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

Anonymous Referee #2

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First of all, I did not read the other online review to this paper prior to writing my own, so this review is completely independent.

This paper aims to explore climate sensitivity and ocean overturning at the Eocene-Oligocene, as well as other aspects of the Earth system. It argues that by using more realistic paleogeographies and a relatively high resolution ocean model, it represents an advancement over previous work.

In essence, it describes a set of simulation at 3 CO_2 levels for the late Eocene, with 2 different parameterisations of ocean mixing, and includes a model-data comparison.

Overall, the paper provides an interesting summary of the basic state and climate sen-

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sitivity of this particular model, and is a solid baseline from which additional sensitivity studies can be carried out.

Major comments:

The introduction is a nice overview of some previous modelling work. However, more quantitative information could be presented. This paper focusses mostly on climate sensitivity and overturning, so a summary table of e.g. climate sensitivities and ocean overturning states at different CO_2 levels in previous work would be very informative and provide more motivation and context for the work presented here (especially if the paleogeogs and ocean resolution were included in the same table).

Section 2 on the model is good, but it appears that several changes have been made (to e.g. the resolution) compared with the published version of the model. As such, it is important to present an assessment of the performance of the model under preindustrial conditions. In addition, at several points in the manuscript comparisons are made with modern, and these are rather meaningless unless they are comparisons with a modern model simulation from the same model.

The spinup plots are useful and interesting, but it would be very useful to see Gregory plots of the three simulations as well, to assess how spun up the simulations are, and the likely equilibration temperature of the simulations.

Specific Comments:

Abstract: Is the model resolution really greater than previous work? A summarising table of previous simulations would help, see above.

P1, Line 25; P22, line 1: what is meant by “robust” in this context?

P2, Line 14: for a review of the mechanisms see Lear (2016, Science)

P2, line 21: seasonality as well as climate sensitivity was important in the Gasson paper.

P2, line 22: make it clear whether CO_2 estimates are for just prior to, or just after the EOT.

P6, line 10: need to make it clear that although using “realistic” paleotopographies is a step forward compared with some previous studies, there are still uncertainties in these that are not captured in this study.

P9, your experiences with model spinup are very interesting and I think warrant inclusion in the conclusions section, and maybe even the abstract.

P9, line 15: a graph illustrating the fully coupled versus asynchronous spinup procedure (e.g. a graph of time against global mean temperature at a couple of ocean depths) would be very interesting and illuminating. i.e. add non-asynchronous run to Figure 2.

P9, line 23: add another plot which is the evolution of ocean overturning strength in each basin over the model spinup period.

P9, line 23: Also add another timeseries which is ocean salinity over time in the Arctic (and maybe other ocean basins). It would be interesting to know if this is still decreasing or whether it has reached an equilibrium.

P11, line 27; p21, line 19: “La Nina-like mean state” I would avoid this term; it has caused much confusion in the Pliocene community. Instead, just say that in the annual mean the west-east gradient in increased.

P12, line 26: I don’t think that the level of confidence in the interpretation of the proxy data justify this statement. I don’t think the data really allows us to say which modelled seaice distribution is best (i.e. there is some seaice at 800ppmv).

P14, line 5: It would be good to note whether these sensitivity studies to mixing scheme were run for exactly the same length of time and had the same spinup process as the ‘standard’ runs.

P16, line 4 – for the comparison with modern climate sensitivity, is this exactly the same model and same resolution etc?

P16, line 16: "We suggest that the ice-free conditions allow for substantial radiative warming of SST in addition to atmospheric polar amplification." I don't understand what this means.

Section 4.4: This section is somewhat superfluous, and doesn't add much to the manuscript unless the location of late Eocene palms are plotted on Figure 13.

Technical comments:

Figure 1a – get rid of the 'hole' at the north pole and 0 degrees east.

Figure 3 caption – state the co2 concentration.

Coloured circles in figure 8 are difficult to see – put a black line around the circumference.

P15, section 4 title – CO_2 not CO2.

Figure 9 would be better presented as both plots being a warming, i.e. 1600-800 and 800-400. In fact, throughout the paper I would prefer presenting the simulations as 3 in a series, rather than one in the middle with a warmer and a cooler either side, to aid consistency.

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