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Climate of the Past Discussions

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Interactive Comment

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.

Y. Donnadieu et al.

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We would like to thank the anonymous referees for their constructive comments and their support for the publication of this paper. The first reviewer has minor comments, mostly technical, that we have taken into account into the revised version of our manuscript. The second reviewer has highlighted different aspects of the paper for discussion and improvement, as such we will respond more specifically to his review. 2 figures and 2 tables have been added in the revised version.

Reply to comments of the Referee 2

Concerning Methods, it is impossible to understand the methodology based on the description given in the paper. 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?

Answer: Most of required informations have already been given in our two preceding papers (Donnadieu et al., 2006, G-cubed, Goddéris et al., 2008, EPSL). Nevertheless, we have modified our paper in order to clarify all the raised points.

For the climatic simulations, the Earth's orbit around the Sun is circular (eccentricity = 0) and the Earth's obliquity is 23.5° (this setting leads to an equal annual insolation for both hemispheres). Solar luminosity is assumed to evolve through time according to the stellar evolution models (from 2.5 %reduction in the Early Triassic to 0.6% in the Maastrichtian (Gough, 1981)).

The atmospheric component of FOAM is a parallelized version of NCAR's Community Climate Model 2 (CCM2) with the upgraded radiative and hydrologic physics incorporated in CCM3 v. 3.2. The atmosphere runs at R15 spectral resolution (4.5° x 7.5°) with 18 levels. The ocean component (OM3) is a z-coordinate ocean model that has been optimized for performance and scalability on parallel computers. OM3 contains 24 vertical layers, a 128 x 128 grid (1.4° x 2.8°) and uses simple second order differencing and a fully explicit time step scheme for the barotropic and baroclinic modes. FOAM successfully simulates many aspects of the present-day climate and compares well with other contemporary medium-resolution climate models; it has also been used previously to investigate Cretaceous and Neoproterozoic climates (Donnadieu et al., 2006; Poulsen, 2003; Poulsen et al., 2001, 2002). For this study, we use FOAM in mixed-layer mode, i.e., the atmospheric model is linked to a 50-meter mixed-layer ocean, which parameterizes heat transport through diffusion, mainly for computation

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time considerations (each GEOCLIM simulation requires up to 12 GCM simulations, as described below). Hence a 30-years iteration is completely adequate.

Otherwise, the coupling between FOAM and LPJ was described in the submitted version of our paper on page 1028, l.13-20. Using the monthly average temperature, precipitation, cloud cover and insolation provided by a one-year run of FOAM, LPJ is run. Then, vegetation types are modified accordingly in FOAM and the GCM is run again for one year. Then, we force LPJ with the new climate etc We have checked for the equilibrium of the vegetation by looking at LPJ variables such as the geographical distribution of PFTs and it appears that PFTs reach the steady-state after 5-10 integrations. A 30-years iteration is thus sufficient for both GCM and vegetation equilibriums.

The relative proportions of silicate (and among them of granitic and basaltic lithologies) and carbonate outcrops is assumed to be the same in each grid cell because of the current lack of precise lithological control. These relative proportions are not expressed in terms of relative area, but rather in terms of contribution of each lithology to the CO_2 consumption flux, so that total silicate weathering reaches 13.6 1012 moles/yr (with 30% due to basaltic weathering), and carbonate weathering 23 1012 moles/yr. This requires that under the same climatic conditions (in terms of mean annual air temperature and continental runoff), the contribution to the CO_2 sink per m2 through granitic or basaltic and carbonate weathering is identical for each continental grid cell changed for all past simulations. In summary, while spatial resolution of the weathering has been introduced, we chose to keep constant the fractions of silicate, basalt and carbonate outcrops for all time slices and for each grid element. Indeed (Gibbs et al., 1999) have demonstrated that the changes in lithology since 250 Ma weakly influence the calculation of the weathering fluxes. This result, although surprising, is mainly the consequence of the rather constant zonal relative abundance of each lithological type.

Reviewer: 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

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in a table. Donnadieu et al. attribute the decrease in runoff to "...changes in thermal proper ties 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?

Answer: First, let us remind here that our main goal was to determine the influence of evolving vegetation on our long-term CO2 trend. As described on the figure 1 of the paper, "calculated atmospheric CO2 for the 7 time slices of the geological past show the same general trend as the one simulated in Donnadieu et al. (2006) (noted D06 thereafter) (Fig. 1), a more or less monotonic decrease punctuated by a large fall at the end of the Triassic. Nevertheless, implementing vegetation as a function of climate induces changes in the absolute values of PCO2. Atmospheric CO2 is always higher when the terrestrial vegetation model LPJ is used." Because the ratio between atmospheric CO2 calculated for each set of simulations (with or without interactive vegetation) remains more or less constant around 2, and because the long-term trend was already described in D06, we have largely reduced this part. However, in order to satisfy the reviewers request, we have added a new table in the paper that shows, for each timeslice, the mean continental temperature and the mean runoff calculated with FOAM and a uniform vegetation cover and with FOAM coupled to LPJ. One can see on this table that the dynamic vegetation induces lower temperature and runoff. We hope that it is making clearer the reasons of the systematic increase in pCO2 between GEOCLIM runs and GEOCLIM-LPJ runs. As discussed more deeply below, we are well aware that dynamic vegetation experiments if compared with non-vegetated experiments should lead to the inverse conclusion, i.e. dynamic vegetations induce warmer temperatures and larger runoff. But the fact here is that in our 2006 G-cubed paper we had chosen to impose in our climate runs a uniform continental vegetation distribution. Concerning specific questions on the separated impacts of thermal properties changes and of roughness changes, we think that this is out of the scope of our paper. Indeed, it will require that we develop a specific coupling between LPJ and FOAM in which we should keep constant the albedo and the potential evaporation of continents to the value of

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the uniform vegetation experiments while changing the roughness of continents as a function of the PFT geographical distribution. The same experiment should be done but with varying albedo and fixed potential evaporation and roughness values to those of the uniform vegetation experiments. And a third experiment to test the impact of the potential evaporation will complete the study. These three experiments should be done for each continental configuration. Then, we should be able to state on effects of roughness, albedo and potential evaporation changes on the runoff.

Reviewer: 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?

Answer: Because our comparison is between simulated vegetation and uniform mixed deciduous vegetation, the largest changes in the biophysical properties of the surface are focused on the tropical belts where large barren areas replace the previously artificial mixed deciduous vegetation. Indeed, on the table below, on can see that values of roughness, potential evaporation and albedo for various type of vegetation remain on the same order while values for sandy surface are not. As a consequence, while FOAM simulations with simulated vegetation are globally cooler than those with fixed uniform vegetation (see Table 1 of the revised ms), the cooling is not uniform and is more prominent over the tropical latitudes (see Fig. 4 of the revised ms). In the same way, when plotting the changes in albedo, one can see that the largest changes are located over the latitudes where deserts take place (Fig 3 of the revised ms). Hence, a preliminary conclusion is that: 1) The occurrence of large deserts in the climatic runs forces our model to reach a new state in which the continental temperatures are globally cooler.

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As a consequence, the runoff also decreases 2) Decreases of temperature and runoff lead to a reduction of the silicate weathering rate 3) Because of the relative similarity of the biophysical properties of the various vegetation types, changes attributable to the vegetation types are more subtle and seem to be of secondary importance.

Reviewer: 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?

Answer: Yes, or an equivalent increase in equatorial oceanic area where more energy is absorbed inducing a compensating effect.

Reviewer: The comparison of the simulated vegetation with the global mixed deciduous vegetation 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)?

Answer: We agree with the referee and we have added in the revised version that the same study but aiming to compare climatic runs with vegetation with climatic runs with barren ground will result in a totally inverse conclusion. It is also true that the

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pCO2, in our model at least, seems rather insensitive to the details of the continental vegetation. Nevertheless, we have only explored here the biophysical effect of plants on climate and CO2. Plants also influence climate and CO2 through their roots system (weathering enhancement compared to barren ground) and the induced organic carbon burial. Neither of these effects has been accounted for in this study. However, contrary to biophysical values that are relatively similar for the vegetation types of FOAM, we may suspect that the quantity of CO2 absorbed by plants and the root system influence on weathering markedly vary for the various vegetation types. This work remains to be done, but requires to account for a process-based description of the weathering processes (a real challenge, even at present day). That being said, we agree with the reviewer that one of our conclusions may be that at the first order, the pCO2 is very sensitive to subtropical deserts, but rather insensitive to the details of continental vegetation.

Reviewer: 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?

Answer: The objective of our work is to simulate the impact of the horizontal tectonic of the climatic evolution. Of course our model cannot match the short term fluctuations of the climate which are due to a very large variety of causes. Our simulated climate

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defines a baseline for the climate evolution over the Mesozoic. The fig 6 helps at isolating the factors at play. When our simulated temperature crosses the 18O temperature, this may correspond to times where the paleogeography is controlling the global climate and carbon cycle. Any departure reveals the existence of other factors, such as the solid Earth degassing or disequilibria in the organic carbon subcycle. This is for instance particularly striking for the Mid Cretaceous warm event.

The former model used in deep time carbon cycling studies was the GEOCARB model. This model relies on phenomenological laws for the climate (CO2-temperature dependence, T-runoff function) and for the geochemical processes. Those laws are calibrated on the present day continental configuration. The role of the paleogeography is also introduced but again as a phenomenological parameter. Here, we are moving towards a numerical model describing the real physics of the climate (3D GCM) coupled to a geochemical model as mechanistic as possible. For this reason, we suggest that the output of our model may be consider as a possible "real" value of the CO2 level in the Earth atmosphere, but only in response to the sole forcing considered here: the continental configuration. The fact that the model line falls in the middle of the data range suggests that our model defines the baseline.

There is a confusion here probably linked to the x-axis of fig 6. We are not discussing any long term cooling trend throughout the Mesozoic and Cenozoic, but only the climatic oscillations around our baseline in the Mesozoic. To avoid any confusion, the fig 6 has been changed and the long-term trend focuses only on the Mesozoic.

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.

As we originally wrote in the text, we talk about the effect of plants through albedo and roughness on climate and at longtime scale. And, to our knowledge, the previous climate-carbon model has never considered this effect because those models did not

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have GCM to represent the climate but rather OD or 1D climate model (such as Energy Balance Model).

2. p. 1024. "These simple tools are useless regarding our purposes." I bet there is a more congenial, less adversarial way of stating this.

We have replaced by "these tools can not be used regarding our purposes."

3. p. 1025, line 4, delete "down". p. 1027, line 6, add "categories". p. 1028, line 13, delete "but". p. 1030, line 26, replace "are diverging" with "diverge". p. 1033. Line 1, replace "exported in" with "applied to".

Done

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.

Our sentence was misleading; we did not want to explain the wet Cretaceous world with the sea level. It was more a sentence meaning that the Cretaceous and particularly the Cenomanian is a time period during which the sea level was very high. There is no relationship between both statement through as a climatologist, I know that high sea level favors increase hydrological cycle. Hence, we modify the sentence accordingly.

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?

Done

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