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Geoscientific Model Development Discussions

Interactive comment on "Implementation of methane cycling for deep time, global warming simulations with the DCESS Earth System Model (Version 1.2)" *by* Gary Shaffer et al.

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First of all we would like to thank Referee 2 for reviewing our work and for her/his positive comments on it. Here follows our responses to specific points raised, points that are enclosed in quotes below.

"...The first is to reiterate David Archer's comments that some first order description of the model being modified would be useful – in the atmosphere, how is water vapor, heat transport, and temperature calculated? How does the ocean evolve? etc. ..."

In response to similar comments by both referees, we now will also include several

additional figures and additional text on our atmosphere and ocean modules in our revised version.

"...The second is that the relationship expressed in section 3.1 between the lifetime of atmospheric methane and its concentration seems to implicitly include an assumption of the water vapor concentration dependence on temperature, and a temperature dependence on the methane concentration. Clarifying the nature of that assumption (what climate sensitivity is used?), and its robustness (does it matter the precise values chosen?), would be useful..."

As described in our manuscript the relationship we derive between the lifetime of atmospheric methane and its concentration is based on fitting a simple generic function (Shaffer et al, 2008, GMD) to results from three different studies using models that include complex atmospheric chemistry (Schmidt and Shindell 2003, Paleoceanography; Lamarque et al 2006, Paleoceanography; Isaksen et al, 2011, Global Biogeochemical Cycles). The results of these models imply that the link "water vapor concentration dependence on temperature, and a temperature dependence on the methane concentration" does not affect their (nor our) results significantly. For example, Schmidt and Shindell write "In the Paleocene, warmer temperatures will have likely lead to increased atmospheric water amounts that affect the production of OH. An estimate of H2O increases in a greenhouse world would be about 30% for a 4°C temperature increase, making the reasonable assumption that relative humidity is roughly constant [IPCC, 2001]. Over a range of GCM experiments, the increase in OH for this magnitude change is less than 10% [Grenfell et al.,2001, personal communication]. This actually leads to an increased sensitivity to CH4 changes, though not by a significant amount." And Isaksen et al. write "In the calculations we have used current atmospheric water vapor content. Since water vapor is expected to increase in a future warmer climate the calculations were repeated for a 40% increase in tropospheric water vapor (but no other changes). ..... We found that the calculated tracer and lifetime perturbations were only slightly affected by this increase (less than 10% impact)". We will mention

this insensitivity in our revised version.

"...The third is that in the last paragraph of page 11, an exponential fit is used. It is not clear to me, but it seems possible that this fit is being applied in regions outside where the fit is performed, which seems like a potential issue..."

The atmospheric temperature results we report in our manuscript all lie within the range 0 to 40°C. The exponential fit we derive to data spanning this range with 5°C increments is excellent (R2 = 0.997) so we see no issue here.

"...The fourth is that I wish there was a more quantitative comparison of the changes induced by the new radiative forcing values, as well as a more explicit discussion of how the addition of the mechanisms that have been added to the Earth system model either confirm or complicate the more schematic picture paleoclimatologists may typically have of these events".

In response to the first part of this comment, in our revision we will extend the discussion found in the first full paragraph on page 6 to compare warming that would be calculated using the old and new radiative forcing values for several specific combinations of pCO2 and pCH4. With regard to the second part of the comment, this is a good idea. However we feel that this is better covered in the follow-up manuscripts on specific deep-time, global warming events that we're working on rather than in the present model description paper.

"...Finally, and building on the last point, following the causal chains of physical processes discussed in Chapter 4 can sometimes get confusing. It might be useful if there were some diagrams illustrating how the various graphed quantities from the figures influence (and feedback on) each other, illustrating the chain of connections leading out from the initial methane release. Comparing these diagrams between cases could then help clarify the qualitative difference between, for example, deep ocean and surface release of methane...."

The figures in Chapter 4 are our best attempt, after much thought and effort, to present these types of relationships in a clear, comprehensive and attractive way; referee 1 thinks that we have succeeded with this ("...The results are clearly and attractively presented in the figures.."). We feel that figures along the lines requested would fit in better in our work in progress outlined above.

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