Forcing regression plots: Firstly, the text states that there is an overall warming in both plots of Fig. 5 (New Fig. 6). This is not apparent. More importantly, I'm concerned about the use of the Student's t-test independently at each spatial grid point in order to establish regions of significance. This method does not account for spatial correlation, which has the effect of exaggerating the degree of significance in localized regions (i.e. for 0.05 significance level, expect 5% of the data to exceed significance limits by chance, but spatial correlation may cause one point exceeding to make more grid boxes exceed significance limits). This method of testing significance was also used for most of the other maps in the paper. In Fig. 5, the regions of significance are pretty patchy. How much do they exceed 5% of the total number of ocean grid boxes?*

Reply: This point is certainly well raised. Fig. 6 is clearly patchy and we might have gone too far with the interpretation of significant areas, and in particular due to the non-accounted effect of spatial correlations. However we believe that it would not change substantially the inferences on the other regression patterns, as we mostly focus our discussion on the main centers of action, characterized by larger regression coefficients. In the new manuscript, the discussion on Fig. 6 has been changed to focus more on the largest discrepancies among the different panels.

Rephrased version: Coherence between both plots is limited and there is a general disagreement in the regions where the largest significant changes occur. This points to a probable different role of internal variability in this period for the two datasets. [It starts in Page 15, line 17]

Comment 5a: (p.4235-4236) Regressions of millennial OHC against forcings: More information is needed about how the authors have performed these analyses. Have they regressed the OHC against each variable separately or at the same time? Have they performed the regressions against the GHG time series for the entire thousand years? Based on Fig. 9 (New Fig. 10), it appears there is very little variation in GHG prior to 1800. Thus, I expect this result is dominated by the period following then. During this time, GHG and solar time series exhibit similarities, so I am concerned that there may be some overlap in their contributions to the GHG fingerprint. More generally, over the thousand years, are the time series for solar, volcanic and GHG forcings independent of each other?

Reply: Each panel in Figs 7a-d represents the regression of an individual variable with the OHC700 data. In the particular case of Fig 7a, the total radiative forcing represents the combined radiative effect of all the three forcings. Likewise, the effective solar constant in Fig 7d integrates the contributions of the solar and volcanic forcings. However, both the total radiative forcing and the effective solar constant are employed as individual time series. In all cases, regressions have been calculated for the whole millennium. This also includes the GHGs, even if we agree that its regression pattern is dominated by the increasing trends in the last 150 years. During this same period, a slow gradual increase in solar variability is also noticeable, with other multidecadal variability superimposed. As a result of this similar trend during the industrial period, both forcings are significantly correlated (Sup. Table 2), as suspected by the reviewer. This can partly explain the similar features in the regression maps of Fig. 7. Indeed,

when those maps are recalculated for the preindustrial period (Sup. Fig. 1), the good agreement disappears (See also Sup. Table 3), and only solar irradiance is strong enough to produce a significant warming in the southern extratropics. However, as the magnitude of the solar trend is really small compared to that of the GHGs, the risk of overlapping in the total radiative forcing is really small. Finally, the volcanic forcing is found to be independent to the other two forcings (Sup. Tables 2 and 3). Part of this discussion has been included in the manuscript.

New text: Note that during the preindustrial period the global influence of GHGs is substantially reduced, and solar irradiance only shows significant impacts on the southern extratropics and the Nordic seas (Sup. Figure 1). [It starts in Page 16, line 17]

Comment 5b: (p.4235-4236) The composite analysis for volcanoes is very interesting. However, I wonder why if the authors are trying to separate the impacts of different volcano sizes, they choose to include the largest volcanoes in both moderate and strong groups.

Reply: The idea of excluding the largest volcanoes in the analysis of the moderate eruptions is reasonable. We have modified the composition analysis accordingly, now separating the response of the top 10 volcanic eruptions, from that of the 15 moderate eruptions following the previous 10 in magnitude. We now notice a warming in the central equatorial Pacific for the strong volcanoes, that is in line with an influence of explosive volcanism on the occurrence of El Niño events (Emile-Geay et al., 2008).

Rephrased version: The impact of volcanoes is thus evaluated through a composite analysis focused separately on the top 10 and the subsequent 15 largest preindustrial eruptions, to distinguish the strong from the moderate impacts... Also of note is the warming in the central equatorial Pacific for the strong volcanoes, thus in line with an influence of explosive volcanism on the occurrence of El Niño events (Emile-Geay et al, 2008). [It starts in Page 18, line 21]

<u>Comment 6:</u> (p. 4237) Wavelet coherency analysis: Did Ortega et al follow Torrence and Webster (1999) exactly in methodology with respect to smoothing filters, etc? Otherwise, there is very little information provided about their calculation. I am not very familiar with this technique, but I find their results very interesting. I also think Fig. 10 (New Fig. 11) is a very useful result.

Reply: Specific details on the choices made for the wavelet functions, the smoothing and the assessment of significance have been now included in the text. They were actually taken following the recommendations in Grinsted et al. (2004).

New text: 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 with 1000 surrogate datasets. [It starts in Page 20, line 15]

<u>Comment 7:</u> (p. 4239) Anomaly deviations: The authors cite Zhang et al (1997) in justifying removing global-mean SSTs from local SSTs as a method of filtering out the global-warming signal. Zhang et al (1997) applied this technique to the ENSO index only. Given regional variability in the OHC700 response to GHG's shown in Fig. 7b, particularly the large region of cooling in the North Atlantic and extra warming in the North Pacific with respect to the ENSO region, I am concerned that Ortega et al expect this method to remove the effect of GHGs in these regions entirely. Can they repeat the analysis of 6b with SSTs to test this hypothesis? I also wonder if this step is actually necessary for the AMO and the PDO, given they detrend the N. Atlantic SSTs before calculating the AMO, and principal component analysis should isolate the PDO signal as long as it is independent of the GHG signal.

Reply: The idea of removing the global SST mean for the calculations of ENSO, PDO and AMO indices is not exactly to remove the signal of response to GHG's but to the total radiative forcing in general. We had actually noted that when this preprocessing

was not applied, all the indices calculated incorporated some part of the low-frequency modulation by the forcing. This also includes the PDO index as we have found that the EOF analysis tends to mix independent signals as long as they operate at different timescales (in this case decadal and interdecadal for the PDO, and multidecadal and centennial for the forcing). Regarding the AMO definition, we certainly agree that the final detrending of SST data was unnecessary, as the filtering of the global-warming signal already removes the final trend. We have recalculated the AMO avoiding this last detrending, corrected the details on its definition in the text,and modified all the figures accordingly. No large changes with respect to the previous results are found as the removal of the global signal leaves a rather stable timeseries for the mean AMO anomalies, which therefore is barely affected by the detrending. From all the final indices shown in the top panels of Figure 15 for the whole FOR2 simulation, only the NAO, for which no filtering of the global-warming signal was possible, shows some low-frequency modulation by the radiative forcing, which is coherent with the cross-correlations in Figure 14. All the other three indices are clearly uncorrelated with this total radiative forcing (Sup. Table 4), thus giving confidence in the technique for the removal of the worldwide radiative signal. Finally, as required in the comment, we have repeated the analysis in Fig. 7a (instead of Fig 7b) with the SSTs. In this way we can check the regression pattern for the total radiative forcing (that combines the three contributions). Regression patterns in Sup. Fig. 7 still show a cooling in the North Atlantic and the extra warming in the North Pacific. This implies that as that signal is also removed, the indices defined only concentrate on the PDO and AMO variability independent from the forcing. That is what we wanted, and we had already seen in Sup. Table 4.

Rephrased version: Likewise, the AMO is defined as the regional average of Atlantic SST anomalies north of the Equator. Unlike in the AMO definition in 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, all the three previous indices computed with SST anomalies are 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]

<u>Comment 8:</u> *(p. 4240) patterns explaining a shift in ln 1-2: the authors argue that the patterns associated with ENSO and PDO may explain a shift in OHC700 trends. Clearly the structure of the modes can not explain temporal changes, so I am assuming that they are referring to changes in the time series of the modes at this time. Are they referring to the change in trend of the PDO around 1990?*

Reply: We agree that exact phrasing employed was not particularly accurate. It is not that the patterns explain the shift. What we really see is that the regions where OHC700 shows larger impacts for ENSO and the PDO (i.e. the regions with larger regression coefficients in Fig. 12) coincide with those areas that exhibited opposing trends between the periods 1955-1990 and 1991-2010 and that we had associated to a change in the polarity of both indices (see Section 3.1 and Fig. 3). We have tried to explain this better in the text.

Rephrased version: The observed ENSO and PDO indices exhibit similar patterns, with the former showing stronger equatorial anomalies, and the latter a larger impact in the North Pacific. Generally, both patterns are associated with an eastern warming and a western cooling of the upper Pacific ocean. This result is consistent with the hypothesis that the local shift in OHC700 trends from 1955-1990 to 1991-2010 is due to a change in the polarity of the PDO (see Figure 3) [It starts in Page 23, line 1]

<u>Comment 9:</u> *(p. 4240) NAO final tendency: There is a discrepancy between what is stated in the text and what appears in Fig. 12d. The text states that the NAO has a tendency toward negative values, whereas the plot shows the values tending toward positive. Which is accurate?*

Reply: The statement in the text was right. There was a mistake in the y-axis in all top panels of Fig. 12, as the labels were reversed by error. This has been now corrected.

<u>Comment 10:</u> *(p. 4241) Mode correlations: I really like the use of Fig. 13 (New Fig. 14) in this discussion. I find it very helpful and the results interesting.*

Reply: We are happy that the reviewer finds this figure helpful and the discussion interesting.

<u>Comment 11:</u> *(p. 4267) Decadal detrending: The caption says the time series were decadally detrended, but there are more-than decadal trends apparent in the time series, particularly after 1900 for the OHC Box I time series, for example. How was decadal detrending performed?*

Reply: For the time series at the top panels we have filtered out all high-frequency (and not low-frequency) variability using a cut-off period of 10 years. This was only intend to smooth the raw time series as they presented large interannual variations that hindered their comparison. The particular filter employed uses least squares coefficients to reduce Gibbs oscillations (Bloomfield, 1976) and is characterised by a sharp transfer window that allows an accurate selection of the different timescales. In the new manuscript, the caption in Fig. 15 has been rewritten to clarify which frequencies have been filtered out.

Rephrased version: In the top panels timeseries are smoothed with a 10-yr low-pass filter to ease comparison at interdecadal timescales. [In page 51]

<u>Comment 12:</u> *(p. 4252-4253) Uncertainties: None of the tables provide any uncertainties for the trends. This information would be helpful in evaluating whether or not differences between different datasets or periods are significant...*

Reply: Uncertainties related to the least square adjustment used to calculate the trends have been included Table 2. Unfortunately, the instrumental uncertainties from the OBS dataset were not available and could not be included. Note that old Table 3, illustrating the trends in SL and thermal expansion for which we did have estimates of their uncertainties, is no longer present.

<u>Comment 13:</u> (p. 4224 ln 7) "latter" spelling error.

Reply: The spelling error has been corrected

<u>Comment 14:</u> (p. 4227 ln 24) "as it will become" - remove "it".

Reply: It has been removed

<u>Comment 15:</u> (p. 4229 ln 17) "lead" spelling error.

Reply: Now corrected in the text

<u>Comment 16:</u> (p. 4230 ln 16) "to" should be "on".

Reply: Now changed in the text

<u>Comment 17:</u> (p. 4231 ln 9-10) sentence is confusing as written - Not clear whether saying there are opposite trends between Atlantic and Pacific or between earlier and later periods

Reply: Trends are opposite in the earlier and the later periods. The sentence has been rephrased for clarity.

Rephrased version: Note that observations now show opposite trends with respect to those from 1955 to 1990 in both the Atlantic and Pacific oceans

<u>Comment 18:</u> (p. 4231 ln 14-16) if downward heat transport decreases, wouldn't that cause local warming?.

Reply: That is right and goes in line with the first comment of the second reviewer. It is not the downward heat transport that decreases, but the vertical heat mixing that brings heat to the surface from the bottom layers. Therefore, as less heat mixing is produced, there is a local cooling at the surface and a warming in depth. The text is now corrected in the new version.

Rephrased version: Yet, a local OHC700 cooling south of Greenland is also seen, in line with a local decrease in deep convection that reduces vertical heat mixing and thus the replacement of dense cold waters at the surface with relatively lighter and warmer waters from deeper levels. [It starts in Page 11, line 26]

<u>Comment 19:</u> (p. 4234 ln 7) one sixth what? fraction of observed sea level rise? Metre?.

Reply: We referred that value to the sea level rise estimates from Jev08. However, that part of the discussion has been now removed from the text.

<u>Comment 20:</u> (p. 4236 ln 2 and p. 4259 label and caption) – what are you calling the effective solar constant? Is this total solar irradiance plus volcanics?.

Reply: The reviewer is right. The effective solar constant incorporates the combined signal of volcanoes and solar irradiance. It is now explicitly said in the text and the caption.

<u>Comment 21:</u> (p. 4236 ln 5) not clear that you are referring to two different analysis groups (i.e. one with 25 and one with 10 volcanoes) – right now, suggests you are talking about 25+10 eruptions.

Reply: We now specify that both composite analyses (for the top 10 and the 15 moderate volcanoes) are done separately.

<u>Comment 22:</u> (p. 4239 ln 1-3) you state "it has been proposed" and "other works" but you don't provide citations – whose proposals and other works are you referring to?

Reply: The first sentence has been rephrased to be more specific on the details. In the second, some references have been provided.

Rephrased version: 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 (interdecadal to secular; see Figure 9). Other works (Willis et al, 2004; Levitus et al, 2005) also support the influence of different modes of climate variability 10 on the OHC700 [It starts in Page 22, line 5]

Comment 23: (p. 4240 ln 18) "analysis is on the following extended" awkward sentence structure.

Reply: The sentence structure has been reformulated.

Rephrased version: In the next subsection, the analysis is extended to the last thousand years... [It starts in Page 24, line 24]

Comment 24: (p. 4241 ln 5 and p. 4266 caption) are you evaluating the lead/lag relationships using FOR2 over the entire period?

Reply: It is right. We now specify that in the text.

Comment 25: (p. 4241 ln 22) "relative" should be "relatively".

Reply: Now changed in the text.

Comment 26: (p. 4241 ln 28-29 and p. 4242 ln 1) I'm confused by the explanation of the arrows here. If they represent phase overlap between the two time series, how can they also correspond to particular climate states? Wouldn't the OHC conditions in a given phase relationship depend on the state of the index?.

Reply: We believe that the paper was lacking a detailed explanation of the wavelet coherence technique, and its interpretation. Arrows highlight the phase relationship between two time series, but only when they show coherent variability at some specific timescale. As common variability at one particular timescale can also happen by random chance, the analysis of the corresponding phase relationships is useful to test to what extent this wavelet coherence relies on a physical basis. If accordance at one

particular frequency band is a product of mere chance, phase relationship will tend to vary throughout time. Conversely, a stable phase relationship during the whole period of analysis will give more confidence on the link among the two variables as long as the lead-lag relationship has a real physical meaning. However, no further conclusions on causality should be undertaken. What we discuss in the last lines of page 4241 and at the beginning of page 4242 is whether wavelet coherence between both the PDO and AMO and the OHC700 in their respective centers of action is coherent a regional modulation of the OHC by the indices. We conclude that this modulation might be real as phase relationship remains always stable, and in both cases its value is such that OHC700 changes lags those of the climate indices.
In the new manuscript, there is now a detailed explanation on the wavelet coherence analysis and how to interpret it preceding the description of the results. We have also sligthly rephrased the particular lines mentioned by the reviewer to be more accurate in the interpretation.

New text: 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. [It starts in Page 20, line 11]

Rephrased version: 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 5]

<u>Comment 27:</u> (p. 4242 ln 27) "upper warming" missing "OHC".

Reply: It has been added now.

<u>Comment 28:</u> (p. 4242 ln 27) periods listed overlap "1955-2010 to 1991-2010" - Did you intend instead 1955-1990, as the plots in Fig. 2 (New Fig. 3) are delineated?.

Reply: It is indeed a mistake. Periods should be 1955-1990 and 1991-2010 as suggested by the reviewer.

<u>Comment 29:</u> (p. 4244 ln 6-8) unclear sentence meaning

Reply: Sentence has been removed from the text.

<u>Comment 30:</u> (p. 4256 Figure 3a / New Fig. 4a) top plot is labelled CTRL2, which does not appear in body of the paper. (p. 4257 Figure 4a / New Fig. 5a) curve labelled CTRL2, which does not appear in body of paper.

Reply: Both typos have been corrected

<u>Comment 31:</u> (p. 4258 Figure 5 / New Fig. 6 caption - did the authors really use 99.5%

significance thresholds, or did they mean to say that they used 0.05 significance levels (i.e. 95% significance thresholds).

Reply: We mean 0.05 significance levels, as it corresponds to the commonly used confidence threshold of 95%. The phrasing has been corrected in the new manuscript.

Rephrased version: Dotted areas enclose values exceeding the 95% confidence according to a Student's t test that takes into account the series autocorrelation to correct the sample size

<u>Comment 32:</u> (p. 4261 Figure 8 / New Fig. 9) caption says there is a horizontal dotted line indicating period 10yrs, but I don't see it.

Reply: The line had been removed, but not the reference in the caption. None of them appears in the revised version.

<u>Comment 33:</u> (p. 4264 -p. 4265) change of colour scales between these two plots makes it harder to visually compare differences in model fingerprints of the various modes between the preindustrial and industrial periods.

Reply: The color scales were selected separately for each of the Figures 12 and 13, so that the regression coefficients over the main centers of action for each index were easily identifiable. However, we agree that a common color scale would help a better comparison between both figures, which do not correspond actually to preindustrial and industrial periods, but to the observational period (Fig 12) and the whole FOR2 simulation (Fig 13). Please note that a new common color scale has been selected for both figures.

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