The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-112-RC1, 2019

The Cryosphere Discussions
Open Access
EGU

Interactive comment on "The Current State and 125 Kyr History of Permafrost in the Kara Sea Shelf: Modeling Constraints" *by* Anatoliy Gavrilov et al.

Anonymous Referee #1

Received and published: 10 December 2019

Review of The Current State and 125âĂĽKyr History of Permafrost in the Kara Sea Shelf: Modeling Constraints by Anatoliy Gavrilov, Vladimir Pavlov, Alexandr Fridenberg, Mikhail Boldyrev, Vanda Khilimonyuk, Elena Pizhankova, Sergey Buldovich, Natalia Kosevich, Ali Alyautdinov, Mariia Ogienko, Alexander Roslyakov, Maria Cherbunina, and Evgeniy Ospennikov for The Cryosphere

1. Does the paper address relevant scientific questions within the scope of TC? Yes, the focus of the paper is offshore permafrost, an important and understudied component of the cryosphere. The central thrust of the paper is to increase our knowledge of the distribution of permafrost below the Kara Sea using the state of the art knowledge

Printer-friendly version

Discussion paper

of regional geology, and ice sheet and sea level histories.

2. Does the paper present novel concepts, ideas, tools, or data? Yes, the study uses a novel mix of physical numerical modelling with specific regional knowledge of processes that influence permafrost.

3. Are substantial conclusions reached? Yes, a new map of the Kara Sea region subsea permafrost is generated. The methods used are somewhat novel in terms of how geological information is combined with palaeohistorical reconstructions. The resulting permafrost distribution is substantial as a contribution to the relatively limited body of literature on Kara Sea permafrost and has implications for the distribution of subsea permafrost elsewhere in the Arctic.

4. Are the scientific methods and assumptions valid and clearly outlined? More detail on the numerical methods used to obtain the presented results needs to be provided.

5. Are the results sufficient to support the interpretations and conclusions? Yes.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? No, as stated above, the model needs better description. Many journals are moving towards requiring provision of the numerical model used to obtain results; it is to be expected that these need to be made accessible in the future. This is not a requirement for publication in The Cryosphere. Nonetheless, the description of the physics behind the model should be explicit enough to allow reproduction.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes.

8. Does the title clearly reflect the contents of the paper? Yes.

9. Does the abstract provide a concise and complete summary? Concise: yes; complete: no. See comment below.

10. Is the overall presentation well structured and clear? Yes.

11. Is the language fluent and precise? The language is fluent but needs improvement in precision and word choice.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? No.

14. Are the number and quality of references appropriate? See comment below.

15. Is the amount and quality of supplementary material appropriate? Yes.

This paper is a timely and well-thought out contribution using a modelling approach to estimate subsea permafrost distribution in the Kara Sea. The authors bring their substantial knowledge of the literature, especially Russian, and their expertise in the palaeohistory and geology of the region to improve the results of their numerical modelling by imposing a hierarchy of geographic regions. This approach and the detailed background work is a significant contribution to the body of knowledge on the topic and will be a paper to which future work in the region will be referred. For these reasons, I recommend that it be published.

I do have one major criticism and a host of suggestions to improve the paper and make it accessible to a wider readership. Although these suggestions may sound overly negative, please accept them in the spirit in which they are made: in the hopes that some of them may lead to improvement of the paper.

General comments: 1. My main criticism of the paper is that the model used has not been adequately explained. It would not be possible for someone using the same or a different model to reproduce the work without a great deal of extra information. In my specific comments, I try to identify and ask questions at points in the text where vague language is used, or where important details are omitted. To allow comparison of their

Printer-friendly version

Discussion paper

work, I encourage the authors to be as explicit and detailed as possible about how they achieved their results. Examples of open questions regarding how the model works are: - Line70: what is meant by “double-layer” explicit solution? Is the Qfrost software publically available (e.g. on GitHub) – if so, why not reference it? If the model is well-documented, this might be a sufficient means of answering many of the questions regarding the method. - Line 80: “extrapolated” – what method was used to extrapolate results from monolithic stratigraphies to more complex stratigraphies? Introducing changes in porosity, grain size, thermal properties, etc. would presumably change the temperature field solutions? - On line 82, all rocks (please replace) were assumed to be saline – why? how saline was the sediment assumed to be? Did salinities vary with depth? Was salt diffusion permitted? How did salt content affect the freezing characteristic curve or liquid water of frozen material? - How are discontinuities avoided at the borders between domains/subdomains/areas/subareas?

As a result, claims are made in the paper, but there is not enough information given to the reader to be able to judge whether the claim is justified or on what basis it has been made. For example: - section 4.2 lists 8 controls on the “pattern of permafrost distribution”, but 2 of the 8 (lithology and properties or rocks, and Holocene climate optimum) are not described in any detail, making it impossible for the reader to follow the argument or design studies that reproduce the work. A 3rd control (thermal effect of river waters) is not even modelled, so it is not clear how the authors can conclude that this acts as a control. It seems to be an assumption in the model design, but not enough information is provided for the reader to be able to judge. - Tables 2-7 lists the model output for “depths to permafrost top” – but what is merant by “permafrost top” has not been defined anywhere. Does this correspond to an isotherm, the presence of any ice, or of some minimum amount of ice? Or does the model output the depth of the phase change boundary?

And finally, also related to a better description of the model: The discussions and conclusions would be strengthened if the authors included a section that takes a critical

Printer-friendly version

Discussion paper

look at how their choice of model, its parameterization and its sensitivity to parameter choice affected the outcome of their study. This is valuable on two levels: it provides a measure for how robust the conclusions of the study are, and it provides a basis for evaluating the sensitivity of the Kara Sea system to changing boundary conditions, allowing some indication of the system's sensitivity to future changes.

3. The language used in the paper is sometimes imprecise or even incorrect; the paper should be proof-read by an native-speaker with some background in the topic. As examples: a. "cold": is probably being used to refer to cryotic conditions, or to conditions below the freezing point. As it stands, it is a vague descriptor. b. "rock": is used to refer to earth material, including either rock or sediment, consolidated or unconsolidated material. In English, "rock" is used to refer only to bedrock material, and would exclude sedimentary deposits of terrestrial, marine or other origin. As it stands, all instances of the use of "rock" need to be replaced with something more precise. c. More examples are given below in the specific comments.

4. The abstract is extremely short and does not provide enough information for a reader to decide whether he/she wants to read the paper. It needs to introduce the larger context for the study, the central question/focus/hypothesis, more detail on the method. It should report key results, findings and conclusions, and may suggest implications or outlook based on the study.

5. The reference list is incomplete. Vasiliev & Rekant (2018) are missing, for example. The reference list needs to be cross-checked with the submitted paper. Some reference citations still include initials (see Fig. 4 caption, for example).

6. This paper stays true to the general phenonmenon of Russian authors citing mostly Russian work, and Canadian/Alaskan researchers citing mostly North American. For citations dealing with regionally specific processes, this is understandable. But neglecting to look at how the North American community has approached modelling the exact same processes under different conditions is harmful in two ways: it exposes the

Printer-friendly version

Discussion paper

work to the criticism of being too narrow in its approach, and it makes it less likely that the work will be found and cited by North Americans. I encourage the authors to show their familiarity with the field by referring to the work of researchers from outside of their region, who have presented novel ideas in the field of subsea permafrost modelling. Some examples: - Whitehouse, P. L., Allen, M. B., Milne, G. A. Glacial isostatic adjustment as a control on coastal processes: an example from the Siberian Arctic, Geology, 35, 747–750, doi:10.1130/G23437A.1, 2007. - anything from the group of Romanovsky and Nicholsky (e.g. Nicolsky, D., Shakhova, N. Modeling sub-sea permafrost in the East Siberian Arctic Shelf: the Dmitry Laptev Strait. Environmental Research Letters, 5(1), 15006, 2010.). - anything from Taylor, A. (e.g. Taylor, A. E., S. R. Dallimore, P. R. Hill, D. R. Issler, S. Blasco, Wright, F. Numerical model of the geothermal regime on the Beaufort Shelf, Arctic Canada since the Last Interglacial, J. Geophys. Res. Earth Surf. , 118, doi:10.1002/2013JF002859, 2013.).

Specific comments: Line 13: the use of Kyr as a unit does not follow SI. Line 24: “In the latest . . . earliest . . .” needs correction. Line 45: “raised high” – please quantify Line 49: add “and” and remove “and so on” Line 58: replace “provide their progress” with “extend their work”? Line 59: what is meant with “geocryological results”? Please specify. Line 61: “obtained estimates” – of what? Please specify.

Fig. 1. This figure provides an overview of the method, but uses many general or non-specific terms that reduce the amount of information communicated: - in the top box, what is meant by “environmental data”? - in the second box, what is meant by “conditions”? - in the left third box, delete “dynamics” (adds nothing to “history”) - in the third right box & in the fourth left box, replace “rocks” - in the fourth right box: “density of the heat flow from the depths” is usually referred to “geothermal heat flux” - in the fifth box: “Testing . . . of the model” is almost entirely free of content. How was which model tested? More specific word choice could make this box informative - the lowest box is actually two steps: “coordination” and “mapping” - what is meant by “coordination”? This question is never really answered in the paper, but is critical for understanding

Printer-friendly version

Discussion paper

what was done. Does the model output get changed in some way by comparison with field data? Where and where not? How? These are important points for anyone wanting to reproduce or apply the method in the same or in other geographical regions.

Line 72: "Permafrost dynamics were. . ." Line 73: "including. . ." suggests that other scenarios/conditions were NOT included? How many and why not? Line 77: how were regions of different geothermal heat flux mapped or determined? Line 79 and in all following text: modelling was probably not restricted to the "rock". Line 87: "subsea permafrost had presumably fully degraded. . ." – this statement requires a reference, especially in light of modelling, for example by Romanovsky, N. N.), showing permafrost elsewhere persisting through interglacials; this point is important, since other researchers have shown that a systematic bias in model results is obtained depending on the initial conditions. Such results show that setting permafrost to zero at the interglacial will introduce a warm bias, that at least would need to be tested.

Fig. 2: it looks like only 14 sites out of more than 100 are located on the shelf, i.e. pertain to subsea permafrost. Is this correct? Please add a description of the red line (which is currently not described until Fig. 8).

Line 106: "the existence of a number of idea about its development. . ." is not a peculiarity of any region, it is true of every region! Line 115: dammed lakes are invoked to explain the unfrozen zone. Why is the sensible and convective heat transport at the river bed and in the estuarine regions not sufficient to explain the absence of frozen material? Surely the rivers maintain and have maintained positive benthic temperatures for long periods? Line 119: "Insignificance of the severity" is convoluted language that should be simplified. Line 154: explain the abbreviation "MMP" Line 156: "sea level" rather than "sealevel" Line 201: on what basis was it decided how long each portion of the shelf spent in the coastal zone (400-2000 years)? Why were waters in this zone saline? – is this not the zone most affected by the freshwater layer above the halocline, by snow melt and river runoff? Dmitrenko et al (2011) show the freshwater nature of the coastal zone in the Laptev Sea. And why was this zone warmer? Bedfast ice can

Printer-friendly version

Discussion paper

result in cooling of the seabed from 0 – 2 m water depth. A little more justification and specification of these boundary conditions, which determine the most immediate and rapid response of permafrost to inundation by seawater, are necessary.

Fig. 8: The map shows permafrost thickness, which can clearly result as output from 1D numerical modelling. What conditions were applied to determine zonation of permafrost based on continuity (continuous, discontinuous, sporadic)? I.e. how do conclusions about distribution result from 1D modelling? Caption: why are only "fragments" of the map shown? Why not present the reader with the whole map?

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-112, 2019.

Printer-friendly version

Discussion paper