

Interactive comment on “Stability of the vegetation–atmosphere system in the early Eocene climate” by U. Port and M. Claussen

Anonymous Referee #2

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"Stability of the vegetation– atmosphere system in the early Eocene climate" explores sensitivity of vegetation distributions and climate to initial conditions and boundary conditions. The study uses the MPI-ESM and finds that for modern conditions or for Eocene conditions with 'bright soil' conditions evidence is not found for sensitivity to initial conditions in terms of the predicted vegetation distribution. This result is compatible with results from prior work on dynamic vegetation modeling in the Eocene (i.e. the work of shellito and sloan, not cited). The novel result of this study is that for one set of imposed boundary conditions, i.e. 'dark soil' conditions, evidence for sensitivity (of the mean resultant state) to initial conditions, or intransitivity is found. These results are not placed within any paleobotanical context so this is essentially a free-standing modelling result, which will probably be mainly of interest to others trying to prognose Eocene paleo-vegetation using predictive models. I will leave it to the editor to decide

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if the paper is appropriate for the broad Climate of the Past community and focus on the strengths and weakness of the modelling itself within the context of other similar modelling studies.

Some strengths.

The model, and diagnoses are of high quality and the writing is also generally of high quality and relatively easy to read (there are some typos throughout, though).

The results will be interesting and relevant to people trying to other specialists trying to perform dynamic vegetation simulations in past climates. Rarely, if ever, does one find intransitivity in vegetation distributions. Indeed, it is rare enough that I am skeptical (I have never seen it in my own dynamic vegetation modelling for this time interval). That said, I am happy to be convinced, but some more effort is necessary to demonstrate that this is indeed happening and if it is happening in the way that the authors suggest.

Some weaknesses.

(a) Is this the Early Eocene? This is a model and so there must always be a certain suspension of disbelief and squinting to say whether a given set of boundary conditions and simulations corresponds to any one time interval. This simulation takes this principle a bit far, however. Looking at the model derived temperatures compared to proxies in the companion paper (and the similarity with the results presented in Lunt et al. 2012) it appears these simulations are substantially too cold and with overly strong temperature gradients. CO₂ is also quite low for the Early Eocene. Indeed, the whole set of simulations looks like the middle or even Late Eocene. I do not see this as a huge problem if this was a standard GCM paper using prescribed vegetation—one could simply replace the word "Early" for "Middle" and it would still be a valid paper. However, for predictive vegetation modeling, the resulting vegetation types are a function of climate and CO₂. So to get a physical/biological consistency one must have the 'right' climate at the 'right' CO₂, or alternatively the study should span a wide combination of parameters. That is not performed here, which is a weakness in the approach. I do not see it

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as a fatal problem, but it should be acknowledged.

(b) Are these results similar to or different from other models or data? A basic context is missing from this paper. Comparable model-predicted vegetation patterns have been simulated, but none of the relevant publications are discussed (Shellito and Sloan, 2006a,b not cited ; Lopston et al., 2014 not cited; Herold et al., 2014 not cited). Nor are the various data-derived compilations and model boundary conditions compared against (such as Sewall et al., 200—not cited; Utescher and Mosbrugger, 2007 cited). So one is at a bit of a loss to ascertain whether the results here are even in the correct ballpark. I believe they are, but that should be shown. For example the results are very close to Figure 6 in Herold et al., 2014, which at least provides some reason to think that the gross results (not the intransitivity) are at least robust (to changes in model, CO₂, and other boundary conditions).

(c) Maybe this is not real intransitivity of vegetation.

Very little effort is given in this paper to describing soil properties or spinup of JSBACH, but this could be a huge issue or nothing at all. For specificity, let me refer to <http://onlinelibrary.wiley.com/doi/10.1029/2010JG001612/full> ("Soil carbon model alternatives for ECHAM5/JSBACH climate model: Evaluation and impacts on global carbon cycle estimates") in which various formulations of JSBACH are described. I am not sure which formulation is included in the study here, but the basic point for either model is that "The soil carbon pools of CBALANCE needed a run for 1080 years and Yasso07 for 3000 years to stabilize."

No information on spin-up of JSBACH before the beginning of the coupled simulations is presented. Given the fact that soil carbon pools will be completely different in equilibrium with different vegetation types coupled with the long equilibration time of soil carbon pools, the simulations currently shown may simply be still far from equilibrium. It is the norm in this kind of study for several thousand year offline spin-ups of the carbon (and potentially other nutrient) pools to be conducted. Was that done here? How

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might this kind of difference in initial carbon pool affect the physical hydrological properties of the soil? What affects the soil permeability in JSBACH, are there long-memory processes in that (such as would be affected by different initializations?). What are the physical properties (normally based on soil texture) set in JSBACH? Is the soil texture and soil permeability the same in modern and Eocene simulations?

(d) Assuming this is intransitivity, a stronger case should be made to back up the proposed mechanisms.

The study proposes that changes in Central Asia drive, through ocean-atmospheric feedbacks, distal vegetation responses, for example in North America. This is a reasonable hypothesis. The regions they have identified correspond to modern regions of high coupling coefficient ("Contribution of phenology and soil moisture to atmospheric variability in ECHAM5/JSBACH model", Bali and Collins, 2015—not cited) and changes in diabatic heating and divergent circulation in the Indian Ocean are well established as links in the chain between all tropical circulations (for example the linkage between the Indian Ocean Dipole and ENSO). Such teleconnections are likely in the Eocene (Huber and Caballero, 2003—not cited). One way of describing these results is that as one changes initial conditions a strong monsoon kicks in over Central Asia (figure 4) at the expense of a monsoon in the other main monsoon regions. Huber and Goldner (2012-not cited) wrote extensively on evidence of a global Eocene monsoon system (in models and data) which is supported by subsequent data (Quan, et al. "Revisiting the Paleogene climate pattern of East Asia: a synthetic review." *Earth-Science Reviews* 139 (2014)). So, the authors interpretation is a reasonable place to start, but they do not actually test this hypothesis with an appropriate model experiment.

In a revised paper I would expect to see a simulation in which the only difference is the initial condition in the key central asian region. If the authors are correct this should be enough to shift the teleconnected regions in Figure 2.

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