

Earth Syst. Dynam. Discuss., referee comment RC2
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Comment on esd-2021-99

Paul Overduin (Referee)

Referee comment on "Subsea permafrost and associated methane hydrates: how long will they survive in the future?" by Valentina V. Malakhova and Alexey V. Eliseev, Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2021-99-RC2>, 2022

I apologize for the long delay in providing a review for this paper. When I accepted the review, I had planned to post it by the end of March.

General comments:

With this paper, Malakhova and Eliseev have made a contribution to our understanding of glacial-interglacial time scale cryospheric processes in the Arctic and push our understanding a step further, in this case by investigating the simultaneous development of permafrost and the gas hydrate stability zone (GHSZ) under conditions of future warming over a long time scale of 100 000 years, including orbital forcing, and considering the length of time required under these warming scenarios until both disappear. The strengths of the paper are its wide temporal range and the fact that it pursues the concept of the ultimate extinction of these important cryosphere components. As such this paper fits well into the scope of ESD, as it addresses scenarios of future climatic change and the interaction of climate system components. The study uses novel concepts and permits substantial conclusions on the timing of permafrost disappearance. The methods are appropriate and well described and the conclusions follow from the results.

In my opinion, the chief weakness of the paper lies in some of the assumptions made in modelling future permafrost development, specifically the direct application of projected air temperature anomalies as changes to bottom water temperatures. This is akin to removing the agency of the shelf sea water column, including ice, as a mitigator of temperature changes. As a result, the projections made in this paper (TR1000 & TR3000) are like some kind of mixture of subsea and terrestrial: initiated as subsea permafrost and GHSZ, and then forced by air temperature changes applied to the seabed. If I have perhaps misunderstood the method, then its explanation at least needs clarification. I suggest that almost any additional assumption, for example of a temperature offset (akin to a marine n-factor), would be better than directly applying air temperature changes directly to the seabed. This short-cut in the model will make both scenarios warmer than

they otherwise ought to be, but to an unknown degree, and leave me suspecting that the base case (TR0, without anomalous warming) may be closest to what we should expect in reality, and not the two other model runs.

Most studies assume that past and current bottom water temperatures are negative for most of the shelf (and esp. the depths under consideration in this paper). The air temperature anomaly that acts as a somewhat arbitrary threshold is therefore the value that somewhat takes this bottom water temperature above the freezing point of the permafrost, or above the temperature that defines the upper limit of the GHSZ zone.

To address these comments, it is necessary to show the forcing temperatures for the past and projected time period. Future forcing is shown in on its own in the Supplementary information. Please allow the reader to evaluate how the applied anomaly compares to historical values.

A second consideration that needs to be addressed is the definition of terms. What is treated as "permafrost" in the analysis of results? Does any amount of ice in the sediment result in its being classified as permafrost? Or does temperature play a role? What is meant by survival? Are permafrost and GHSZ surviving if ANY are present in the sediment column? This is perhaps interesting, but may be misleading, and MUST at any rate be explicit. Please add definitions of these terms and perhaps a discussion of the impact of the definition.

The authors do a good job of presenting the results of their analyses, either as bar graphs comparing some variable of interest (e.g. timing of extinction) or as depth-time cross-sections. It is however important that the results are presented in a way that permit comparison with the work of others. For example, to put their projections of permafrost and GHSZ into context, it would be necessary to also show past permafrost and GSHZ since 400 kyr BP for at least one scenario (e.g. H_D 50, G 60 W/m²). In particular, this would allow comparison with the seminal work of Romanovskii – do their results compare, is there a shift to more or less permafrost, a different timing of GHSZ persistence, etc. and how abrupt are the changes expected in the shift to projected values. This has direct relevance to the extent of the GHSZ in their analyses, and the possibility, as Romanovskii describes, of intra-permafrost hydrates migrating upwards following interglacial warm times with upwardly migrating lower permafrost boundaries.

Specific comments:

- Mention the "locations" that are modelled in the methods to set the reader up for what follows (i.e. H_b 10, 50, 100).
- What were initial salinity values after spin-up? Are they reasonable?

Abstract

Line 11: replace "Time instants" with "The timing"

Line 16: I am not sure what is meant by the word "instrumental" in describing the effect of warming on MHSZ loss. Do you simply mean "important" or something more specific?

1 Introduction

Line 18: you refer immediately to methane hydrates, rather than gas hydrates. I suppose that you assume methane-only in order to use existing stability relationships? Perhaps add a short discussion of how your results might be affected by a mixing of gases in hydrates?

Line 23: "so called" is a somewhat pejorative word in English, and does not work in the way that many Russian authors use it. I suggest deleting.

Line 24 & 26: the term "survived" implies something alive and is a dramatic word. I am fine with this, however, it is not clear what you mean by the "survival of PAMH". Do you mean that any hydrates still exist? Or do you mean that a hydrate stability zone still exists? This question comes up throughout the paper for hydrates and for permafrost. What do you mean by the "survival/extinction of permafrost"? No cryotic sediment? No ice?

Line 27: "are projected"

Line 29: Yang et al (2014) also wrote about riverine heat flux for North American Arctic rivers (Polar Science 8 (2014) 232-241).

Line 40: "aftermath" not used correctly

Line 42: I do not know what "This inception" refers to here.

Line 47: "isotope" is sufficient, not "isotopology"

Line 54: replace "These fluxes might become much stronger near the timing of complete local extinction of the permafrost and hydrate layers," with "These fluxes might become much stronger when permafrost and hydrate layers are completely extinguished"

Line 56: you say "below" the frozen sediment layer, but as Romanovskii et al show, intrapermafrost hydrates and gas can be expected to develop over glacial cycles, when permafrost thins through thaw from below. "within and below"

Line 57: "dissolved" is usually reserved for the incorporation of solids into a solvent. I understand that "dissolution" implies "dissolved", but this word will be confusing for most readers. What happens to hydrates when they destabilize? I do not like "degrade" (which means to lower in elevation) or "decompose" (which implies organic decay). "Destabilize" does not necessarily mean that the hydrates have disappeared, they may still exist in a metastable state. "Decay" is not a bad choice, although similar to "decompose". After considering all alternatives, I feel more accepting of "dissolve". But it will be confusing.

Line 58: improper use of "aftermath"

Line 58-59: I suggest re-formulating this sentence ("Despite the latter phenomenon...") to: "The catastrophic release may be attenuated by the transient existence of pathways through taliks that form below paleo-river channels, lakes and lagoons, especially...".

2 Model and simulations

Line 78: "For the heat diffusion equation, ..." and "is imposed"

Line 83: I hope that the magnitude of the possible effect of latent heat is discussed later in the paper!

Line 91: "marine" instead of "oceanic"

Line 96 please add "water" to "depth", otherwise it is not clear

Please state explicitly that you run 3 locations with varying HD and call them "shallow" "middle shelf" and "outer shelf" – this would make all explanations and figures much more accessible and intuitive

Line 101: Is there a difference between ΔT_{fut} and T_{fut} ? If not, please use the same variable name

Line 101: Since you are combining water temperatures with air temperature anomalies, it is important that you show these data series for the entire modelling period – are they reasonable?

Line 109-114: I understand that these are bracketing or a window of possibilities, but you should make the case that TR1000 and TR3000 are indicative of something possible, i.e. the TR0 is not the most likely scenario (see general comment above)

Line 128-135: This paragraph belongs in the Discussion, not in the Methods

Line 136: I prefer "following" rather than "via"

3 Results

3.1 Permafrost

Line 145: what is "thick" permafrost? Quantify

Line 146: When I read the second sentence, I did not understand what cases had been defined – see comment above about more explicitly defining the HD values run and giving them names; again, I still do not know what you mean by "permafrost disappears" – please define explicitly in the methods

Line 148: what is "shelf depth"? Water depth or depth in sediment? Relative to what?

Line 152: permafrost does not "melt", it "thaws"

Line 153: "independent of", not "from"

Line 156: replace "During the most part of..." with "For most of..."

Line 166: again, I stumble over "shelf depth", but I realize that what is meant is "water depth", correct?

Line 167: surely the water temperature is very important? How does it figure in? Is it directly a result of water depth?

Line 171: I do not feel that Archer (2015) obtained "similar" time scales. Please provide the numbers that you find similar, or perhaps choose different wording?

3.2 Methane hydrates stability zone

I feel that the paper would be strengthened by showing the relationship of permafrost and MHSZ distribution relative to each other, at least for the main scenario, which I think is 50/60 – the concept of intra-permafrost gas hydrates should be discussed in this context

3.3 Methane release from the sediment to the water

Line 215-225: this really belongs in the methods section; this is where I looked for it when reading the paper: how did the authors calculate fluxes?

- Why is "f" sometimes used, and sometimes "F"? is there a difference? If so, define in the methods.

Line 218: in fact, the saturation limit depends on the rate of delivery of methane to the sulphate reduction zone. Is this consistent with the use of a simple coefficient?

Line 224-5: should be "...is adapted from Ruppel and Kessler (2017), who synthesized..." and then say what they synthesized.

Line 243: It is important that you compare your fCH₄ results to available observational data. However, I cannot see any values on Figure 4 that correspond to the values that you report for Shakhova. Please state more explicitly which values of yours are comparable to the range that you quote.

3.4 Implications for the pan-Arctic

Line 247: "rudimentary", not "rudimental"

Line 251: "We assume limit..." should be replaced with "We limit..."

Line 253-4: When you refer here to subsea distribution, do you mean depth, lateral area or both?

Line 260-1: I am confused by the sentence "This anomaly is apparently different even from temperature in other model grid cells." - it seems expected that the anomaly in the East Siberian Arctic shelf would be different than in other cells?

Line 264-6: I am not sure what this -12°C reference temperature is or how it is used. For what is it a reference? I understood from the methods section that the anomaly was added to the water temperatures?

Line 280: replace "could" with "to"

Line 284: replace "devote" with "require"

Line 337: "permafrost disappears", not "permafrost is disappears"

Line 338: "a few centuries"

Line 353: "rudimentary"

Line 357: " by up to 2%"

Line 361: "depends more weakly on the applied emission"

Line 423: "lose" not "loose"

Figure 1.

- this would be more effective with the same X/Y axis limits

- it is difficult to evaluate these figures without having seen the "big picture": Please include -400 ka to 100 ka for at least 1 scenario, for example HD 50 / G 60

Figure 3.

- You model to a depth of 1500 m – it is misleading to have y-axes that extend beyond this depth, and it appears that the base of permafrost was exactly 1500 m.
- this figure would also work better if all y-axes were the same. At the moment, it makes the impression of equally thick permafrost under all scenarios.