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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.

Anonymous Referee #4

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General Comments

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.

Specific Comments

1.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.

On 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.I 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

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their transient runs in order to use it as a basis for correcting trends. OR12 states that the model already uses flux corrections based on the spin-up run for CTRL in order to avoid climate drift. Furthermore, it appears in Fig. 3 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 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.

2.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, they present data with annual resolution. How was this data generated?

3.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. What is the source of this discrepancy? How weak is weak?

4.p. 4258 Total rad. Forcing regression plots Firstly, the text states that there is an overall warming in both plots of Fig. 5. 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?

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5.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, 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?

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.

6.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 is a very useful result.

7.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. 6b, 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.

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8.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?

9.p. 4240 NAO final tendency There is a discrepancy between what is stated in the text and what appears in Fig. 11d. The text states that the NAO has a tendency toward negative values, whereas the plot shows the values tending toward positive. Which is accurate?

10. p. 4241 mode correlations I really like the use of Fig. 13 in this discussion. I find it very helpful and the results interesting.

11.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?

12.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.

Technical Corrections

p. 4224 ln 7 “latter” spelling error

p. 4227 ln 24 “as it will become” - remove “it”

p. 4229 ln 17 “lead” spelling error

p. 4230 ln 16 “to” should be “on”

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

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p. 4231 ln 14-16 if downward heat transport decreases, wouldn't that cause local warming?

p. 4234 ln 7 one sixth what? fraction of observed sea level rise? Metre?

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?

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

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?

p. 4240 ln 18 "analysis is on the following extended" awkward sentence structure

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

p. 4241 ln 22 "relative" should be "relatively"

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?

p. 4242 ln 27 "upper warming" missing "OHC"

p. 4242 ln 27 periods listed overlap "1955-2010 to 1991-2010" - Did you intend instead 1955-1990, as the plots in Fig. 2 are delineated?

p. 4244 ln 6-8 unclear sentence meaning

p. 4256 Figure 3a top plot is labelled CTRL2, which does not appear in body of the paper

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p. 4257 Figure 4a curve labelled CTRL2, which does not appear in body of paper

p. 4258 Figure 5 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)

p. 4261 Figure 8 caption says there is a horizontal dotted line indicating period 10yrs, but I don't see it

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

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Interactive comment on Clim. Past Discuss., 8, 4223, 2012.