Review of "Subsea permafrost and associated methane hydrates: how long will they survive in the future?" by V.V.Malakhova and A.V.Eliseev, in Earth System Dynamics Discussions, esd-2021-99

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March 3, 2022

In this interesting manuscript, the authors present the development of the subsea permafrost and the methane hydrate stability zone up to 100.000 years into the future simulated by a 1D-model for points representing different points on the East Siberian Shelf. They perform scenario simulations using different climate projections in combination with different assumptions about the geothermal heat flux. The manuscript assesses the very relevant question on the stability of the Arctic subsea permafrost and its role in the climate system using a set of interesting experiments.

Abbreviations used in this review: SSPF = SubSea PermaFrost, MHSZ = Methane Hydrate Stability Zone.

Major comments

Both in the abstract and in the discussion/conclusion many numbers from the results are stated. However, I would (in both places) like to read one or two sentences on the main conclusions/the "take-home-message(s)" of the paper. My personal favourite is, that according to this study, MHSZ development is independent on the chosen climate projection, at least for several thousand years.

At several places, it is mentioned that this study (in contrast to earlier studies, e.g. Archer [2015]), the changes in the orbital parameters of the Earth are taken into account. It is however nowhere discussed which influence this has on the results.

The upscaling to pan-Arctic scale (Sec. 3.4) is — as it is also clearly stated in the manuscript — somewhat speculative due to the many assumptions needed for the upscaling. It could be considered part of the discussion instead of as "a" result. This specifically holds for the comparisions to other studies (e.g.

Wilkenskjeld et al. [2021], lines 274–285).

Also a part of the model description (line 128–133, comparing the setup to Archer [2015]) could advantageously be postponed to the discussion.

Minor comments

That the geography is in the model represented by "representative points" should be more emphasized — specifically also in the abstract.

The vertical setup (and thus type) of the model is needed in the model description. I.e. that it's a discrete 0.5 m vertical grid down to 1500 m.

Much of the model description is found both in the manuscript and the supplement. The supplement could be shortend.

Line 78: "a condition of temperature continuity". How is continuity defined on a discrete grid?

Figure 1: The general shape of the figures is intuitive, however some features seems rather pecuilar:

1. Some very steep deepening (from top)/rising (from bottom) is present, most obvious in $H_D = 10\ m, G = 45mW\,m^2$ for TR1000/TR3000. Expected is more a shape like TR3000 in $H_D = 50\ m, G = 45mW\,m^2$
2. The wave-like structure on the lower boundary, mainly visible in $H_D = 100\ m, G = 45mW\,m^2$.

Comments on these features would be appreciated.

Figure 1: I would also show the panel on $H_D = 100\ m, G = 75mW\,m^2$ even though it's empty. It would save many explanations, and the space for the panel is anyway available.

Figure 1: Consider using the same Y-axis for every subplot in a row.

Line 189: As I read the figures, MHSZ never extends above 200 m (Fig. 2) depth whereas SSPF is present near the surface at $t = 0$ (Fig. 1). This seems to contradict the sentence here.

Line 198: Which simulations are meant by "simulations with shallowing rate of $v_{\mathrm{MHSZ,b}}$"? Meaning is here not clear.

Line 216-218: Would it not be more realistic to assume that (also) SSPF prevents methane from escaping the sediments? In this way the methane puls will only escape when both MHSZ and SSPF is gone.

Line 216-218: How would the methane flux to the ocean develop without this assumption? Of course it is reasonable to argue that SSPF and MHSZ acts as a lid preventing outgasing. However, it is likely that this lid is not completely closed (due to cracks and other geological features), and thus it would provide an interesting upper-limit to the methane fluxes in the relatively near future to look at the results without this assumption.

Line 222: Should be "sediment-to-ocean" rather than "ocean-to-atmosphere"? (Since the chemical fate of the methane in the ocean water colume is nowhere quantified.)

Line 238-239: I don't understand how an order-of-magnitude difference can arrise as a consequence of a factor-6 difference in averaging length of a quantity given as a flux.

Figure 5b: The Y-scale make the results hardly readable. Better would be to let the extreme values ($G = 75\ mW\,m^2, 2-5\ kyr$ and evt. $G = 60\ mW\,m^2, 5-10\,kyr$, TR3000) go off-scale (values stated in the figure caption) and plot only $Y = 0..10\ g\,m^{-2}\,yr^{-1}$ (as in subfigure a).

The lines 274–284 are devoted to a comparison to my (and co-autor's) study (Wilkenskjeld et al. [2021]), where the authors speculate on the big differences between our results. I guess the most important reason for the differences is our use of "partially frozen cells", an approach partly inherited from the SuPerMAP model [Overduin et al., 2019] delivering our inititial conditions and partly necessary due to our rather coase resolution horizontally and in-depth also vertically. Though the initial conditions of the present and our study roughly agree on the location of tbe bottom of the SSPF, the present study likely have a much large volume of deep (below 100 m) SSPF ice (Fig. 1, see also Fig. 1b in Wilkenskjeld et al. [2021]). This ice is not affected by climate within the next 1000 years, and therefore we, by thawing the upper ice away, have be thawing a much larger fraction of the total SSPF ice, eventhough the two studies likely thaw similar amounts of ice.

Line 297-299: The numbers presented for methane captured in the MHSZ are huge compared to any to me known estimated. Also it is not very clear where these numbers come from. Is it due to the assumption that the MHSZ is completely saturated? If "yes": is this assumption realistic?

Line 316-317: As I read this sentence, it is clamed that 1.3 (or 3.4) is less than 0.4?

Line 358: "scenario of fixed temperature": Guess this means "TR0", which would be more readable.

In many cases of the bar charts (Fig. 5-7), I could imagine that the message would be clearer by using (properly smoothed) time series — eventually with non-linear time axes. This is of course a very personal opinion.

Not so much for the manuscript, but rather for my personal curriosity: Is any statement possible on the influence of salinity diffusion (which was not included in my own study)?

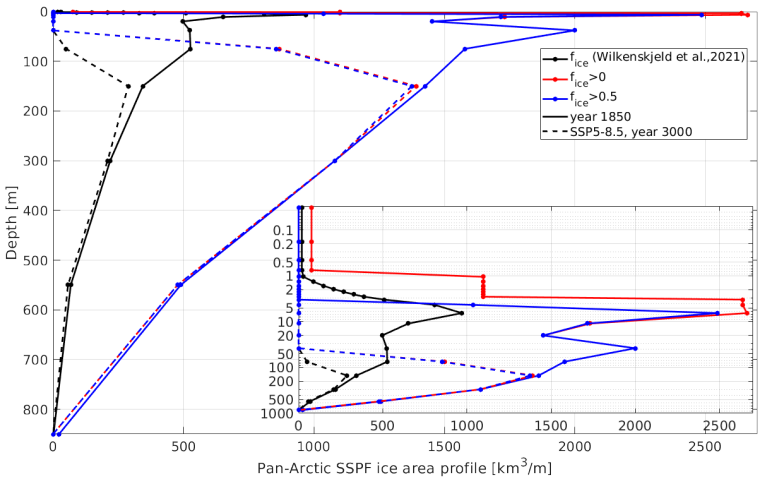

Figure 1: *Area of SSPF ice as function of depth for the initial conditions (solid lines) and at end-of-experiment for the SSP5-8.5 scenario (dashed lines) in Wilkenskjeld et al. [2021]. Different interpretations of the data are shown in different colors: Black: fractions applied (as used in Wilkenskjeld et al. [2021], red: any ice present is interpreted as cell full of ice, blue: more than 50% ice is interpreted as cell full of ice (less than 50% means no ice). The insert shows the same thing but at a logarithmic depth (Y) scale. The alternative interpretations (blue and red) of the scenario data (dashed) can of course only be interpreted as a momentary state and not as a simulation result based on the initial state, since here much more ice has disapeared than in the actual simulations. They are shown to illustrate that very little happens in the depth, also using the alternative interpretations.*

Language, presentation and technical comments

The language of the manuscript is with few exceptions fully understandable. However, the readability could be in many cases be improved, presumably by correction by an englisch native speaker.

In many cases an additional word (often conjugations of "to be") is present in a sentense. This could either be leftovers of previous versions of the sentences or some general language differences between russian and english.

Line 2: "Earth System Model" (all with initial capitals).

In section 3.3 (specifically from Eq. (2)) the term f_{CH_4} is used, later on and in the figures F_{CH_4} is used. Please choose one of the versions.

Equation 1: The factor ϕ is either there by accident or not described in the text.

Line 232: Repetition of "TRx000" unnecessary.

Line 234: Guess the meaning is "ceases to exist" (not "exit").

Line 357: "0.5 kyr centuries" seems to be a mixture of two sentence versions.

Line 376: Reference style error (wrong bracket placement).

Line 400: "sown" = "down"?

References

D. Archer. A model of the methane cycle, permafrost, and hydrology of the siberian continental margin. *Biogeosciences*, 12 (10):2953–2974, 2015. doi: 10.5194/bg-12-2953-2015. URL `https://bg.copernicus.org/articles/12/2953/2015/`.

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