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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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The authors describe a suite of four climate model simulations that use topography and boundary conditions representative of the Eocene-Oligocene transition (∼ 34 Ma ago). The use of a recent reconstruction of the 34 Ma topography (Baatsen et al. 2016) and of relatively high resolution (∼ 1° 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 CO2 (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.

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.

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-Thethys 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)?

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.)

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 CO2, 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 approach 0°C, 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.

Technical corrections:

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

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

p1, L20: “CO2” -> “atmospheric CO2”.

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

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

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

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

p3, L18: Delete “data”.

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

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

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

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

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”.

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

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

p10, L14: Delete “similarly”.

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.

p11, L7: “forces” is perhaps too strong, given that other factors (e.g. Nilsson et al.

2013) also play a role?

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

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

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

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

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

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

p14, L8-10: Rephrase.

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

p14, L15: “bottom roughness” -> “bottom-enhanced”.

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

p15, L16: “apsects” -> “aspects”. p16, L5: “for example higher albedo”: not obvious, given potentially stronger albedo feedbacks.

p19, L7: “clear” -> “immediate”.

p20, L17: ”The” -> “the”.

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

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

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

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.

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