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Climate of the Past Discussions

Interactive comment on "Warm Paleocene/Eocene climate as simulated in ECHAM5/MPI-OM" *by* M. Heinemann et al.

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We would like to thank all three anonymous referees for their productive comments and suggestions.

Response to comments raised by multiple referees

A) Why is this Paleocene/Eocene (PE) simulation closer to proxy data than previous PE runs?

In the revised paper, we add a paragraph addressing this question to the 'Discussion and conclusions'.

We can not ultimately say why our PE simulation is closer to the proxy record than the previous fully coupled GCM simulations. There are many poorly constrained parameters that may have a large effect on the model solution; greenhouse gases, land surface, vegetation, and soil parameters are obvious examples. We do not know all of these parameters from Huber and Sloan (2001) nor from Shellito et al. (2009), but assuming that the boundary conditions in the previous CSM1.4 and CCSM3 setups were similar to those we use here, CSM1.4 and CCSM3 must have a smaller sensitivity than ECHAM5/MPI-OM with respect to the PE-PR boundary condition differences. This smaller sensitivity with respect to the PE boundary conditions (including the $p\mathrm{CO}_2$-doubling) would be in line with the smaller climate sensitivity to a $p\mathrm{CO}_2$-doubling alone of the NCAR models compared to ECHAM5/MPI-OM (Kiehl et al. 2006, Randall et al. 2007). One way to analyse the differences between our PE simulation and the previous NCAR Eocene simulations would be to also apply the EBM analysis described here to the NCAR runs. We speculate that especially the water vapour feedback in our simulation is larger than in the NCAR simulations. An explicit investigation of the $p\mathrm{CO}_2$-sensitivity of our PE model solution will be subject of a future study.

B) Meridional extension of the EBM.

To clarify the different role of clouds in different areas, the first reviewer suggested to apply the zero-dimensional EBM to the poles, the mid-to-high latitudes, and to the low-latitudes selectively. The second reviewer suggested to extend the 0-dimensional EBM in the meridional direction, to really get a handle on the causes of the change in the latitudinal surface temperature gradients; Dorian Abbot sent us an email

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Interactive Discussion

Discussion Paper

suggesting a specific 1-dimensional EBM for that purpose. In the revised paper, we apply that 1-dimensional EBM to our PE model solution.

Response to Referee #1

The authors explain that this is the first Eocene GCM simulation that is consistent with the proxy record. The difference from Huber and Sloan who also used CO2=560 is especially interesting, as their results were significantly further away from the proxy record. Can the authors explain, or at least attempt to discuss, what is different about this model that made this result possible?

See above, cumulative answer (A).

High latitude vs polar vs global cloud emissivity effects: If I understood this correctly, clouds seem to have some important effects: warming over polar areas, and cooling over low-latitudes. these effects seem very relevant to the Eocene challenge of reducing the equator to pole temperature gradient while keeping the equator cool. The use of the 0d global EBM masks this to some degree because of the cancellation of cloud effects between the different regions. How about applying it selectively to the polar areas, the mid-to-high latitudes, and the low-latitudes? In any case, discussing the cloud effects separately for these latitude ranges in the conclusions section may help clarify the different role of clouds in different areas.

We apply a 1-D EBM in the revised paper to tackle this issue.

CPD
5, C800–C814, 2009

Antarctic emissivity: perhaps I just missed this: is the large difference in antarctic long-wave emissivity a consequence of the topographic changes, allowing more water vapor at lower elevations there during the PE?

The PE-PR emissivity difference is indeed in part a consequence of the topographic differences, especially in the Arctic. In the revised paper, we clarify this aspect, and separate the topographic effect from the clear-sky emissivity difference assuming a constant lapse-rate, and using the 1-D EBM.

Reduction & increase...: the discussion of emissivity is somewhat confusing because a reduction of emissivity is repeatedly mentioned. Upon careful reading I think I understand that the emissivity was higher in the PE and lower in the PR, but saying so explicitly throughout rather than using "decreasing" or "increasing" may help avoiding some confusion.

The emissivity in PE is lower than in PR. We agree, the use of "decreasing" or "increasing" when comparing PE and PR is confusing, we avoid that in the revised paper.

Hydrological cycle and clouds: would it be difficult to discuss and analyze the polar areas separately from the mid-latitudes? As the manuscript explains, the clouds can have opposing effects (albedo vs emissivity) in the mid-latitudes, but there are only emissivity effects during polar night over the poles. These different behaviours may justify separate analysis? Also, how about briefly discussing the seasonality of the behavior over the poles, given the potentially different behavior of clouds during polar day and polar night.

CPD

5, C800–C814, 2009

We adress the poles and mid-latitudes separately in the revised paper using a 1-D EBM. However, we would like to leave the analysis of the polar seasonality for future studies.

Response to Referee #2

What is the effect of using homogenous vegetation in the Eocene run? This is hinted at in the Discussion section but I feel it is critical, and accounts for a large proportion of the polar warmth. Some assessment could be made by including a more realistic Eocene albedo in the EBM.

In the revised paper, we use a 1-D EBM to diagnose the effect of the PE-PR albedo difference. The largest difference is due to the assumption that there are no glaciers in PE, due to the reduced snow cover, and due to the lack of sea ice. These high-latitude PE-PR albedo differences are presumably much larger than the albedo differences that would arise from a more detailed, more realistic PE vegetation reconstruction. The direct effect of the *large* high-latitude albedo differences is already smaller than the effect of the PE-PR emissivity differences.

We prefer not to follow the reviewer's suggestion to assess the effect of a more realistic Eocene albedo using the EBM. The EBM uses the planetary albedo, not the surface albedo. The shortwave cloud radiative forcing would change in response to the surface albedo change. To include such feedbacks, we would need a GCM simulation. In this study, however, we use the EBM only as a diagnostic tool, not as a predictive tool.

For the comparison with SST data (section 3.2), I would like to see more data sites *included here, e.g. from New Zealand. Also, in figure 5 you should compare the data with the local gridbox in the model (or average of closest few gridboxes), rather than with the zonal mean. These points should be plotted in Figure 5. Also, these sites should be plotted in Figure 4, including the croccidilian data, and maybe the 278.7K contour in the model (for comparison with croc data).*

We compare our PE simulation to paleodata from the period right before the PETM – this had not been clear in the previous draft, and we clarify this in the revised paper. We suspect that the reviewer refers to Hollis et al. (2009) as the New Zealand data. Unfortunately, the data from Hollis et al. (2009) only covers the early Eocene, not the period before the PETM. In the revised paper, we add proxy-data from Tanzania (Pearson et al. 2007). We include the SSTs from the local grid boxes in Fig. 5, and we add the paleo-sites, crocodilian data, and contour line in Fig. 4 as suggested.

WHY is this run relatively more successful than previous attempts? The work implies it is the albedo change, or water-vapour feedback in this particular model. I know it is hard to conjecture about model-model differences when you only have access to a single run, but maybe some conjecture towards the end would be nice.

See above, cumulative answer (A).

The EBM analysis is interesting, but can only really tell us about the causes of global mean temp change. Would it be possible to use a 1-D EBM (ie introduce latitude dependency), given the meridional heat fluxes from the GCM, to really get a handle on the causes of the change in latitudinal gradients? I would not insist on this, but it may give some useful insights and improve the paper further.

Yes, it is possible. We introduce a 1-D EBM as an additional diagnostic tool in the revised paper. Thank you for the suggestion!

P1299, line 14 – more refs for modeling attempts which have failed to produce low pole-equator gradients.

Ok, done.

P1303, line 1. Are the SSO parameteristion switched off in both the present-day and Eocene runs? Mentioned p1305, line 21, but also include here please.

No, it is switched off in PE, and it is switched on in PR. But we have tested switching it off in another pre-industrial simulation, which we do not use in this study (except to argue that the effect of the SSO parameterisation is relatively small). On p1303 the reader does not yet know about PR. We would like to maintain the order of the paper, and only introduce PR in Section 2.4.

Section 2.1 Boundary conditions – I assume you remove all ice sheets for the Eocene run? Please confirm in this section.

Correct, confirmed.

P1305, line 14: Fig 3 – can you add another part (b) which shows the arctic spinup?

Yes, done.

How about the trend in the arctic surface temps?

The Arctic SST hardly shows a warming trend, despite the deep ocean warming. (Arctic SST trend less than 0.3 K in 1000 years, hard to distinguish from variability)

Section 2.1,2.2 – need to make clear, when including changes in Eocene run, what is done in the preindustrial control, e.g. vegetation, orbit, positioning of poles in ocean model etc.

See above / we would like to maintain the order of the paper.

P1305, line 27 – what time period does PR run over in terms of orbit? 2000BP to modern? Or into the future?

The first year of PR is 800 AD, the last year is 2999 AD; the comparison with PE is based on the 'VSOP87 orbital years' 2800 to 2999 AD. We add this information in the revised paper.

P1306, line 14. Give the modified PR a new name – e.g. PR'. Please make clear what the differences are now between PR' and PE. Is it just land-sea mask, topography, bathymetry and co2? What about veg? I think more could be made of this run in the paper.

The difference between PE and PR' is the land-sea mask and topography, the vegetation, the (now constant as in PE) orbital forcing, and the (now exactly) doubled $p\mathrm{CO}_2$. We now explicitly state this in the revised paper, and we use the suggested naming.

Section 3.3, Figure 5b. Here, the big changes seen at high latitudes are due to the presence of seaice, and in particular the fact that the seaice surface cen get very cold, even if the air above is a low warmer. Of more interest is the SST change itself (which is shown, but due to the vertical scale is hard to see) - I would like to see the 2m air temp change, as I think this will be lower at high latitudes and is more climatically relevant in my view.

The 2m air temperature change is very similar to the surface temperature change (it would be indistinguishable from the surface temperature change line in Fig. 5b). We prefer to use the surface temperature for consistency with the EBMs.

P1311, line 6 – can you give reasons why the EBM gives (slightly) different surface temps to the GCM? Is it the effect of orography in the GCM?

The main reason for the 0-D EBM - GCM surface temperature differences is the meridonal averaging of the albedo and emissivity before applying the 0-D EBM. The meridional averaging must be the main reason, because the 1-D EBM - GCM surface temperature differences are very small.

P1311, line 11 – also cloud differences. This seems more important than surface albedo diffs in the tropics (which are small in zonal mean) if you look at figure 9a.

We agree, and correct it.

CPD

5, C800–C814, 2009

Interactive Comment

P1313, line 18 – CH4 and N2O are different in PE and PR.

True, they are not the same, but they are very similar. We correct the formulation.

Section 4.5 – this is rather weak. Can you tell from your other pre-industrial simulation (PR') the affect of orbit? Also, you mention solar irradiance here so maybe change the title of section 4.5.

We strengthen the Section by using the 1-D EBM, and we change its title. In contrast to PR, the orbital parameters in PR' are constant, still they are not the same as in PE (Table 1).

P1316, line 22 – statement about orography not affecting global mean temperature – actually it can affect global mean temp as the orography change can induce albedo changes, for example if orography rises above the snow accumulation line.

We did not expess ourselves clear enough. A variation of the global mean surface height in itself does not affect the global mean surface temperature in ECHAM5, since the global mean surface pressure is prescribed at 985.5 hPa (assuming a constant mass of the atmosphere). The possibilty of a changed snow-cover as a feedback mechanism to the orographic change is a different story. We formulate this more carefully in the revised paper.

Conclusions – Here you are really talkig about the relative contribution of the different factors to Eocene warmth. But in some cases you use W/m2 and some K, so please be more consistent. A table might be useful for comparison here.

We stick to the effect on temperature in the revised paper.

P1298, line 4: delete warm. Use ^{o}C instead of K? also other places e.g. p1306, line 23.

We prefer keeping the sentence as is. We use Kelvin, because it is the SI unit, and for consistency with the computation of the blackbody radiation.

P1299, line 23 – clarify here you are talking about taking the data at face value.

Done.

P1303, line 13 – surface geopotential.

Ok.

Fig 1 – remove black gridcell outlines.

Done.

P1307, line 22 – reference for seasonal bias in proxies?

Stated by Sluijs et al. (2006) themselves. Clarified in the revision.

CPD
5, C800–C814, 2009

P1309, line 4 – 25%? I find this hard to believe looking at figure 6 but I might be wrong. Precip change averaged over the globe looks less than 25%.

We double-checked the number, it is 24.5%.

P1311, line 11 – according to table 2 this should be 0.040?

No, the surface albedo difference does amount to 0.043, also according to Table 2. The clear-sky planetary albedo difference amounts to 0.040. The difference between the clear-ky planetary albedo and the surface albedo originates from the effect of gases and aerosols on the shortwave radiation.

P1312, line 5 – increase relative to what?

The effect of clouds on the albedo is larger in PE than in PR. We clarified this in the revised paper.

Response to Referee #3

Near line 23, the authors state: "In this study, we aim at reducing this gap between modeling and proxy data for the late Paleocene / early Eocene (PE). . ." However, the paper presents very little comparison between PE model results and proxies – . . .
Finally, zonal mean annual SSTs in the model are only compared with six PE SST proxies. Why only these six? (If the intention is to focus on recent estimates, there

Full Screen / Esc

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Interactive Discussion

Discussion Paper

should be a reference to Hollis et al., 2009). Also, a bit more detail in the comparison to terrestrial proxies in section 3.2 would certainly strengthen this paper.

We aim at comparing our PE simulation to paleodata from the period right before the PETM. Hollis et al. (2009) unfortunately do not cover that period. We think that mixing data from before the PETM and the early Eocene at different locations may lead to a biased reconstruction of the temperature gradient. Such bias could arise, e.g., from the long-term warming trend towards the Early Eocene Climatic Optimum. Moreover, we aim at using relatively recent, and to our knowledge well-preserved paleo-data (especially regarding oxygen isotopes to avoid potential biases towards lower temperatures in the tropics; see Huber 2008). We add data from Tanzania (Pearson et al 2007). However, we are no proxy-data specialists, and would be happy about any other reference to well-preserved proxy-data for the period before the PETM.

It is also not clear what parameterizations in the model allow for the improvement over previous fully-coupled modeling studies. Given that this is the first PE experiment in ECHAM5, I expected some comparison with other early Eocene coupled modeling studies – particularly Huber and Sloan (2001), which was also run with a background pCO2 of 560 ppm. Figure 5 suggests that ECHAM5 produces temperatures at 560ppm in the high latitudes that (while still below those of proxies), are significantly higher than experiments run in the NCAR CSM at 560ppm (Huber and Sloan, 2001), and are comparable to temperatures in higher pCO2 experiments (2240ppm) in the NCAR CCSM3 (Shellito et al, 2009). What is different about ECHAM5?

In the last paragraph, the equable climate of this simulation (PE) is attributed to topography, surface albedo, and effective longwave emissivity. If this is the case, as other models also account for these effects, why is it that ECHAM5 can produce such an equable climate at 560ppm CO2? I'm not sure it is within the scope of this paper to go into a full analytical comparison with other models here, but it must be mentioned.

See above, cumulative answer (A).

The Arctic in this Eocene configuration in connected to other basins via shallow sills. What sort of transport is there across these sills? Does this play any role in Arctic temperature? (I expect this is minor. Or, perhaps this will be examined thoroughly in another paper?)

There is a northward transport of about 0.7 Sv (1 Sverdrup=10^6 m^3s^{-1}) from the Tethys into the Arctic between Europe and Asia, and a small northward transport between Greenland and Europe ($<$0.1 Sv). This Arctic inflow plus net precipitation in the Arctic leads to an outflow through the Bering Strait into the Pacific of about 1.1 Sv. We have not performed bathymetry sensitivity experiments yet. The ocean circulation will be addressed in detail in another paper.

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