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Geoscientific Model Development Discussions

Interactive comment on "A suite of Early Eocene (~ 55 Ma) climate model boundary conditions" *by* N. Herold et al.

Anonymous Referee #1

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Herold Eocene Review

Overall, this paper deserves publications after some minor/moderate revisions. It describes a community asset – boundary conditions for community use for the Eocene.

I think that the authors need to hedge the purpose of making a standardized boundary condition for the Eocene. Yes, it is important for GCMs to have a 'unified' set of boundary conditions. But, considerable uncertainty exists in every paleotopographic reconstruction. Ones as 'recent' as 21 kya, and certainly in ones from 55Ma. (Bracconot 2012 does point out the impetus to apply a consistent set of boundary conditions. But, if you take this too far, it leads to poor/unphysical choices, like making a Franken-Ice (thrown together combo of many separate reconstructions) for the last glacial maximum.)

It is EQUALLY important that these simulations are paleoclimate simulations – simulations meant to emulate a time in the past for which proxy data to constrain the output exists. Uncertainty in the boundary conditions is just as important. Otherwise, you could easily envision a countless number of 'MIPs' involving aqua-planets of one degree or another that are really much cleaner. I have serious doubts about the circular nature of using water isotopes to diagnose paleotopography. The intro makes it sound like the only problem is one of consolidation, with 0 discussion of uncertainty. Uncertainty should be a HUGE part of the discussion. (And should be a part of the figures!)

I don't know that I would say that topography was the most important. In terms of mean climate – GHG concentration is what is setting global mean temperatures, for instance. All the boundary conditions are important. I am glad to see that they have included some aerosol components. But – there could have been more.

In this vain, this paper seems to rely very heavily on the Markwick (2007) paper, under the pretext that one should only convert to GCM input once. I am a bit confused here – automated tools already exist to convert from shape files to gridded data. Application of such a basic tool set does not seem to merit a whole paper, just a mention in a methods section. So, I think I must have misunderstood what the authors did – i.e., please revise so that it is clearer what NEW bit is being presented for the topography here. (I was left with the impression – I hope erroneous – that this is just Markwick 2007 version 1.1)

For the sub-grid scale parameterization. Is the modern – a product of waxing and waning glacial conditions for the past few million years – a good analog for the Early Eocene – a period following extreme warmth, enhanced hydrologic cycle (and likely higher P-E)?

There has already been a bathymetry dataset produced (Bice et al 1998 – which was missing from the intro (!!??!!) AND the bathymetry section). I appreciate that the authors on this paper have done much to investigate Eocene climate, but they need to

reference those who have facilitated and furthered the science, but non-collaborators, on this current project.

It seems like they may have included some water isotope paleo-topography information. This is extremely qualitative information. And, it isn't clear from the text (you must track down the references) that this has been brought in. The paper should be more readable (don't leave the reader infer data sources and uncertainties from the references).

They have addressed such issues as Eocene vegetation reconstructions being fundamentally at odds with topography (rain forests in the rain shadow of mountain belts), but where is the quality of the vegetation reconstruction constrained? Here they have presented a single realization of BIOME4 from a single model. The whole point of projects like EO MIP was to compile Eocene diagnostics from multiple models. Why not tap into the EOMIP data? Also, even with multiple models, problems still occur (and need to be explicitly addressed): Precipitation biases exist in this GCM in the modern, and the vegetation model has been primed to account for this – is this still valid for another climate? Are there uncertainties? Or, is imposing a bias? Also, the partial use of 8xCO2 conditions (At the upper end of estimates) to determine climate, then only 4xCO2 conditions to determine vegetation isn't okay. You can either choose one and stick with it, or do the experiment consistently several times. This uncertainty needs to be explored not only for its impact on climate but also for 'greening' impacts on plants, anyhow. Also, you might interpret the failure of 8xCO2 to produce the plant distribution well enough might be indicative that it was not the appropriate GHG concentration to use for the climate simulation. If you are going to tell the reader to interpret grassland as only dry shrubland, you need to explain briefly what the difference is in BIOME4 between these two pfts.

I still do not think any of the GCMs has a good handle on the reduced meridional temperature gradient present in the Eocene... and becoming every more present in the modern. There could be some underestimate of this from uncertainties in paleo-

data reconstructions, but that isn't the whole picture. Because the authors use a GCM to 'fill in the blanks' are they reinforcing this bias?

It feels like the river routing was done as an afterthought. Why produce it at all if its all downhill – no information has been added. Are there indications of what drainage basins were like in the Eocene. Are there any discrepancies/uncertainties?

Interactive comment on Geosci. Model Dev. Discuss., 7, 529, 2014.