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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 #2

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In this paper, the authors address a relevant research question. The variability of ocean heat content needs to be further assessed in order to compare it against the observed trends. It is also important to understand the development of the regional structure of ocean heat uptake further. In these respects the submitted ms. is timely.

My concern is, however, whether it was a wise choice to use ECHO-G for the purpose of this study. As the authors discuss, the drift in ocean temperature is strong. In Fig. 4c, they have to go great lengths (for details see below) to explain the the large discrepancy between the trends of their model and the data.

Moreover, the model uses flux corrections, which affects the surface heat fluxes and

therefore certainly the ocean heat uptake. I acknowledge that in the CMIP3 class of models some still used flux corrections, but I expect that ocean heat uptake and its regional patterns will be affected by the flux corrections particularly strongly.

In addition, in a recent model intercomparison study (Kuhlbrodt and Gregory, 2012, GRL), ECHO-G stands out with an exceptionally small ocean heat uptake efficiency, actually by far the smallest of all CMIP3 and CMIP5 models. I am aware that the authors could not have seen this study upon submission of their ms., but perhaps they would still like to discuss this feature.

In another intercomparison study, ECHO-G stands out as well. Wang et al. (2011, JGR 116) found that in the A1B scenario it has the strongest wind stress change of all CMIP3 models, but no consequent increase in ACC volume transport, which could point to an unusual response of the ocean stratification to the wind stress.

In conclusion, I would suggest that the authors (1) discuss the results of their model in the context of other AOGCMs, and (2) if possible, repeat at least a few of the analyses with another AOGCM, to be sure that their results are of general relevance, and not particular to ECHO-G alone. Therefore, my recommendation is to return the ms. to the authors for a major revision.

Detailed remarks ^^^^^^^^ ^^^^^^^

p.4225, l.23 and p.4231, l.15.: Inconsistent discussion. Does the shutdown of convection lead to deep warming (p.4225) or to deep cooling (p. 4231)? This might well depend on the surrounding stratification, but that should be discussed here.

section 2.3: I can see why the authors use proxies of total sea level rise for its thermosteric component. However, in the North Atlantic the thermosteric and halosteric components can cancel each other out (e.g. Pardaens et al., 2011, Clim. Dyn.). What does that imply for using the Kemp et al. record here?

p.4233, upper half: As said before, the authors have to make a really great effort to

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discuss the large discrepancy between their model and the data. Still, I don't find the discussion convincing. The model trends are dominated by the model drift. That's why their trends have the wrong sign up until 1600. They also don't capture the trend after 1850. According to Church et al., 2011, GRL, the thermosteric contribution to the total is about 40% in the late 20th century, not one sixth as the authors claim. Using the years 1800 to 1900 to de-trend the model simulations appears as a very ad-hoc approach, and from Fig. 4c it cannot be assessed how the trends in that time compare with the data.

Fig. 2: Please discuss the big patch of ocean heat loss in the Pacific sector of the Southern Ocean. What is happening there? There is no similar feature in the observations, but this patch dominates some of the regression patterns in Fig. 6.

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