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Climate of the Past Discussions

Interactive comment on "Paleogeographic controls on the evolution of Late Cretaceous ocean circulation" *by* Jean-Baptiste Ladant et al.

Stuart Robinson (Referee)

stuart.robinson@earth.ox.ac.uk

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I enjoyed reading this well-written paper that uses an Earth System model to explore the sensitivity of ocean circulation to long-term changes in Cretaceous palaeogeography by comparing simulations of Cenomanian and Maastrichtian conditions. This is an important and active area of research as geochemical proxy records for Late Cretaceous ocean circulation (such as those from neodymium isotopes), although useful in documenting stratigraphic changes in local water mass chemistry, have proven harder to interpret unambiguously in terms of water mass sources and flow path. In this context modelling exercises are highly complementary and informative. I am certain this paper is appropriate for Climates of the Past.

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My only significant issue with this paper is the treatment of CO2 levels and the comparison of the models with climate proxy records (Section 2.4).

The baseline simulations have CO2 set at 1120 ppm (4 times pre-industrial atmospheric levels, PAL), which is an entirely appropriate starting assumption for much of the Cretaceous, but as acknowledged (lines 981-993), was, in the latest Cretaceous, likely lower than in the mid-Cretaceous. It is surprising that no sensitivity analysis to CO2 levels has been included (especially as the same group has just published limited aspects of the model results, but including 2x and 4x simulations, in Haynes et al in press in Geology; https://doi.org/10.1130/G47197.1). The Haynes proxy and model results need to be included in the paper throughout, especially if they allow a consideration of how the sensitivity of the simulations to CO2 change.

The results at 4xPAL show substantial differences in the pattern of ocean circulation to those reported by Donnadieu et al (2016). It is suggested (lines 740 onwards) that this might be due to the more complex nature of CCSM and the role of continental run off in these different models. However, other model simulations (not discussed in the paper) have suggested that CO2 plus palaeogeographic change may be significant effects on ocean circulation at various times. in particular, Farnsworth et al (2019) in GRL highlight that in their Maastrichtian simulation using HadCM3 ocean circulation is extremely sensitive to CO2, with a switch between South Pacific deep-water production at 4xPAL to South Atlantic deep-water production at 2xPAL. It would be good if the authors could include some discussion of the results of Farnsworth et al. (2019) and their thoughts on the significance of the Farnsworth result.

Section 2.4 compares proxy temperature records with the simulations and finds (line 981) a disagreement which the authors relate to the constant CO2 value used. Given this major assumption in the simulations presented, I think that much of the oxygen-isotope discussion in section 2.4 is either irrelevant or should be prefaced by the caveat that CO2 does not vary in the simulations and, therefore, the temperatures (and temporal evolution of temperature) in the model simulations is unlikely to match

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the proxy records. The conclusion to this section (lines 1014-1016) raises the question of whether Section 2.4 is necessary.

Minor issues/suggestions:

Use of past and present tense – in a number of places (e.g. line 149, 153), the present tense is used to describe events in the geological past.

Line 80-82: Odd structure to this sentence - consider rephrasing as: “This conjecture is corroborated by studies of the temporal trends and spatial variations in neodymium (Nd) isotopes. . .... a quasi-conservative tracer of waters masses. . .”

Line 83: insert commas before, and after, “in particular”

Line 84-85; papers led by Robinson should be included in this list as they were amongst the first to show the long-term shift in Nd between the mid and latest Cretaceous.

Line 86 onwards – I think it may also be worth mentioning here that another area of uncertainty is regarding palaeowater depth – some sites are rather poorly constrained and thus the possibility exists that different water masses are being sampled but considered as broadly of the same depth.

Line 154: change to “black shale deposition”

Line 157: insert “the” before “South Atlantic”

Line 201: Insert “The” before “presence of a. . .”

Line 214-216: what is the reason for the shallower depth of the Drake Passage in the Maastrichtian versus the Cenomanian?

Line 221: On the basis of results in Donnadieu, it is argued that the shallow depth differences in the Drake passage are not significant for global ocean circulation. However, given the differences in results overall, can the authors be certain that differences in depths <1000m will make no difference in their model framework?

Line 279: replace “Indian” with “Tethyan”

Line 293: comment on why Sewall is used rather than Tabor.

Line 330-332: Awkward sentence structure – consider rephrasing.

Lines 356-357: small scale regional features of the simulations are described but the use of “South America” as the geographic descriptor is a bit too vague – please be more precise when describing where these local oceanographic features occur.

Line 436: replace “to the subpolar” with “with the subpolar”

Line 437: insert “of” before “the South Atlantic”

Line 522: insert comma after “experiment” and delete “and”

Lines 760-771: This paragraph contains a lot of fundamental, introduction-level material about Nd-isotopes that might be better worked into the introduction to the paper rather than included in the discussion.

Line 796: Robinson and Vance should be included in the citations here.

Line 819-824: I don’t really see the justification for suggesting that the deep-waters exported from the Pacific in the Cenomanian were relatively low in eNd given that the values shown from the Pacific in Haynes et al (in press) suggest the south Pacific had relatively high eNd values (>-6) in the Maastrichtian, and, if the simulations are correct, probably the Cenomanian too. Given the relatively unradiogenic values of the eastern Australian coast and Ross Sea, is it not surprising that the Pacific data have values of >-6 in the Maastrichtian, if those regions are the source of the water masses? This section seems to be at odds with aspects of Haynes et al and the actual Cenomanian values of Indian and Atlantic water masses.

Line 825-827: Would it be possible to test the effect of imposing barriers in the Southern Tethys (around Kerguelen, I would imagine), even if they are not very well constrained?

Lines 842 onwards. The Soudry data is incorporated into the discussion of Cenomanian circulation in the Tethys, yet, on line 846, it is stated (correctly) that the Soudry data come from neritic settings so cannot be used to interpret intermediate or deep-water circulation. Thus, it seems misleading (and a bit confusing for those unfamiliar with the datasets) to discuss Soudry if the conclusion (line 847) is that the data from neritic settings cannot be used to support the simulations.

Line 857: replace “and that...” with either “that...” or “but we are unable to exclude them.”

Lines 875-881: Whilst I agree that boundary exchange could be an issue for the eNd data from Demerara Rise, the existence of very saline waters (based on the Mg/Ca and d18O data of Friedrich et al., 2008) do point to local formation of warm saline bottom waters and suggests that this feature of ocean circulation is missing in all model simulations.

Line 890: The papers by Robinson support increased exchange by, and during, the Maastrichtian and should be cited here.

Line 910-912: One important consideration could also be the significance of palaeowater depth differences and variable amounts of boundary exchange between sites (and through time) and the effect these factors might have on the records of eNd. Furthermore, the use of different archives of eNd, could also be a source of offsets between different datasets.

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