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Interactive comment on "Deep Ocean Temperatures through Time" *by* Paul J. Valdes et al.

Anonymous Referee #1

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TitleÂă: Deep Ocean Temperatures through Time

Authors: Valdes et al.

In this paper, the fundamental question of benthic oxygen isotopes as a proxy for global surface temperatures is addressed using a coupled General Circulation model (HadCM3). Based on extended set of simulations covering the whole Phanerozoic, this study demonstrates that the mean Earth's temperature is consistent with benthic oxygen isotopes for the Cenozoic era. Simulations using late Cretaceous boundary conditions suggest that deep ocean temperatures tend to be less well-correlated with polar temperatures. For deeper periods of time, the link becomes less precise and deep water temperatures cannot be interpreted anymore as a measure of the mean global mean temperature. If the CO2 is mentioned as a potential explanation, the

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changing geography seems to be preferred.

Scientific interest

In my opinion, the present manuscript is good. The paper is very well written and considerable care has been taken to perform compare modeled temperatures and the proxy record (especially the statistical evaluation of the variability). Consequently this paper will be a useful resource for the community.

However this paper could become far better with an extended discussion. Here authors conclude that the change in the deep water temperature should not be taken as representative of the globally change of surface temperature for Mesozoic-Paleozoic eras. However the discrepancy between data and models is mainly driven by two warm periods (Jurassic and Triassic) where deep waters were formed in shallow sea at low latitudes (lines 517-519). This result is very interesting and challenges the initial suggestion of Emiliani (1954) that deep water continues to be formed at high latitudes in climate much warmer than today's. By adding a few diagnostics, the present study may become a key paper to understand the origin of warm deep waters and which conditions are required to form warm deep water at low latitudes (as initially proposed by Chamberlin, 1906). By identifying conditions for formation of deep waters in very warm climates (Cretaceous, Triassic), the scatter between deep ocean/mean surface temperatures should be significantly reduced for pre-Cenozoic periods. Since this result may substantially change the conclusion of the present paper, I recommend a major revision.

In addition to my general comment, here are some recommendations that the authors may consider to improve the paper (ranked by order of importance)

lines 381-395 (section Correlation of Deep Temperatures to Polar Sea Surface Temperatures) Here, the authors assume that most of the deep water in the model is formed at the surface at high latitudes (which explain why they explore the polar amplification later). However HadCM3 seems to be able to form dense warm saline bottom water in

subtropics when the Earth's climate becomes very warm. This process likely explains why purple dotes (fig. 6 and fig.9) are clearly above the proposed slope and why polar surface/deep ocean temperatures (fig.6) and deep ocean temperatures/global temperature (fig.9) are not well-correlated. Consequently this result seems to indicate that we need to distinguish two oceanic states for the formation of deep waters 1) when the main cause for buoyancy loss is salinity and 2) when the main cause for buoyancy loss is cooling.

This issue appears fundamental because this results implies that the change in polar surface temperature is not representative of the deep ocean temperatures. Consequently a "systematic" correlation between polar/deep ocean temperatures cannot be made. If the authors want to keep their initial conclusion, they have to demonstrate that the Emiliani's suggestion (1954) that most of deep waters continue to be formed at high latitudes is always true - even in a climate much warmer than today's.

For solving this issue, the authors should: - reconstruct the Earth temperature (for the whole Phanerozoic) using deep water temperatures assuming that "deep ocean water does not always form at polar latitudes" (line 386-387). - explore the effects of shallow sea at low latitudes for warm periods (here Triassic and Cretaceous) to decipher why the formation of deep water is so different, in both cases.

I realize that the authors may not want to undertake the project I have outlined. In that case they should claim only a speculation, not a conclusion, and they have to rewrite the paper in taking into account this major issue.

References to add: On a Possible Reversal of Deep-Sea Circulation and Its Influence on Geologic Climates. Chamberlin. T. Proceedings of the American Philosophical Society Vol. 45, No. 182 (1906), pp. 33-43 Temperatures of Pacific Bottom Waters and Polar Superficial Waters during the Tertiary, C. Emiliani Science (1954), Vol. 119, Issue 3103, pp. 853-855 DOI: 10.1126/science.119.3103.853

- lines 394-395: If Poulsen et al. (2001) has mentioned the formation of dense warm

saline water in subtropics by the geographic isolation (Mid-Cretaceous boundary conditions), Poulsen's simulations did not show a global circulation driven by deep water in the sub-tropics. Moreover, I do not understand why this result could be considered as model-dependant. Indeed both cases are generated by the same model.

- lines 349-355 (section 3.2: Comparison of Model Sea Surface Temperature to Proxy Data) I disagree with the following sentence - lines 351-352"... 15°C mismatch between models and data. If we assume the data has a seasonal bias, and select the summer seasons .... reduced by 4°C" This sentence implies that deep waters in polar regions are formed during the summer season (which means that the cause for buoyancy loss is salinity (without the formation of sea-ice)! Moreover this sentence is inconsistent with correlations made later in the paper. For example the caption of the figure 6 considers the polar temperature averaged in winter. This sentence should be removed and the discrepancy between models and data more discussed in the manuscript (see comment above).

- lines 508-513 (section Discussion and Conclusion) Paleogeographies are often mentioned as the main reason of the results outlined in the study. Unfortunately the direct effect of this factor is not well illustrated. Since the polar amplification depends on geographies used in this study, a figure showing the Poleward Energy Transport (or/and a paragraph) could be included in the section devoted to the " polar amplification"

- section 2.4 The authors implicitly assume that the initial state for the ocean has a marginal effect (which means that the final state is always the same whatever initial conditions used, so there is no hysteresis). However this point may have importance. Indeed, because late Jurassic deep waters are warmer than for the Cretaceous, a sensitivity test should be performed using temperature profiles obtained for Jurassic (instead of values from previous model simulations) to initialize the Cretaceous ocean.

- section 3.1 To demonstrate that the GCM is well designed to compare deep ocean temperatures to benthic ocean data, the revised version of the manuscript should con-

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tain a validation test using modern conditions (deep water temperatures simulated versus modern data) - or at least a reference.

- line 265 “less than 1000m” seems to be not consistent with the caption of the fig.4 (line 644)

- general shape of the manuscript The manuscript is organized by headings and subheadings from pages 2 to 10 but not after, why ?

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