Climate
of the Past
Discussions

Open Access

CPD

**Interactive
comment**

Interactive comment on "How sensitive are modeled contemporary subsea permafrost thaw and thickness of the methane clathrates stability zone in Eurasian Arctic to assumptions on Pleistocene glacial cycles?" *by* Valentina V. Malakhova and Alexey V. Eliseev

Valentina V. Malakhova and Alexey V. Eliseev

eliseev@ifaran.ru

We are grateful for the reviewer for constructive comments which led to the improved presentation of our results.

The most important changes in the manuscript are as follows:

- The paper title is changed to 'The stability of contemporary subsea permafrost and associated methane hydrates to Pleistocene glacial cycles'. This is done in

order to shorten the title and emphasise that we study the hydrates which are formed during the Pleistocene cold epochs.

- We revised the abstract to the paper as well as sections *Introduction* and *Conclusions* to highlight our motivation to undertake this study. In particular, we show that, while it is widely acknowledged that the response of the shelf sediments to imposed oceanic warming is a slow process, the time scale of such response is not yet quantified. Some references are added with respect to this discussion. We show that this time scale is of the order of 10-20 kyr for the deep present–day shelf, which is as twice as large in comparison to similar estimates obtained by Romanovskii et al. (2005).

- To highlight our results for the time scales of the response of the sediment thermal state to temperature changes at the ocean–sediment interface, we extended our paper by new Fig. 2, which shows the lag of the HSZ thickness D_{HSZ} with respect toÂǎT_{B}, and by the paragraph devoted to the discussion of this Figure. Previous Fig. 2 is now referred to as Fig. 3.

- In response to the comment made by the second reviewer, we extended our paper by supplementary Fig. S2, which shows the results of additional simulations in which impacts of the pressure changes due sea level change are neglected.

- The language is checked and ameliorated.

- In addition, we discovered and corrected a technical error for our output for $z_{\mathrm{HSZ,t}}$ (Figs. 1, 3, and S1). This error does not affect the major outcome of our manuscript.

Below, the point-to-point replies to the comments are listed. Original reviewer's comments are typed in italic.

General comments

The paper is poorly written, the logic is hard to follow. The paper fails to present the science-driven issue – what is currently understood and why is this particular study advancing the field. What is expected of the study and how does it achieve its ends. Clearly this is associated with the initial conditions of anthropogenic climate change and the rate of future emissions such as described in the papers below. All these papers make use of a sediment model for the HSZ to the model itself is not unique. The authors do recognise this and so do not use much text to describe it. Even through it may not be the best model available, it is adequate for the task to which it is applied – namely a sensitivity study. Kretschmer, K., A. Biastoch, L. Rüpke, and E. Burwicz (2015), Modeling the fate of methane hydrates under global warming. Global Biogeochem. Cycles, 29, 610–625. doi: 10.1002/2014GB005011. Marín-Moreno, H., T. A. Minshull, G. K. Westbrook, B. Sinha, and S. Sarkar (2013), The response of methane hydrate beneath the seabed offshore Svalbard to ocean warming during the next three centuries, Geophys. Res. Lett., 40, 5159–5163, doi:10.1002/grl.50985. The paper is rather to qualitative in that it does not attempt to quantify the uncertainties associated with the timing of the ocean inundations. It could perhaps use a metric of the volume of the HSZ as a function of the phase of the inundation. The writing of the paper still retains Russian phraseology with missing English definite article ('the') and indefinite articles ('a' and 'an'). I have suggested some changes for the introduction below, but the issue of confused logic and linguistics is common throughout, and would require major effort to provide corrections.

We are grateful to the reviewer for this very stimulating comment. It guided us how to express our motivation more clearly and how to emphasise the novelty of our study. In response to this general comment, we revised our manuscript as follows:

- In the abstract, as well as in *Introduction* we highlight our motivation to undertake this study. In particular, we show that, while it is widely acknowledged that the

response of the shelf sediments to imposed oceanic warming is a slow process, the time scale of such response is not yet quantified. In most previous papers the length of the performed simulations is up to few millennia which is not sufficient for such quantification (some references are added with respect to this discussion). The only exception, which we aware of, is the paper (Romanovskii et al., 2005) who also performed the simulations covering the whole glacial cycle. They obtained the time scale of the response of the subsea permafrost and of the subsea hydrates developed in this permafrost, which is of the order of 5–10 kyr.

• In sect. 2, we show that this time scale is of the order of 10–20 kyr for the deep present-day shelf, which is as twice as large in comparison to similar estimates obtained by Romanovskii et al. (2005). The likely reason of the latter difference are site–specific, non–monotonic profiles of the sediments thermophysical properties employed in the Romanovskii et al.'s paper. This, in principle, may diminish the generality of the conclusions of that paper.

• In *Introduction* and in *Conclusions* we stress that we do not attempt to estimate release of methane from hydrates which dissociate due to heat propagation into the sediment column from above during interglacials. A biogeochemical model explicitly simulating methane geochemistry in the sediments and formation and dissociation of methane hydrates should be used to pursue this goal. Such models need detailed (vertically resolved and, thus, tied to particular geographic locations) input sets though. The latter precludes to perform idealised, easier to interpret study as that presented here. Moreover, this modelling has its own uncertainties, e.g. due to the model structure, which are beyond the scope of the present manuscript. In particular, some of these models ignore hydrates in the subsea permafrost formed during Pleistocene glaciations. A simplified approach for assessment of hydrate methane volume could be an implementation of the transfer functions for hydrate stock as suggested by the reviewer. However, this approach is not pursued in our paper, because our goal is to study the hydrates

developed during the Pleistocene glacial cycles, while such transfer functions ignore these hydrates.

- In the abstract, in *Introduction* and in *Conclusions*, we highlight that our study is focussed on the hydrates in the subsea permafrost. These hydrates are distinct from those occurring, e.g., near Svalbard, where permafrost is not necessary for formation of hydrates. As a result, our results can not be directly compared with those reported by Marín–Moreno et al. (2013, 2015).

Detailed comments

- *This title need to be shortened. Perhaps - "The stability of contemporary Eurasian Arctic methane hydrates to Pleistocene glacial cycles".*
  The title is changed to 'The stability of contemporary subsea permafrost and associated methane hydrates to Pleistocene glacial cycles'.

- *NOTE: you use 'hydrates' in the abstract and HSZ throughout so the title should use the same nomenclature*
  Upon revision, all instances of word 'clathrates' are replaced by 'hydrates'. The only instance, where word 'clathrates' is kept in the paper is in the first sentence of the Introduction, in which, however, it is used only to explain the term 'hydrates'.

- *Page 1. Line 1-8. The abstract needs to start with a sentence of context. What is the scientific question being addressed and why is this important? i.e. "Why should I read this paper?*
  The abstract is extended by sentences discussing our motivation to undertake the study.

- *Line 1-2. Single–point simulations with a model , describing the thermal state of subsea sediments driven by the forcing constructed from ice core data, show that*

the impact of the initial sediment conditions is lost after 100 kyr.
The sentence is revised.

- *P. 1, LL. 3-5. The timings of continental shelf exposure during oceanic regressions, and flooding during transgressions, are important for the representation of the sediment thermal state and hydrate stability zone (HSZ).*
  The phrase is ameliorated.

- *P. 1, L. 11. Replace 'in last' with 'over the last'*
  The sentence is changed accordingly.

- *P. 1, L. 12. Replace "is not known with a sufficient certainty" with "is not well known" or otherwise you need to quantify "sufficient certainty"*
  The phrase is ameliorated.

- *P. 1, L. 13. Rather than addressing a "controversy" it might be better if the paper were presented as "reducing uncertainty", rather than about taking sides in a controversy. I thus suggest replacing "there are controversial claims about origins" with "there is uncertainty about the origins"*
  Words 'controversy' and 'controversial' is replaced by 'uncertainty' in this and other instances in the text.

- *P. 1, L. 17. I suggest "The uncertainty in the driver of methane release has important implication for the release rate from hydrates over the coming centuries (O'Connor et al., 2010)"*
  The phrase is revised according he reviewer's suggestion.

- *P. 1, L. 19. I suggest "Understanding may be improved, in principle, by means of . . ."*
  The sentence is revised.

- *P. 1, L. 20 to P. 2, L. 2. I suggest "Such modelling contains its own uncertainties associated with its parameterisations (e.g. . . ..) as explored by Eliseev et al (2015), and initial state originating from sea level changes during glacial cycles."* According to the reviewer's suggestion, these phrases are merged together.

- *P. 2, L. 3. Replace "assumptions" with "approximations"* The word 'assumption' us replaced by word 'approximation'.

- *P. 2, L. 4. 'Obviously' is redundant, start with "The timing of the regressions and transgressions depends on the contemporary shelf depth"* The sentence is shortened.

- *P. 2, LL. 7-11. Poorly posed. I suggest "Previous studies either assume instantaneous exposition and flooding over the entire shelf (Portnov et al., 2014; Razumov et al., 2014), or that the permafrost, formed during the last glaciations, persists up to the present (Denisov et al., 2011; Anisimov et al., 2012).* These phrases are ameliorated.

- *P. 2, L. 12. Suggest "The impact of such approximations on the presence of the hydrate stability zone (HSZ), the region of the subsea sediments in which hydrates are thermodynamically stable, regardless of their presence or not, is still unexplored. With this purpose we undertake a series of one- dimensional simulations using a model describing the thermal state of subsea sediments."* The statements are revised according to the reviewer's suggestion.