Reviewer #1 responses

We thank the reviewer for their constructive critique of our manuscript. Below we outline the changes made to our manuscript in response to their suggestions, or outline reasons why changes were not made.

1. The reviewer suggests we should hedge our rationale for making standardised boundary conditions. Similar to reviewer 2's suggestion, we elaborate on this in the introduction and explicitly state that our reasons for publishing such datasets are that it provides a baseline for multiple groups simulating Eocene climate, and moreover that it will help the community more efficiently identify robust discrepancies between models and data.
2. Both reviewers stress a valid point regarding the representation of uncertainty in our datasets. While we make it clear that these boundary conditions are merely one interpretation of the data we do believe that specific areas of uncertainty should be better highlighted. To that end we have included a figure of bathymetric error (Fig. 3e) and have done a better job of stating which geographic regions suffer the most uncertainty and would benefit the most from future data collection.

   We do, however, wish to re-iterate that we aim for the future development of these datasets to be a community effort. It would be expedient for the majority of the uncertainties to be identified, quantified and hopefully corrected by members of the community who specialise in different geographic regions and Earth science disciplines. As a corollary, we are certain that there are areas of uncertainty in our boundary conditions that we have failed to identify. It is here that we rely especially on community involvement.
3. We have removed the statement that paleogeography is the most important boundary condition in paleoclimate modelling.
4. As we stated in our initial response, a lot of the work regarding our paleotopography has been 'under the hood'. The re-rotation of each continent from the plate rotation model used by Markwick (2007) to that used by Müller et al. (2008) required considerable effort, as did the approximation of sub grid cell topographic variation. Furthermore, while elevations in most regions have been left as they were presented in Markwick (2007), several areas were substantially changed, particularly the geography of the Tethys region (Fig. 1). Use of subsections (as recommended by reviewer 2) hopefully better highlights new elements regarding our paleotopographic boundary condition.
5. The reviewer queries whether the modern topography is a suitable analogue on which to base Eocene sub grid cell topographic variation. While there may be differences between the Eocene and modern in mean roughness, due to an enhanced hydrological cycle, there is simply no way to determine what Eocene sub grid cell topographic variability was. In other words, the modern is the only analogue we have.
6. We have already cited Bice et al. (1998) in the bathymetry section and have changed the reference in the introduction from Crowley and Burke (1998) – the book containing the above reference – to Bice et al. (1998), which should have been our original choice.
7. We have made it clearer as to which references we are referring to when discussing paleo elevation estimates (section 2.2). We also briefly highlight the

positive systematic bias that can exist in isotope-derived estimates of paleo elevation.

8. Regarding the modelling of our Eocene vegetation distribution, there are two main points the reviewer raises. Firstly, that we have only presented one realisation of an Eocene vegetation (one climate model input into one vegetation model). Secondly, that we use a climatology based on 8x pre-industrial CO_2 to force a BIOME4 simulation with only 4x pre-industrial CO_2.

   It is true that it is less than ideal to use one instance of a climate model to force a single vegetation model. Indeed, some dynamic vegetation model simulations are slowly in progress on this front which would alleviate, to some extent, the dependence on one vegetation model. While utilising the EoMIP output would alleviate our dependence on one climate model the vastly different resolutions and paleotopographies used in these models would create large inconsistencies between our simulated vegetation and our paleotopography. For instance, mountain ranges are rather different in shape, location and extent between the topography of Sewall et al. (2000) and that presented in this manuscript (Fig. 1 of manuscript; e.g. the North American cordillera). This is a point the reviewer also raised. This issue may be improved over time if more models adopt the paleo topography presented here and thus provide a consistent set of input for vegetation models.

   The reviewer states "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?" I do not fully understand the statement. The typical execution of an offline model like BIOME4 is via the 'anomaly method' where the difference between the paleoclimate and modern climate simulated by a model is applied to the specific climatology that BIOME4 was built upon (this is touched upon in section 5 of the manuscript). This would account for biases in the control climate experiment. However, given the changes in geography this is not possible for Eocene climate, but we believe that the Eocene-to-modern changes in temperature and hydrology are so large that this is not as big an issue as it would otherwise be (in say, Quaternary simulations).

   Regarding our choice of different CO_2 between our climate model and vegetation model; we stand by our rationale that we are only interested in an 'Eocene looking' vegetation and not so interested in how we get it. How do we know what Eocene vegetation looks like? Using known compilations of data for regions where data exist. This has been noted and cited in the manuscript (Morley, 2007; Utescher and Mosbrugger, 2007). The reviewer is right in that there is a scientific question here regarding the inadequate vegetation growth at 8x pre-industrial CO_2 in our vegetation model and what this could mean for the realism of an 8x pre-industrial concentration in our climate model. This is indeed something we have looked at in the past for the Miocene (Herold et al., 2011). Though we also note that climate models in general appear too insensitive to paleoclimate boundary conditions and that particularly high levels of CO_2 are often used as proxies for the 'missing forcing' (Huber and Caballero, 2011).

   The use of a vegetation model to 'fill in the blanks' is questionable, I agree. But I think it's important to ask whether it's more questionable than intuiting what vegetation may have been in regions where no data exist (as used to be done (e.g. Wolfe, 1985)). As we mention in the manuscript any bias in our climate model can certainly show up in our modelled vegetation (though one benefit of an offline vegetation model is the lack of erroneous positive feedbacks between vegetation and climate). Again, this may be improved in the future as additional simulations are completed with the paleotopography presented here. Our vegetation qualitatively matches most records of paleoflora.

The most prominent bias that our model – and most Eocene models to date – has is an insufficiently low equator to pole temperature gradient. Lunt et al. (2012) show that at 2240 ppmv the NCAR CCSM3 – a close approximation of the model used here – produces a gradient that is almost, though not quite, as low as terrestrial temperature proxies suggest (Fig. 7d of that paper). Our model does a satisfactory job of capturing the present day gradient http://www.cesm.ucar.edu/experiments/cesm1.0/diagnostics/b40.20th.track1.2deg.001/atm_1986-2005-obs/set3/set3_ANN_TS_NCEP_obsc.png

9. Using the topographic gradient is a crude way of representing the river runoff boundary condition – and we stress this more in the revised manuscript – though this is the method used in the overwhelming majority of deep time paleoclimate simulations. Hence there are potentially huge uncertainties in this boundary condition (looking at the modern day, rivers do not necessarily flow directly downhill). We could remove this section from the manuscript but we leave it for the sake of completeness and to highlight the lack of constraints on this potentially important boundary condition.

References

Bice, K. L., Barron, E. J., and Peterson, W. H.: Reconstruction of realistic early Eocene paleobathymetry and ocean GCM sensitivity to specified basin configuration, Oxford Monographs on Geology and Geophysics, 39, 227-247, 1998.

Crowley, T. J., and Burke, K.: Tectonic Boundary Conditions for Climate Reconstructions, Oxford University Press, 1998.

Herold, N., Huber, M., Greenwood, D. R., Müller, R. D., and Seton, M.: Early to Middle Miocene monsoon climate in Australia, Geology, 39, 3-6, 10.1130/g31208.1, 2011.

Huber, M., and Caballero, R.: The early Eocene equable climate problem revisited, Clim. Past, 7, 603-633, 10.5194/cp-7-603-2011, 2011.

Lunt, D. J., Dunkley Jones, T., Heinemann, M., Huber, M., LeGrande, A., Winguth, A., Loptson, C., Marotzke, J., Roberts, C. D., Tindall, J., Valdes, P., and Winguth, C.: A model–data comparison for a multi-model ensemble of early Eocene atmosphere–ocean simulations: EoMIP, Clim. Past, 8, 1717-1736, 10.5194/cp-8-1717-2012, 2012.

Markwick, P. J.: The palaeogeographic and palaeoclimatic significance of climate proxies for data-model comparisons, in: Deep-Time Perspectives on Climate Change: Marrying the Signal from Computer Models and Biological Proxies., edited by: Williams, M., Haywood, A.M., Gregory, J. and Schmidt D.N., Geological Society Special Publication, 251-312, 2007.

Morley, R. J.: Cretaceous and Tertiary climate change and the past distribution of megathermal rainforests, in: Tropical Rainforest Responses to Climatic Change, Springer Praxis Books, Springer Berlin Heidelberg, 1-31, 2007.

Müller, R. D., Sdrolias, M., Gaina, C., Steinberger, B., and Heine, C.: Long-Term Sea-Level Fluctuations Driven by Ocean Basin Dynamics, Science, 319, 1357-1362, 2008.

Sewall, J. O., Sloan, L. C., Huber, M., and Wing, S.: Climate sensitivity to changes in land surface characteristics, Global and Planetary Change, 26, 445-465, 2000.

Utescher, T., and Mosbrugger, V.: Eocene vegetation patterns reconstructed from plant diversity — A global perspective, Palaeogeography, Palaeoclimatology, Palaeoecology, 247, 243-271, 10.1016/j.palaeo.2006.10.022, 2007.

Wolfe, J. A.: Distribution of major vegetational types during the Tertiary, in: Geophysical Monograph, edited by: Sundquist, E. T., and Broecker, W. S., American Geophysical Union, Washington, DC, 357-375, 1985.

Reviewer #2 responses

We thank the reviewer for their constructive feedback. Below we respond to each point raised.

1. In light of comments by both reviewers we have made references to the paleo elevation estimates more explicit (section 2.2 of manuscript).
2. We have added, in the introduction, to our justification for creating these datasets. As the reviewer points out there are multiple challenges faced by the community in simulating Eocene climate and the utilisation of consistent boundary conditions by multiple research groups will help identify robust deficiencies between models and data.
3. The reviewer raises several points regarding uncertainty in our datasets. Firstly, they question the appropriateness of a single paleotopography for the Eocene. Secondly, they suggest a better (perhaps spatial) representation of uncertainty. Thirdly, they question the appropriateness of using modern day topography for calculating sub grid cell roughness. Fourth, they identify an important detail we did not mention originally, that the ANTscape topography was specifically created to represent Antarctica at 34 Ma. The first reviewer shared some of these concerns.

   In response, firstly we re-iterate that we do not propose that this is the 'correct' or most realistic Eocene paleotopography but that this is merely one interpretation of the data available presented within a consistent reference frame. Whether our boundary conditions are suitable for any particular group will depend on the science being pursued. We also intend that these boundary conditions be modified to suit different needs or interpretations.
   Regarding the reviewers second point, we agree that uncertainty was not addressed adequately in the first version of the manuscript and we have been more explicit about which topographic regions uncertainty is greatest (section 2.2). We do not feel a figure is required to show this for topography, particularly since it is difficult to demarcate where exactly uncertainty relating to, say, a particular mountain range should begin or end. Paleo elevation estimates are of course point estimates and we – like most people I imagine – are hesitant to draw geographic boundaries of uncertainty (this might be done with oxygen isotopes where records are relatively abundant (Mix et al., 2011), but all other proxies are far more scarce). We leave this to each user. We do, however, include a bathymetric uncertainty map, which can be calculated at all grid points where oceanic crust exists given this is based on uncertainty in the calculated age of the seafloor (Fig. 3e).

   Thirdly, regarding our use of modern topography for approximating Eocene sub grid cell topographic variability (of which reviewer 1 had similar concerns), we agree this is not perfect though it is in fact the only option available given that such detail is impossible to directly infer for the Eocene. The reviewer does raise a good point, however, regarding Antarctic and Greenland bedrock and we have now utilised this in our approximation of sub grid cell topography (cf. new and old Figure 2). This slightly increases roughness of low elevation areas though the overall effect is small.

Finally, we have now mentioned and briefly discussed our use of the ANTscape Antarctic topography for our boundary conditions especially given that it is representative of 34 Ma (section 2.2). Our main justifications are that this reconstruction is significantly better than using isostatically adjusted modern bed rock and that our use of the 'maximum' Antarctic topography should account to some extent for the younger crust that existed in the Eocene. Finally, given that no substantial continental ice likely existed between the Early Eocene and Eocene-Oligocene transition (Cramer et al., 2011), the uncertainty in using this dataset only arises from regional tectonics and not emplacement of thick ice-sheets. Future ANTscape reconstructions will include early Eocene topographies (http://www.antscape.org/currentwork/) and we anticipate that this would supersede the Antarctic topography used here.

4. We have described the tension factor (section 2.1), which essentially tunes our cubic spline to be less cubic and more (but not completely) linear. The choice of a tension factor of 10 was subjective and, as the reviewer alludes to, complex relief will be less well represented than more broad regions, though given the predominance of the latter this compromise seems reasonable. Nonetheless, in climate models at least, the importance of rough topography lies more with the sub grid cell topographic variation prescribed (section 2.3), which we believe we have treated well.
5. We have re-organised the text to include more subsections, as recommended. This makes navigation easier.
6. The reviewer brings up an important philosophical issue regarding the use of models vs data to construct boundary conditions. We have added a brief discussion at the end of the paper regarding this and justify our use of models for tidal dissipation, vegetation and aerosols (section 8).
7. We now include the original 27 biomes as well as the 10 mega biomes in the accompanying vegetation netCDF file.
8. Lastly, the river runoff is not typically calculated at runtime by climate models. It is a very poorly constrained boundary condition which we include here for completeness. Our method of representing river runoff is admittedly crude and we hope that our demonstration of this will inspire others to conduct a survey of the Eocene geomorphology literature to try and constrain such a boundary condition in the future. We have added discussion to this section (addressing concerns by both reviewers). It is correct that the topographic gradient was used to derive these river directions, and this is clearly stated.

References

Cramer, B. S., Miller, K. G., Barrett, P. J., and Wright, J. D.: Late Cretaceous–Neogene trends in deep ocean temperature and continental ice volume: Reconciling records of benthic foraminiferal geochemistry (δ18O and Mg/Ca) with sea level history, Journal of Geophysical Research: Oceans, 116, C12023, 10.1029/2011jc007255, 2011.

Mix, H. T., Mulch, A., Kent-Corson, M. L., and Chamberlain, C. P.: Cenozoic migration of topography in the North American Cordillera, Geology, 39, 87-90, 10.1130/g31450.1, 2011.