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Climate of the Past
Discussions

Interactive comment on "Climate sensitivity and meridional overturning circulation in the late Eocene using GFDL CM2.1" *by* David K. Hutchinson et al.

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We thank the reviewer for his thoughtful and constructive comments, which have helped to improve the manuscript. We present the reviewer's comments in black text, and our response in blue text.

Summary:

The authors describe a suite of four climate model simulations that use topography and boundary conditions representative of the Eocene-Oligocene transition (âĹij 34 Ma ago). The use of a recent reconstruction of the 34 Ma topography (Baatsen et

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al. 2016) and of relatively high resolution ($\sim 1^\circ$ in the horizontal, 50 levels in the vertical) in the model ocean distinguishes these numerical experiments from previous attempts to model the late Eocene climate. The sensitivity of the simulated climate state to the prescribed level of atmospheric CO_2 (400 vs. 800 vs. 1600 ppm) and to the parameterization of ocean vertical mixing (bottom-enhanced mixing vs. Bryan-Lewis diffusivity profile) are both examined. This represents a substantial modelling effort. The text and figures provide a clear overview of the simulated surface climate and deep ocean circulation as a function of CO2 and as compared with present-day climate. I therefore recommend publication. Specific comments and suggestions that may help to improve the manuscript are provided below.

We thank the reviewer for the overall positive assessment.

Specific comments:

1. Link between ACC and NADW (p3, L10-12).

I believe it is not clear (from the literature) that the ACC favours NADW formation via mechanical mixing and Ekman upwelling. The latter occur in the absence of a significant ACC. Toggweiler and Samuels (1995) argued that the presence of a deep ACC may force the southward flow into the Southern Ocean, that compensates for the surface northward Ekman flow, to be relatively deep. Subsequent work, reviewed by Marshall and Speer (2012), showed that eddy-driven mass transports bypass the constraint identified by Toggweiler and Samuels. Elsworth et al. (2017) suggest that the impact of the ACC on NADW may occur via density decrease of AABW. The authors may want to clarify the mechanistic link between ACC and NADW.

We have clarified the text in this section to say: “This notion rests on the modern-climate interpretation that, in addition to diapycnal mixing in the interior ocean (Munk and Wunsch, 1998), wind-driven upwelling in the ACC is a major driver of the deepwater cell presently associated with the Atlantic MOC (Toggweiler and Samuels, 1995). However, transports by mesoscale eddies counter the wind-driven upwelling in the

ACC, reducing the link between the winds over the ACC and the strength of the MOC (Marshall and Speer, 2012).”

2. Topography (p7, L11-14).

It would be useful to provide some more information about the methodology and uncertainties underlying the Baatsen et al (2016) topography used here, since this a crucial (distinguishing) ingredient of the simulations. For example, how well constrained are the sizes of the shallow Arctic-Atlantic and Arctic-Tethys gateways? You mention smoothing of the land topography using a 3-point mean filter: is this a standard procedure (and if so, is there a reference for it)?

The Baatsen et al (2016) topography is distinct from previous reconstructions (e.g. Markwick, 2007) in that it uses a Paleomagnetic reference frame to position the continents, rather than a hotspot reference frame. The other feature is that it starts from ETOPO modern day topography on land and relocates the continents by plate tectonic motion. For the deep ocean, an age-depth relationship is applied from Müller et al (2008), and adjusted to the Paleomagnetic reference frame.

Manual adjustments are then applied to areas where elevation changes are well constrained by geological evidence. Specific regions of adjustment include Antarctica, the Himalayas, the Amazon, Turgai Strait and the Tethys Sea. Perhaps the biggest drawback of this method is that it defaults too much to modern elevation. I.e. where paleoelevation data was either missing or unknown to the authors at the time of publication, the ETOPO data fills in the spaces. We have added text to clarify this point. We also discuss alternative reconstructions of the Arctic-Atlantic and Arctic-Tethys gateways.

The 3-point mean filter was an ad-hoc adjustment, due to numerical noise in the atmospheric velocities that we encountered in the high-latitudes when first testing the model. This noise was most likely due to topographic variations on Antarctica, which needed to be smoothed out due to convergence of meridians on the topography grid. In hindsight the smoothing may not have been necessary other than on Antarctica. We acknowledge that this representation of atmosphere topography could be improved.

3. Vertical mixing schemes (p8, L6-7; p14, L5-20).

A more detailed description of the two mixing schemes and of the diffusivities they prescribe/predict would be welcome. I do not think the Bryan-Lewis (BL) scheme is “specifically tuned to modern observations of mixing”: it is merely an ad hoc diffusivity profile, with a smaller value in the upper-ocean than in the deep ocean. It would be useful to mention the BL diffusivity values and the transition depth. To better understand why the two mixing schemes produce similar results, calculating/showing the stratification(N2)-weighted global (or basin) mean diffusivity profile would help. A map of the stratification-weighted vertical mean diffusivity would also help to visualize the heterogeneity/intensity of mixing, in particular within shallow regions when using the modified Simmons et al. (2004) scheme.

Does the bottom-enhanced mixing scheme produce similar effective diffusivities than BL in the thermocline and in the abyss? Is the poleward heat transport also insensitive to the mixing scheme? Could relatively high effective thermocline diffusivities contribute to the relatively warm climates simulated by this model configuration?

It should also be kept in mind (perhaps mentioned) that the Simmons et al. (2004) scheme assumes that the energy input to mixing within each grid column is proportional to the simulated bottom stratification. An increase in bottom stratification (as may result from increased high-latitude surface density gradients) is thus immediately paralleled by an increase in mixing energy, effectively maintaining roughly-constant diffusivities and roughly-constant circulation rates despite intensified density contrasts. This makes the Simmons et al. (2004) somewhat akin to the BL scheme, in that the diffusivity rather than the mixing energy is fixed. (Both schemes disallow a control of the energy consumed by mixing, making the interpretation of circulation rate sensitivities delicate.)

We agree that the Bryan-Lewis mixing scheme is more accurately described as an ad-hoc diffusivity, with stronger mixing in the abyss than in the upper ocean. We have

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added the diffusivity values and the transition depth to the manuscript in order to more properly describe the Bryan-Lewis scheme. In addition, we will compute stratification-weighted diffusivity values for each mixing scheme, in order to provide an ‘effective diffusivity’ as suggested. We have also added a basin-by-basin estimate of effective diffusivity. We do find that the effective mixing through the thermocline is similar between the two schemes, and this does help to explain the similarity in circulation.

We have also clarified our description of the Simmons et al (2004) mixing scheme, and its drawbacks associated with energy constraints, as suggested above.

4. Sensitivity of Southern Ocean deep water formation to CO2 (p17, L7-8; p18, L10-11; p19, L25-27; p20, L1-5).

Deep convection in the Weddell Sea shows an interesting non-linear dependence on CO_2, being stronger in the control state (800 ppm) than at 400 or 1600 ppm. This dependence merits more in-depth discussion.

Reduced convection and overturning at 1600 ppm is suggested to result from a lower meridional temperature gradient. This is not clear (existing theoretical arguments for a MOC-strength dependence on meridional density gradients are debatable and do not apply to the abyssal overturning), and if true relies on the stratification-dependence of the specified bottom-enhanced diffusivities. Enhancement of the hydrological cycle and consequent surface freshening of high latitudes (Figure 10) seems a more plausible/direct cause.

The cause of the absence of Weddell Sea deep convection at 400 ppm is not discussed. It is only noted that reduced sinking contributes to a fresher surface in the Weddell Sea, and that seasonal sea ice forms. The fact that deep convection ceases despite a weaker hydrological cycle and colder atmospheric winter temperatures may appear as a paradox. In fact, this response could reflect a fundamental regime shift as surface winter temperatures fall below 0°C. If temperatures remain well above freezing, winter cooling drives sustained open-ocean convection. When winter temperatures

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approach 0âŮęC, cooling barely elevates surface densities (the thermal expansion coefficient is small) and sea ice forms before thermal open-ocean convection can occur. The cycle of sea ice formation and melting then establishes the halocline (the rejected brine mixes over a relatively deep layer, whereas the melt accumulates within a thin surface layer), which further stabilizes the near-surface ice-ocean system (e.g. Goosse and Zunz 2014). Here, there is sufficient subsurface warmth that sea ice formation remains modest (e.g. Martinson 1990) and that haline coastal or open-ocean deep water formation does not replace the thermal open-ocean convection simulated at 800 ppm.

We have added a new figure of temperature-salinity properties in the sinking regions. This helps to clarify the role of freshwater forcing in the hot case (1600 ppm), and a change in regime in the cold case (400 ppm). We agree that when the surface approaches freezing, the seasonal density forcing undergoes a shift due to a reduced thermal expansion coefficient. We have included further discussion of this with reference to the above papers.

Technical corrections:

p1, L17: “We employ an ocean resolution of... ” -> “The atmosphere and ocean horizontal resolutions are... respectively.”

This has been changed, though we prefer to mention the ocean first.

p1, L19; p8, L4: “simulate the model” -> “run the model”.

This has been updated.

p1, L20: “CO2” -> “atmospheric CO2”.

“atmospheric” has been added

p1, L26: “salinities” -> “surface salinities”.

“surface” has been added to both references to salinity in this sentence.

p2, L10: “role of the ocean circulation” -> “the role of ocean circulation”.

This has been updated.

p2, L11: “leading theories of what caused” -> “prevailing proposed mechanisms for”. Should albedo feedbacks be mentioned as essential to these mechanisms?

The suggested edit has been made, and a sentence added on the importance of ice albedo feedbacks.

p2, L16: “atmospheric CO2” -> “atmospheric CO2 (thereafter referred to as CO2)”.

This has been updated.

p3, L18: Delete “data”.

“data” has been deleted.

p5, L3: “it does not capture the subsequent” -> “early and late Eocene topographies differ significantly due to”.

This has been done.

p5, L19: “a necessity of” -> “dictated by”.

This has been updated.

p6, L12: “future perturbations” is somewhat unclear/ambiguous. I would delete the unnecessary second half of the sentence (“and any. . .. late Eocene”).

We agree – since we did not specify what “future perturbations” we had in mind, we have removed that statement.

p6, L15: “the vertical diffusivity” -> “parameterized oceanic vertical mixing”.

This has been updated.

p6, L16-17: “mixing is set to a constant background value and then enhanced in the vicinity of rough-topography" -> "the diffusivity is set to a constant background value and enhanced near the bottom".

This has been updated.

p8, L6-8: "since the BL scheme is. . ." -> "since the former is more physical".

We have changed this to "since the former is more physically realistic".

p8, L8-10: Delete this last sentence of the paragraph, which is somewhat misleading.

We have deleted this sentence.

p10, L14: Delete "similarly".

Deleted.

p11, L4: "cannot" -> "does not". Here and elsewhere, it is argued that salinities are too low for convection to occur. More cautious statements recognizing that what matters is the low salinity relative to that of other high-latitude seas (and hence relative to the deep ocean) would be preferable.

We agree and have changed this in the manuscript.

p11, L7: "forces" is perhaps too strong, given that other factors (e.g. Nilsson et al. C5 2013) also play a role?

We have clarified this statement to say that it has a freshening influence in the North Pacific.

p12, L12-14: Not clear to me, please explain why.

The areal extent of the western Pacific warm pool in our model is larger than the present day. We suggest that this is partly due to fewer land barriers in the western Pacific and a wider basin. We argue that this creates a larger thermal inertia in the western Pacific, implying a reduction of variance in the east to west thermal gradient.

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p13, L21-22: Delete “Both of these sectors. . .tracer distribution.”

Deleted.

p13, L22: “The shallow gateways. . .provide barriers”: rephrase.

We have rewritten this to indicate that the shallow gateways through Drake Passage and Tasman Seaway inhibit the exchange of deep water between the basins.

p14, L1-2: It is not sufficient that the cell is “warmer and saltier”. What matters is the temperature contrast traversed by the circulation.

We agree and have adjusted this statement to mention the temperature contrast.

p14, L3: “due to warm, salty water masses”: unclear.

This has been rewritten.

p14, L6-7: Delete “The shape of. . . modern geography”.

We have clarified this sentence to say that the Bryan-Lewis scheme simply enhances deep ocean vertical mixing relative to upper level mixing.

p14, L8-10: Rephrase.

We have separated the two clauses into separate sentences and made clearer that we are talking about a priori expectations.

p14, L14: “roughness” -> “bottom-enhanced”. Roughness is constant in your implementation.

This has been changed.

p14, L 15-16: “a larger separation. . .. theoretical predictions” -> “a weaker diffusive heat penetration into the abyss”.

This has been changed.

p15, L16: “apsects” -> “aspects”.

This has been fixed.

p16, L5: “for example higher albedo”: not obvious, given potentially stronger albedo feedbacks.

We have clarified with model diagnostics that our net incoming shortwave radiation is indeed higher than the modern case.

p19, L7: “clear” -> “immediate”.

We have changed this to “large”, since we wish to emphasise the magnitude of change rather than the rate.

p20, L17: ”The” -> “the”.

Done.

p22, L4: “bottom roughness” -> “bottom-enhanced”.

Done

p22, L8: “freshwater” -> “fresh water”.

Done

p22, L9-10: “even in the absence of tectonic barriers in the Nordic seas”: what do you mean here?

This clause has been removed, since it was superfluous and did not flow properly.

Figures 6 and 12: Showing the MOC of the three (or four) runs in the same figure, in absolute values rather than as differences, would be much clearer.

We have altered the MOC figures to show the total circulation rather than the anomalies.

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