Clim. Past Discuss., 4, S576–S579, 2008
www.clim-past-discuss.net/4/S576/2008/

Climate of the Past Discussions

Interactive comment on "Exploring the climatic impact of the continental vegetation on the Mezosoic atmospheric CO_2 and climate history" *by* Y. Donnadieu et al.

Anonymous Referee #2

Received and published: 24 November 2008

In this study, Donnadieu et al. estimate the physical influence of plants on past climate and atmospheric pCO2. They show that a "reasonable" distribution of plants leads to an increase in pCO2 due primarily to the occurrence of deserts and a reduction in silicate weathering. This modeling study is innovative, and nicely builds on previous work by Donnadieu et al. It should be published after moderate revision. I think that clarification of some of the major results, as well as further consideration of the implications and conclusions, is required before the paper can be published.

Both the Methods and Results sections are inadequate. Concerning Methods, it is impossible to understand the methodology based on the description given in the pa-

per. Specifically, information needs to be added about the boundary conditions (orbital parameters, solar luminosity), and the GCM. (A table summarizing the BCs would be ideal.) The fact that FOAM is being run with a mixed-layer, rather than full, ocean model should be stated. Otherwise, the 30-year iteration is completely inadequate. The coupling between FOAM and LPJ is not described. Are 30-years sufficient to bring the system to equilibrium? How many years is LPJ integrated for? Does the surface lithology (granite vs. basalt) evolve through the Mesozoic, or is it assumed to be the same throughout? Is this appropriate?

The Results also suffer from a lack of detail. The reduction in silicate weathering in all simulations is attributed to a decrease in global runoff. It would be useful to see the magnitude and distribution of the change in runoff either as a figure or in a table. Donnadieu et al. attribute the decrease in runoff to "...changes in thermal properties but also in roughness" but never fully explain. How exactly is the change in roughness affecting the hydrologic cycle? How important is this compared to changes in the thermal properties? Apparently the increase in deserts "induces cooler tropics" that reduces silicate weathering. This may be the case, but it is impossible to tell from Fig. 3. Is the silicate weathering so sensitive to temperature changes that imperceptible temperature changes can cause a factor of 2 change in pCO2? (Please illustrate the tropical cooling.) The analysis of the effect of vegetation on pCO2 is pretty shallow. Are deserts the only factor that influence pCO2? Vegetation types don't matter? Why not plot global average roughness or vegetation albedo of all the vegetation, and then the non-desert components?

The divergence of continental and marine temperatures in Fig. 4 is interesting. The authors adequately explain that continental cooling is due to continental drift to high latitudes. The marine temperatures are a bit harder to understand. The pCO2 is falling, and yet the marine temperatures rise. Is the 2C rise in marine temperatures also due to a net decrease in high latitude ocean?

The comparison of the simulated vegetation with the global mixed deciduous vege-

CPD

4, S576–S579, 2008

tation is unfortunate. It would be more straightforward if the comparison were with barren ground. Confusion may arise in the summary of the main points. For example, in point 1 on p. 1031, the authors write "free evolving vegetation induces a cooler and drier world". In fact, free evolving vegetation induces a warmer and wetter world, only in comparison with the artificial deciduous vegetation world is this statement true. (I know that the authors are fully aware of this. My comment is that this needs to be made very clear to avoid any confusion.) Donnadieu et al. also emphasize "the need for explicit modeling of continental vegetation in the modeling of past pCO2". But, it seems that one of the major points is that the pCO2 is very sensitive to subtropical deserts, but rather insensitive to the details of the continental vegetation? If this is the case, then is explicit modeling of continental vegetation necessary, or just a description of the desert locations (which might be better prescribed by looking at geologic evidence)?

In the Discussion, the authors assert that "our mid-latitude temperature estimates fall on the mean trend of the data set and are in good agreement in terms of absolute values...These results support our conclusion that the continental configuration is the first order process." The authors are making claims for which they do not have sufficient support. The long-term trend (shown by the gray line in Fig. 6) is mainly defined by the Cenozoic cooling. There are no Cenozoic simulations in this study (and the paleogeography evolves little throughout the Cenozoic). The authors put a favorable spin on Fig. 6; but, it could be just as easily (and more accurately) be argued that Fig. 6 shows the deficiency of their model. It fails to simulate any of the Mesozoic variability. This might be due to unaccounted for, shorter term processes as the authors argue. But how can we differentiate between this possibility, and the possibility that their model is simply wrong? Minor comments. 1. p. 1024. The authors comment that effect of land plants on Earth have been "neglected up to now". Of course this isn't true, many climate studies have looked at the effect of plants on climate. Some of these should probably be cited. 2. p. 1024. "These simple tools are useless regarding our purposes." I bet there is a more congenial, less adversarial way of stating this. 3. p. 1025, line 4, delete "down". p. 1027, line 6, add "categories". p. 1028, line 13, delete

CPD

4, S576–S579, 2008

"but". p. 1030, line 26, replace "are diverging" with "diverge". p. 1033. Line 1, replace "exported in" with "applied to". 4. p. 1029. In the explanation of a wetter Cretaceous world, the authors cite the higher sea level as the reason. But, isn't it more likely the fact that continents are distributed rather than configured as a single supercontinent. I think there are lots of references to support this. 5. The text on p. 1039 (lines 5-15) is very difficult to follow. This is made more difficult by the fact that Fig. 14 doesn't have a pCO2 scale. Couldn't the right-hand y-axis be pCO2?

Interactive comment on Clim. Past Discuss., 4, 1021, 2008.