

Interactive comment on “A model of the methane cycle, permafrost, and hydrology of the Siberian continental margin” by D. Archer

Anonymous Referee #2

Received and published: 28 October 2014

Review of the paper A model of the methane cycle, permafrost, and hydrology of the Siberian continental margin by D. Archer

This paper presents a 2-D model of the methane cycle for the Siberian continental margin, including permafrost, hydrates and transient fluxes. Model is described in a differential way compared to the initial version used for another region (section 2). Results are presented (section 3) concerning initial spinup, impact of freshwater hydrology, glacial cycles and upcoming global warming. A discussion on the model limitations and implications of climate change on Siberian methane fluxes close the paper (section 4).

General comment

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This long paper presents a lot of interesting and original results about the methane cycle in the Arctic. The question of methane emission from the Arctic regions has known an increasing interest these last years. However, a process-based model of methane emissions to the atmosphere, including all sources and sinks from under the permafrost to the atmosphere through the sediment and the water column does not exist to my knowledge. Many processes occur making this model very difficult to build, although very important to be built to quantify the possible methane feedbacks in the Arctic. To my opinion, this paper is a step towards such a model. It has the strength to integrate several processes in the same model (and then be able to compare their relative effects) and the weakness to be obliged to make a lot of assumptions and simplifications, which appear clearly and honestly in the text but may limit the robustness of the conclusions. I can understand that such a model can be easily criticized by scientists studying individual processes. However, it brings elements to the question of methane in the Arctic for regional scale and long-term changes, which I find important and useful. The long-term (several centuries) perspective given at the end of the paper for the evolution of methane flux to the atmosphere is of particular interest to me, so is the glacial/interglacial detailed analysis proposed. The paper is rather dense and most of my specific comments ask for clarification/shortening of some parts to help the reader do through the paper. I also suggest to add recommendations in section 4 to improve the constraints of model parameters : what observations would be needed to constrain these parameters ? What is missing to have a complete model of methane fluxes from below permafrost up to the atmosphere ?

Specific comments

P7857, l7-18 : this paragraph is already a result and should be moved later in the paper please. precise what is originally the SpongeBOB model in one sentence before indicating the modification made in the model.

P7858, l22 : What kind of observations are used to tune the parametrisations. A reference is needed.

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P7861 l3-7 : did you evaluate the impact of this elastic hypothesis ?

P7862 : it is not clear to me how the different parameters for the canyons are chosen. Please precise this more clearly.

P7863 : please precise the unit of FREEZEmax and more clearly its physical meaning. V is not defined (pore volume ?)

P7865 : It is not clear to me why the crude representation of methane fluxes to the atmosphere allows to capture the sensitivities to the different processes. Please be more precise here.

P7866 : Isn't a more extensive comparison with Romanovski (2005) possible ? Else, as it is, the section is not really a comparison with other models but just a notification of different approaches & hypotheses

P7866 l20-25: it is said that permeable channels have a strong impact (not so clear on fig 4d) but at the end they seem to be neglected. Please clarify this paragraph

P7868 l1-5: what is more realistic, 10km canyons or 100km canyons ? The reason of 100km is not clear to me.

P7868 l25- 28: please explain more in details the methane structure of fig7 and how the groundwater pumps impacts it.

P7869, l18 : is it possible to plot air temperature as well on figure 8 ? Maybe as a second Y scale on fig8a ?

P7871, l1-5 : so the conclusion here is that the flow on fig 10h is an artefact or not ? please clarify.

P7873, l3-5 : Are there reason to think that all factors are homogeneous all along the width of 1000km ? More justification for this simple extrapolation must be given here.

P7873, l14 : "solid regions are cumulative methane sinks" : unclear sentence, please

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rephrase and link to figure 15

P7874, l1 : as I understand figure 15, the spikes are oxidation spikes in the sediment (grey) and in the water columns (blue), linked to the release of trapped methane. The text gives the impression of a source whereas it seems to me that fig 15 shows the destruction of this source by oxidation. Please clarify this paragraph and possibly legend of figure 15 more clearly indicating sources and sinks.

P7874, l10-20 : you said before that the representation of methane flux to the atmosphere was crude in the model. What uncertainty can you put on fluxes of figure 16?

P7876 : As noticed in the paragraph many parameters of the model remain poorly constrained. Could you make recommendation to improve this situation ? What kind of observations would be needed to constrain the more sensitive of the unconstrained parameters ? A few sentences on this question would be important here.

P7876, l26-27 : Among all the missing or simplified processes listed before, can you find one or more that would imply a much larger methane flux to the atmosphere. Considering the large emissions proposed by Shakhova in 2010 (8Tg/yr for ESAS), such an analysis would be interesting to be done here.

P7877, l5-10 : Can you comment here the 8Tg/yr estimated by Shakhova et al 2010 for ESAS in regards to the methane flux to the atmosphere you derive with your model ?

Interactive comment on Biogeosciences Discuss., 11, 7853, 2014.

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