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# *Interactive comment on* "Assessing the potential for non-turbulent methane escape from the East Siberian Arctic Shelf" *by* Matteo Puglini et al.

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Puglini et al comments I have a lot of respect for the sophisticated details of the diagenetic reaction-transport model BRNS described in the manuscript by Puglini et al. It is a sophisticated, well-established model framework and has been used in many important publications, not the least already in the sensitivity analysis of anaerobic oxidation of methane in many different marine settings. This study takes advantage of the long developmental work that has been done previously with respect to AOM with this model. Here it is used to simulate sediment methane cycling for one of the big hotspots for potential future marine methane emissions – the East Siberian shelf sea, with its potential for thawing submarine permafrost and the potential presence of gas hydrates (although the presence of both is often contested in the literature for good reasons).



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Discussion paper

The model uses the conventional setup of a network of biogeochemical reactions directly or indirectly coupled to the degradation of organic matter deposited at the sea floor. The paper is mostly not about the Siberian shelf, but is a very thorough assessment of AOM dynamics with explicit treatment of upward flow, bioenergetics controls of AOM, and a complex reaction network of biogeochemical redox reactions as they may occur in Siberian shelf sediment. The manuscript is well written up section 3.3.1., after which it deteriorates conspicuously. In principle, there were two objectives: 1. Broad-scale simulation of AOM dynamics: It does a very good job at simulating a range of broadly set environmental conditions with direct impact on the filter efficiency of anaerobic methane-oxidizing microbial consortia that use methane and sulfate. The range of the environmental conditions is set broad enough to encompass conditions that may be encountered on the East Siberian shelf. However, this part is not very novel and AOM dynamics and filter efficiency have been reviewed by Regnier et al. (2011) previously. Therefore all sections of the manuscript that relate to the simulation tests should be significantly shortened. 2. Regional application: The second part of the manuscript is the application of the model to the East Siberian shelf. I found this part the more relevant one, given the title, but unfortunately also less well constrained due to the paucity of data used to constrain their model in face of the diversity and size of the targeted marine region. For reference, my guess is that the authors would certainly not model the whole of the North Sea or the Baltic Sea with this model, two marginal seas of similar size or even smaller than the Laptev Sea. My specific critique relates to the following points, which to my opinion are important in controlling the biogeochemical rates and flux output of the model, but that are not or too poorly constrained in the model to substantially further our understanding of how efficient anaerobic methane oxidation is and will be in the Siberian shelf sediments. âĂć Even with the reduction of the investigated area to the Laptev Sea only, the depositional environments and geological settings are so much more variable that a simple sedimentation rate/bathymetry-based prediction of present-day organic carbon accumulation gives a starting condition for the model that is too simplifying to be acceptable. For example, the authors rely on a selected handful of Pb-210 data (there are more available in the literature for better coverage (see Bröder et al., 201; Strobl et al., 1988) for sedimentation rates. The model doesn't consider the regionally diverse sediment types, permeabilities and rates in the Siberian Shelf Sea (see for example Dudarev et al., 2006 Oceanology; Rekant et al., 2015). The model doesn't consider known clay/sand/sand grain size variation and their influence of carbon concentration, permeability, transport, and resulting biogeochemical rates. âĂć The model assumes Barents Sea depositional conditions as a good analog, however, these are unlike those of the Siberian shelf, since the Barents Sea is much deeper, has higher marine productivity, less ice cover, and much less input of terrestrial organic matter. In addition, it does not have terrestrial permafrost underneath the recent Holocene sediments. It is therefore not a particularly good analog. If the authors are interested I can provide porewater methane, sulfate and ammonium data from this region. âĂć The reactive continuum approach employed here probably overestimates the reactive organic carbon amount that is available to organic carbon degradation at depth. In reality, the reactivity of the organic matter below the oxic horizons is one to two orders of magnitude lower than commonly observed in marine shelf sediments (see Figure 9, Brüchert et al., 2018). âĂć The model doesn't consider Holocene sealevel change to elaborate on the mass of sediment available for methane generation since the last glacial maximum, which is the time since reactive sedimentary organic carbon accumulation began. Given the very low reactivity of carbon in these sediments (See Brüchert al., 2018; Bröder et al., 2916; Tesi et al., 2014), sulfate is likely never exhausted and methanogenesis and AOM may not even take place in these sediments at all. I am therefore not surprised at all that the authors arrive at such low regional dissolved benthic methane fluxes, seemingly at odds with the broadly published claims of extensive methane emission from the Siberian shelf. In fact, these fluxes confirm my own direct measurements of porewater methane concentrations and methane fluxes from a range of stations investigated in the summer of 2014 during the SWERUS expedition with the Swedish icebreaker Oden. If the authors are interested, I am willing to share these data with them to better constrain their model. âĂć The model design relies on a sequence of thermodynamically regulated terminal electron acceptor reactions driven by fresh carbon accumulation at the top of the model domain. In reality, non-biogenic or old Pre-Holocene-produced methane transport from below (of thermogenic or Pleistocene age, i.e., terrestrial) is the key unique characteristic of the Siberian shelf with respect to methane cycling. This carbon is old and uncoupled to recent carbon accumulation. In addition, carbon accumulation varied greatly through time on the Siberian shelf. The model appears to assume continuity of recent depositional conditions back in time and space, which is most certainly incorrect. Only the section with the transient model scenarios therefore applies to the Siberian shelf and only scenarios with an explicit upward flux of methane are relevant for investigating AOM dynamics in these sediments. However, because of the difficulties in constraining the regional distribution of seeps, flux rates cannot be reliably extrapolated and one should refrain from a regional flux estimate. My objections to the present manuscript are therefore not whether the model's capabilities are useful to the scientific community in general, which it certainly is, but a critique of the attempt to mimic biogeochemical as well as recent and past depositional conditions on the Siberian shelf to better predict sediment methane emissions from this region. I am fully aware of the infected discussion of the relevance of the Siberian shelf sea's role as a potentially huge methane source to the atmosphere put forward by Shakhova and co-authors. The outcome of the model simulations presented here, even in their most generous state (high advective upward flow and moderately to high sedimentation rates), would imply that the emissions proposed by Shakhova and co-authors are very hard to achieve without invoking massive gas emissions (which are not seen regionally in atmospheric measurements). However, the inability of this 1D model to encapsulate environmental conditions that are found in the Laptev and East Siberian Sea make it impossible to use its scaled model output to the current system or to use the model to make reliable assessments of how the shelf environment may change methane fluxes in the future. Particularly the latter requirement is key to the use of a reaction transport model such as this one in climate science. The authors may therefore consider a new title for their manuscript for the first section and resubmit it under this new title without much reference to dissolved methane emissions on the East Siberian shelf, since this is not what they can model reasonably with the data they have available. The study and conclusions give the false impression that this particular model is capable, with certainty, to predict the non-gaseous methane flux emanating from this 1.5 million square kilometer large region, if one only knows the sedimentation rate and water depth. Alternatively, the model simulations can be tested with actual data from the Siberian shelf, which I am willing to share. In this case, I would suggest to reduce the first part of the manuscript and focus on the application of the BRNS to the Siberian shelf sea rather than a broad treatment of the model's performance.

Specific comments: See attached summary comment file.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-264/bg-2019-264-RC2-supplement.pdf

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