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Interactive comment on "Radiative forcing and feedback by forests in warm climates – a sensitivity study" *by* U. Port et al.

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We thank Referee 2 for constructive comments and questions. Referee 2 found the results generally clearly presented with a few exceptions to be detailed below. Referee 2 proposes new aspects which would be interesting to investigate: a comparison of radiative forcing from simulations with enhanced CO_2 and a detailed analysis of changes in atmospheric circulation. That is indeed an interesting suggestion, but it goes beyond the original scope of our paper. Specifically, a consistent comparison between radiative forcing from land-cover change and from changes in CO_2 concentration would require new simulations and, hence, would require setting up a new project (see Response below).

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Item 1: *page 2578, line 2. The first sentence is strange. I would replace is with a summary of the known impact of forests on climate.*

Response: We agree, and we will skip the first sentence and reformulate the second sentence. A brief summary of the known impact of forests on climate is given in the beginning of the introduction.

Item 2: *page 2578, line 19: T he rationale for analysing the differences between a "forest" world and a "dark desert world", i.e. to get rid of the albedo effect, should be introduced, otherwise this sentence is misleading compared to the sentence on the "forest" vs. "bright desert" worlds on line 10.*

Response: We agree. We add: "and hence, albedo differences between forests and soil are small," in line 19 of the original version of the manuscript. (See page 1, line 15 in the revised version of the manuscript).

Item 3: *The authors could complete their review of studies on the impact of forests/land cover with the study by Alkama et al (Climate Dynamics, 2012) who study the impact of desertification for three different climatic backgrounds.*

Response: We thank Referee 2 for bringing this paper to our attention. We will add a brief assessment at the end of the second paragraph of the introduction (see page. 2, lines 40 ff) as follows: "Alkama et al. (2012) analyse the impact of global desertification in present-day climate, in the cold climate of the last glacial maximum and in a warm climate caused by an increase in greenhouse gas concentrations. They find that the prevailing signal in terms of surface energy budget is dominated by the reduction of upward latent heat fluxes. In their simulations, desertification yields a similar temperature Response for all climate states in regions south of 20°N. At high latitudes the effect of desertification differs because of the difference in snow albedo feedback."

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Item 4: *section 2.1: given that this manuscript investigates impacts of land surface changes, the land surface model JSBACH should be better described, in particular in terms of its set-up. I guess JSBACH is used "just" for its representation of its dynamical feedbacks in this study, and that phenology is not interactive, but it is essential to describe which processes are interactive and which are imposed.*

Response: We agree. We add (see page.3, lines. 83 ff): The JSBACH model simulates fluxes of energy, water, momentum, and CO_2 between land and atmosphere (Raddatz et al., 2007). JSBACH follows the fractional approach to the vegetation cover representation. The land grid cells are divided into tiles (fractions) covered with several plant functional types or PFTs (trees, shrubs, and grasses), and two types of bare surface - seasonally bare soil and permanently bare ground, or desert (Brovkin et al., 2009; Reick et al., 2013). In the JSBACH model setup used in this study, the fraction of land surface covered with particular PFTs is prescribed and phenology (LAI) is kept constant throughout the season in order to simplify analysis of interactions between land and atmosphere.

Additional reference: Raddatz, T. J., C. H. Reick, W. Knorr, J. Kattge, E. Roeckner, R. Schnur, K. G. Schnitzler, P.Wetzel, and J. Jungclaus (2007),Will the tropical land biosphere dominate the climate-carbon cycle feedback during the twenty first century?, Clim. Dyn., 29, 565– 574.

Item 5: *page 2582, 2nd paragraph: Is this savannah vegetation globally homogeneous? Please also quantify the deep ocean temperature trends.*

Response: Yes, the savannah is globally homogeneous. The trend in deep ocean temperatures amounts to 0.04 K/100 y (see revised manuscript page 3, line111).

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Item 6: *pages 2582 (end) and 2583, about model-data comparisons for the initial Eocene simulation (Fig 1). I think this section could be improved. In light of the text accompanying Fig 1, I think it would be appropriate to separate out the ocean and continental Response and give an idea of the deviation from the zonal mean on the figure, e.g. by representing, with a shaded area, +/- one standard deviation of the temperature from the zonal mean. Also, given that some reconstructions could be interpreted as summer temperatures rather than mean annual temperatures, it would also be worth showing the winter/annual/summer results. Nonetheless, presented as they are on Fig. 1, these results appear to compare reasonably with available reconstructions. The additional figures would be useful for illustrating the text in section 2.2.*

Response: Since we do not have all information available on which proxy is representative for summer or winter temperatures, we prefer to stick to annual mean values. But regarding geographical interpretation, instead of presenting zonal mean values with point data, a new figure (see attached figure) might be more instructive.

Item 7: *page 2588: Is epsilon constant throughout the simulations?*

Response: The emissivity is not constant throughout the simulations, nor is the temperature in Equation 12 is a constant. However, the Planck feedback parameter and the other feedback parameters are computed from the linear regression, thereby implicitly assuming that feedback parameters (i.e., the product of emissivity and T^3 in the Planck parameter) do not change over time. This is one of the limitations of Gregory's method as explained in the text, for example at the end of Section 4.1.

Item 8: *page 2589 and fig 6. I think the figure should also show the differences in surface albedo, to allow for a better appreciation of the impact of cloud changes on planetary albedo.*

Response: We prescribe globally homogeneous land-surface conditions. Hence

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differences in surface albedo are globally homogeneous as well. The surface albedo is included in the planetary albedo. Therefore, we would like to keep Figure 6 as it is.

Item 9: *page 2590, 2nd and 3rd paragraphs, on* $\Delta\ Q_{SWcl}$*. Although the terms "cloud adjustment" and "cloud masking" are well defined in these paragraphs, I don't think these names are the appropriate ones. Instead of the unclear sentence "* $\Delta\ Q_{SWcl}$ *composes of the cloud adjustment on one hand and the masking effect by clouds on the other hand", which is quite puzzling when read for the first time, one could write "* $\Delta\ Q_{SWcl}$ *describes the radiative impact of changes in cloud cover as well as an indirect impact of clouds which we define below as "masking effect"".*

Response: This is a sensible suggestion. We will modify the sentence in question in the revised version of our manuscript (see page 11, lines 301f).

Items 10: *The relationship between cloud cover and circulation changes could be discussed e.g. at the end of the 2nd paragraph after line 15.*

Response: see Response to Item 12.

Item 11: *page 2591, comparison with the results from Caballero and Huber (2013). Can the Response to GHG compared to the Response to changes in land surface be compared? Your set of simulations could help and compare, as well as simulations under* $2xCO_2$ *or* $4xCO_2$ *forcings.*

Response: This is an interesting comment. Therefore, we add a word of caution that the Response to CO_2 forcing might not be comparable with the Response to changes in land surface (see page 13, lines 325ff). To evaluate the effect of different external drivers on radiative forcing and feedback, new simulations with boundary conditions that allow for a consistent analysis have to be set up. This, however, is beyond the

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scope of this project which was finished last year.

Item 12: *page 2592, about the possibility of state-dependent cloud feedback. On fig 6, it does indeed seem that the cloud cover changes are different for the Eocene and pre-industrial changes, although on the very large scale they are comparable. This could be related to atmospheric circulation changes (cf. comment above) or to Eocene vs. pre-industrial differences in land/ocean/altitude/bathymetry configurations. It would be worth including an analysis, or at least some thoughts, on this topic. Also, if low clouds are to be blamed, why not showing and commenting them?*

Response: These are indeed interesting additional points. However, new analyses would require a considerable extension of the project. We could thereby add some information without, however, changing the main message of our paper. As we have outlined in our conclusion, we assume that the main results are robust, while the results regarding cloud feedbacks are likely to be model specific.

Item 13: *page 2592, last lines of section 4.1, commenting Fig 9, right panel: yes, the last 250 years behave very differently from the first 150 for the Eocene case. In fact, they seem to "catch up" with the pre-industrial behaviour. It would be good to comment on this fact.*

Response: At the moment, we do not have a good explanation why, in contrast to the other cases, only for the short wave radiation at the top of the atmosphere over the Early Eocene ocean reveals this peculiar behaviour. We checked whether the ocean temperatures would reveal any trend reversal, but we could not find anything. We will comment on this problem in the revised version of our manuscript (see page 15, lines 367ff) as follows: "Figure 9b, however, indicates that the TOA cloud short-wave radiation over the oceans seems to evolve non-linearly with temperature in the early Eocene. There is no reversal in the trend of ocean surface temperature detectable in

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the Eocene simulation between the first 150 years and the last 250 years. Hence we are currently not able to explain this behaviour."

Item 14: *page 2593, line 10: on feedback parameters on the regional scale: please explain and possibly illustrate what is meant here.*

Response: At a second glance this sentence does not make sense indeed. Feedback parameters are evaluated globally, not locally. We tried to do a Gregory analysis for regional land-cover change. This, however, failed because of the small signal to noise ratio. Therefore, we delete the sentence.

Item 15: *Conclusions, last page: at this stage, commenting on the simplified set-up, it would be useful to comment on "real world" soil albedo vs. forest albedo. Which regions are "dark" vs "bright"?*

Response: This is a sensible suggestion which we have implemented (see page 18, lines 416ff): In the real world, the values of surface albedo are within the range of the values prescribed in our sensitivity study. In most regions, the surface albedo is much closer to the low value of 0.1 than to the high value. Only in some desert regions with desiccated palaeo lakes, like the Bodélé depression in Northern Africa today, values are as high as 0.4. We assume that this is valid also for the Early Eocene climate. Therefore, we assume that our results for the dark soil are applicable to the real world qualitatively albeit with a smaller amplitude of values.

Minor comments
page 2578, line 5: add an s to "type" and "climate"– Obsolete, as text Is changed in the revised version.

page 2579, line 15: no s at the end of "warms"– Done

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page 2579, lines 23-24 “which presumably was such a warm, nearly ice-free climate” could be replaced by “which was a warm, presumably nearly ice-free climate” – Done

page 2583, line 25: “as realistic as possible” to be replaced by “as realistically as possible” – Done

page 2586, line 20-21: “aim at estimating” instead of “aim to estimate” – Done

page 2587, line 1: “define” instead of “receive” – Done

page 2587, line 13: add “The” at the beginning of the sentence – Done

page 2587, line 15: “consist” instead of “constitute” – Done

page 2588, line 13: “stabilises climate” to be replaced by “stabilises the climate” – Done

page 2588, line 21: add “respectively” after “Eocene climate” – Done

page 2590, as well as in the conclusions: “Feedbacks stabilise the early Eocene stronger than the pre-industrial climate”. This sentence could be reformulated as “The feedbacks stabilising the climate are stronger for the Eocene than for the pre-industrial case”. – Done

page 2593, last lines of section 4.2: “can likely be attributed”. This could be checked so that the “likely” is removed from the sentence. – To isolate the sea-ice albedo feedback from the other feedbacks cannot be easily done. A feedback separation which would require new simulations is beyond the scope of the study.

page 2595, line 2-3. Do the authors mean “We assume that this simplification will only weakly affect the results of our study, at least in the qualitative sense”? – Yes, thanks for pointing at this cumbersome formulation.

Figs 5, 7 and 11: difficult to see the hatching. – This might be a matter of the resolution of the printer. On our print outs and on my screen, the hatching is clearly seen. We will

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contact the publisher regarding this issue.

Fig 4: would benefit from a similar treatment to Fig. 9, with the first years being indicated a different color. – In Figure 4, only the first 150 years are shown, while in Figure 9 both the first 150 years and the last 250 years are shown. Hence a similar treatment in both figure does not seem to be sensible.

Interactive comment on Earth Syst. Dynam. Discuss., 6, 2577, 2015.

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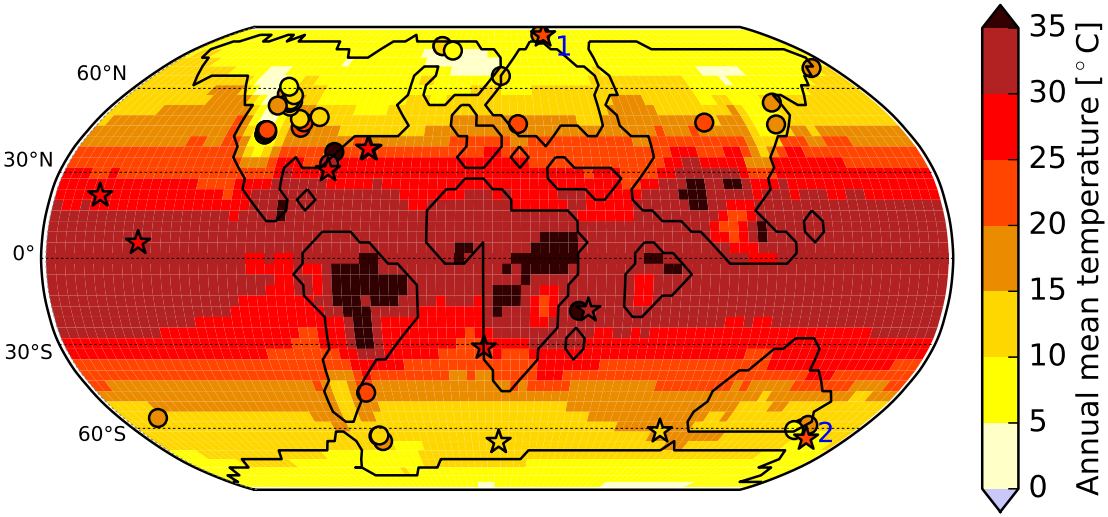

Fig. 1. Annual mean 2-m temperature in the early Eocene simulation in shaded colours. Reconstructed annual mean SST and terrestrial temperature of the early Eocene are shown in stars and circles.

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