Reply to the reviewer's comments to Subsea permafrost and associated methane hydrates: how long will they survive in the future?

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

The most important changes in the manuscript are as follows:

- Supplementary information is extended by figures showing
  - profiles of temperature and salinity at $t = 0$;
  - T_B before $t = 0$;
  - results of the ACCESS ESM-1.5 SSP5-8.5 simulation for seafloor temperature in support of our choice for future scenarios of climate change;
  - permafrost layer and MHSZ simulation from 400 kyr B.P. to 0 kyr B.P.
- We dropped out the assumption that MHSZ is an impermeable layer for CH_4 transport. As it was expected, this resulted in a larger methane flux at the sediment-ocean interface during the gradual MHSZ degradation and eliminated the pulse release of methane at the end of this process. However, we still discuss the respective results from the previous simulations (because it is a potentially interesting sensitivity study) The former Fig. 5 is moved into the Supplement.
- Some figures are redrawn and restructured. In particular, the former Figs. 2 and 4 and combined into a single Figure (now referred to as Fig. 2). This is done to make it easier to compare the time of disappearance for permafrost and for MHSZ. Other figures are renumbered accordingly.
- Upon revising our paper, we found an error in our calculations: flux f_{CH_4} *was not* multiplied by K_S except that belonging to the pan-Arctic estimates. Now, this error is corrected.

- We found a logical inconsistency in our notations. In particular, the present-day shelf depth (thickness of water layer above the sea floor) was denoted as H_{D}, while temperature at the sediment water interface was referred to as T_{B}. Now, H_{D} is replaced by H_{B} to highlight that they point to the characteristics at the same physical surface.

Below, the point-to-point replies to the comments are presented. The original comments are typed in italics, and the replies are typed in regular font.

General 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.*
  Yes, we agree that abstract should be shortened. Upon revision, some numbers are removed, and some statements are revised. In particular, it is stated that MHSZ dynamics is independent on the chosen climate projection, at least for the next 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 most important impact of the future orbital forcing is a non-monotonic change of T_{B}. It impact on simulations is more important for the permafrost than for MHSZ. In particular, it leads to retardation of the permafrost table thaw rate in TR1000 and TR3000. However, its effect is not a dominant one, because such thaw (albeit with a much reduced rate) is exhibited in simulation TR0 as well.

- *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 comparisons to other studies (e.g. Wilkenskjeld et al. [2021], lines 274–285).*
  We agree that this upscaling is rather speculative. Thus, we moved this material to the 'Conclusions and Discussion' section. The latter is now subdivided into three subsections to simplify reading.

- *Also a part of the model description (line 128–133, comparing the setup to Archer [2015]) could advantageously be postponed to the discussion.*
  This paragraph is moved to Sect. 2.

Specific comments

- *That the geography is in the model represented by "representative points" should be more emphasized – specifically also in the abstract.*
  This clarification is added to Sect. 2 of the manuscript.

- *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.*
  An information on the vertical grid is added to Sect. 2 of the main text.

- *Much of the model description is found both in the manuscript and the supplement. The supplement could be shortened.*
  We prefer to keep the model description in the Supplementary Information as detailed as possible. Otherwise, it would be quite tedious for a reader to merge different pieces of information from the main text and from the Supplement.

- *Line 78: "a condition of temperature continuity". How is continuity defined on a discrete grid?*
  We agree that the wording is awkward. Temperature continuity is a step in derivation of the Stefan condition. Namely, it is assumed that temperature is the same just above and just below thaw freezing/thaw interface. Thus, the sentence on temperature continuity is removed from the manuscript, and the only note on Stefan condition is kept in the text.

- *Figure 1: The general shape of the figures is intuitive, however some features seems rather peculiar:*
  1. Some very steep deepening (from top)/rising (from bottom) is present, most obvious in $H_D = 10$ m, $G = 45$ $mW\ m^{-2}$ for TR1000/TR3000. Expected is more a shape like TR3000 in $H_D = 50$ m, $G = 45$ $mW\ m^{-2}$.
  2. The wave-like structure on the lower boundary, mainly visible in $H_D = 100$ m, $G = 45$ $mW\ m^{-2}$.
  Comments on these features would be appreciated.
  Yes, thank you for pointing this out. The comments are as follows:
  1. Fast (but at the multi-millennium timescale) thaw from the top is a continuation of the thaw induced by the last glacial termination. The bottom thaw rate is always between 10 and 20 $\mathrm{m\,kyr^{-1}}$ in our simulations. Fast thaw from above is exhibited only in simulations which are forced by anthropogenic emissions. These emissions result in increase of the permafrost table thaw rate from $\approx 1.5\ \mathrm{m\,kyr^{-1}}$ (TR0) to 13 $\mathrm{m\,kyr^{-1}}$ (TR3000). Both conclusios are in the text already.
  2. Wavy structure is a combination of two phenomena. The first one is an impact of isothermic thaw of pore ice leading to stop of the thaw front movement when heat is accumulated, and a renewal of such movement when the accumulated heat is enough to melt the pore ice at a discrete vertical grid. The second one (playing the more important role at the bottom of the permafrost) is a coarse-scale output of our model – we store

the data once per 100 yr. Taking into account the mentioned-above thaw rates, such coarse-scale output results in the movement of the thaw interface by less than one grid step during a single time step, thus, leading to the 'jumps' when such interface suddenly changes position between the nearby grid cell. Upon revision, boundaries in Fig. 1 are smoothed with a window length of 1 kyr to remove the mentioned-above 'jumps'. The respective note is added to the caption of the Figure.

- *Figure 1: I would also show the panel on $H_D = 100$ m, $G = 75$ $mW\ m^{-2}$ even though it's empty. It would save many explanations, and the space for the panel is anyway available.*
  When this figure is redrawn with the subplot-independent Y-ranges (see the next comment; the same comment was due to the another reviewer as well), panels for $H_{\mathrm{B}} = 100$ m look very non-informative. Therefore we chose to remove these panels from figure entirely.

- *Figure 1: Consider using the same Y-axis for every subplot in a row.*
  Upon revision, this figure is redrawn with Y-range from 0 to 1500 m.

- *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.*
  Yes, we agree. 'Smaller' is replaced by 'larger'.

- *Line 198: Which simulations are meant by "simulations with shallowing rate of $v_{\mathrm{MHSZ,b}}$"? Meaning is here not clear.*
  It was a misprint. A correct sentence reads, 'In the simulations with other values of G, the rate of $v_{\mathrm{MHSZ,b}}$ averaged over 5 kyr A.P. $\leq t \leq$ 10 kyr A.P. is close to 100 m kyr^{-1} for all three emission scenarios'.

- *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 pulse will only escape when both MHSZ and SSPF is gone.*
  This is potentially interesting, but it would increase the volume of our paper dramatically. We would plan to do this in future.

- *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.*
  Yes, we agree. In our new calculations, MHSZ is permeable for a methane transport. This removed the pulse release of methane at the time of the complete extinction of MHSZ and increased methane fluxes during gradual degradation of MHSZ. Old calculations are moved to the Discussion section, and the former Fig. 5 is moved to the Supplement. Supplement. In addition, our estimate for methane fluxes was constructed assuming an

instantaneous transport of methane from MSHZ to the sea floor (Sect. 2). In reality, this transport is controlled by diffusion and vertical advection. Both processes result in a finite timescale for such transport (Xu, Ruppel, 1999). Therefore, it is likely that our assumption of an instantaneous transport of methane leads to the overestimated corresponding flux at the sediment-ocean interface. The respective paragraph is added to Sect. 4.3.

- *Line 222: Should be "sediment-to-ocean" rather than "ocean-to-atmosphere"? (Since the chemical fate of the methane in the ocean water column is nowhere quantified.)*
  Yes, thanks. The sentence is corrected.

- *Line 238-239: I don't understand how an order-of-magnitude difference can arise as a consequence of a factor-6 difference in averaging length of a quantity given as a flux.*
  We meant that different averaging intervals may either include or not include the pulse release at the timing of the MHSZ disappearance. However, we agree that this sentence is unclear and might be misleading. Moreover, pulse release is not exhibited in our new set up. It is excluded upon revision.

- *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 $Y = 0 \ldots 10\ g\ m^{-2}\ yr^{-1}$ (as in subfigure a).*
  Because the assumption of MHSZ impermeability is dropped in the revised manuscript, there is no need to this construction of the figure. The revised figure (which is Fig. 4 now owing to merging previous Figs. 2 and 4 into a single figure) is drawn in a more spectacular way.

- *The lines 274–284 are devoted to a comparison to my (and co-author'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 initial conditions and partly necessary due to our rather coarse resolution horizontally and in-depth also vertically. Though the initial conditions of the present and our study roughly agree on the location of the 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, even though the two studies likely thaw similar amounts of ice.*
  We are very grateful for this insightful comment. The respective note is added to Sect. 4.2.

- *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?*
  We do not assume that hydrates are completely saturated. It is stated in the paragraph right after Eq. (1) that our assumed saturation is $\theta_{\mathrm{CH}_4} = 0.05$. However, we acknowledge that our value (1230 $\mathrm{PgCH_4}$) is an overestimate. The likely reasons for obtaining such value are i) the assumption that hydrates exist everywhere in the MHSZ, while Xu and Ruppel (1999) and Mestdagh et al. (2017) pointed out the hydrates are mostly absent in the uppermost part of MHSZ, and they do exist in the lowermost part only provided that $\mathrm{CH_4}$ flux from below is large enough, ii) possible unfrozen (and, thus, unable to support the thermodynamic conditions for hydrate formation) horizontal subgrid-scale regions. Both assumptions might lead to the several-fold overestimated methane stock, and in combination they might lead to the corresponding overestimate by order of magnitude. The respective discussion is added to Sect. 4.2.
  Nonetheless, quite a similar value (1400 $\mathrm{PgCH_4}$) was reported by James et al. (2014) as based on Shakhova et al. (2010). We this estimate to new Fig. 7 for clarity.
  In addition, it is important that even our (likely overestimated) methane stock in the Arctic shelf sediments is unable to support large methane fluxes which are reported, for instance, by Shakhova et al. (2010). The respective note is added to the subsection on pan-Arctic estimates.

- *Line 316-317: As I read this sentence, it is claimed that 1.3 (or 3.4) is less than 0.4?*
  Sorry for this misprint. It should be 'larger' rather than 'smaller'.

- *Line 358: "scenario of fixed temperature": Guess this means "TR0", which would be more readable.*
  Upon revision, the entire paragraph is removed from the manuscript.

- *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.*
  We tried this option many times during manuscript preparation and revision. This was always less readable than our bar charts, especially for the permafrost and MHSZ (which dynamics are the major goal of our paper) because of the necessity to put 9 (3 cases for G and 3 cases for H_{B}) on the same plot.

- *Not so much for the manuscript, but rather for my personal curiosity: Is any statement possible on the influence of salinity diffusion (which was not included in my own study)?*
  In our previous manuscript (Malakhova and Eliseev, 2020b) it was found that the impact of salinity diffusion on the permafrost-associated methane

hydrates is not marked due to deep level of their occurrence in the shelf sediments. While this is not a strong conclusion, we prefer not to go deeper in this matter at the date, because to arrive at a firm conclusion require to set up specific simulations.

Language, presentation and technical comments

- *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.*
  The language is checked and ameliorated.

- *Line 2: "Earth System Model" (all with initial capitals).*
  The sentence is revised accordingly.

- *In section 3.3 (specifically from Eq. (2)) the term $f_{\mathrm{CH_4}}$ is used, later on and in the figures $F_{\mathrm{CH_4}}$ is used. Please choose one of the versions.*
  Upon revision, we clarified our terms. We use f for fluxes per unit area (mass per unit area per unit time) and F for the area-summed fluxes (mass per unit time). We agree that these letters were used in a somewhat confusing way in our previous manuscript version. Now this is ameliorated. In addition, a note is added on the difference between f and F as well as on the difference between m and M.

- *Equation 1: The factor ϕ is either there by accident or not described in the text.*
  This is porosity. It is defined earlier, in a brief description of SMILES.

- *Line 232: Repetition of "TRx000" unnecessary.*
  Now this repetition is replaced 'with external CO_2 emissions'.

- *Line 234: Guess the meaning is "ceases to exist" (not "exit").*
  The misprint is corrected.

- *Line 357: "0.5 kyr centuries" seems to be a mixture of two sentence versions.*
  Upon revision, this sentence is removed from the paper.

- *Line 376: Reference style error (wrong bracket placement).*
  The sentence is revised accordingly.

- *Line 400: "sown" = "down"?*
  The misprint is corrected.