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The Cryosphere Discussions

Interactive comment on "Diverging responses of high-latitude CO_2 and CH_4 emissions in idealized climate change scenarios" *by* Philipp de Vrese et al.

Philipp de Vrese et al.

philipp.de-vrese@mpimet.mpg.de

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Response to comments of reviewer 1

September 25, 2020

Please note that, in the following point by point address, we repeat the reviewer's comments in red letters while our response is given in black letters.

General

As a whole, the paper is well written. The topic is timely in light of the ongoing attempts to understand and quantify the multi-centennial climate response to the continuing anthropogenic greenhouse gas emissions and their decline in future.Thus, I vote for publishing this manuscript subject to addressing the following comments.

Major Comments

The major comment to the paper is due to the lack of studying the regional pattern of the hysteresis–like phenomenon in the manuscript. Eliseev et al. (2014) found that the hysteretic response of permafrost extent is due to strong difference in thermophysical properties between the mineral soil and peat. I expect that this issue could be applicable to this manuscript as well.

In addition, there is a subtlety in term 'hysteresis'. In physics, this term is reserved for the response of a multi–stable system to change of an externally imposed governing parameter. This is different from the phenomenon studied in the present paper. Here,the hysteresis–like response is due to transient properties of the system under investigation – basically, because of difference in response time scales between different compartments (e.g., due to different thermal inertia between peat and mineral soil in (Eliseev et al., 2014)). This is highlighted by the fact that both variables forming the hysteresis curve (e.g., in Figs. 3-6 of the manuscript) are internal variables of the system. As a result, term 'transient hysteresis' was introduced by Eliseev et al. (2014).I suggest to discuss this issue in the paper under review as well.

We apologize for not giving the hysteresis-like behaviour the attention it deserved. However, we did conduct an extensive investigation into this feature, including several additional long-term simulations. Unfortunately, we did not see a way to adequately present the respective findings without substantially increasing the length of the manuscript. Thus, we decided to discuss the hysteretic behaviour and its spatial pattern in a separate study. Nonetheless, we are happy to provide a short summary addressing Prof. Eliseev's comments: We found three main factors that determine the dynamics; Most importantly, the hysteretic behaviour is – as Eliseev (2014) proposed – due to the large inertia of permafrost-affected soils. Here, the response time depends largely on the energy required for or released in the phase change of water, hence the signal indeed has a high spatial variability, with larger delays in regions with high soil water contents. Secondly, the timescales on which vegetation shifts occur and soil organic matter decomposes are much larger than the timescales of the imposed climate change. Thus the simulated vegetation and soil carbon pools simply lag behind the warming/cooling signal and the rise/decrease in CO2. The third factor is the change in the soil organic matter concentration that alters the hydrological/thermophysical soil properties. And even though the latter may initially not be the strongest of the factors it is highly important because it alters the boundary conditions under which physical and biophyiscal soil processes take place. Thus, the difference in soil carbon pools before and after the temperature peak has the potential to lead to an actual hysteresis – in the sense of multistability – rather than a transient hysteresis. To acknowledge that we can not estimate to which extent the hysteretic behaviour is transient in the present manuscript, we included the following statement at the end of the section describing the soil CO2 emissions: *It should be noted that the hysteretic behaviour arises partly because the characteristic timescales of the high-latitude carbon cycle – most importantly of vegetation shifts and the decomposition of soil organic matter – are larger than the timescales of the investigated climate change scenarios. In addition, high latitude soils have a large thermal inertia, especially due to the large amounts of energy required or released by the phase change of water within the ground. Thus, the simulated behaviour does not necessarily indicate the multistability of the system but may merely exhibit a transient hysteresis as described by Eliseev (2014). However, the question whether the hysteresis is purely transient or indicative of multistability is beyond the scope of this study and the subject of an ongoing investigation.*

Minor Comments

- ll. 74 and 789: The correct year for Eliseev et al.'s paper is 2014.
  The reference was corrected to Eliseev (2014).
- l. 110: '... very different properties... ' Very different for thermophysical or for hydrological processes?

The text was changed accordingly.

- l. 137:zin Eq. (1) lacks units. Otherwise, this equation is ambiguous.
  Here, the units were added.
- l. 163: '... the number of days per year in which surface temperatures crossed... '. I guess, it should be 'the day of the year when surface temperature crossed... '.
  We actually use the "number of days" in the calculation of the vertical transport velocities. The idea behind this is that repeated thawing and refreezing leads to a more effective mixing of the soil properties within the active layer.
- l. 201: it should be 'anaerobic or aerobic'.
  The text was changed accordingly.
- l. 201: it should be 'its shape'.
  The text was changed to *"the shape of the litter"*.
- l. 265: the better spelling would be 'soil chemical composition'.
  The text was changed accordingly.
- l. 270: the better spelling would be 'soil pore space'.
  The text was changed accordingly.
- l. 343: '2' and '4' in chemical formulae should be subscripts.
  Spelling was changed to subscript numbers throughout the manuscript.
- l.537: I guess, one of two numbers is wrong, because 9 MtC yr-1 is 12 Tg CH4yr-1.
  Prof. Eliseev is absolutely correct and flux is indeed 12 Tg CH4yr-1.
- Fig. 2: This figure is difficult to read. I suggest to place ensemble means in the left column and draw the maps in the middle and right columns as differences

from these ensemble means. In addition, phrases like 'Ensemble–minimum thaw depth (annual maximum)... ' in caption to this figure is quite difficult to understand for a general reader. I suggest to put the wording in form 'Ensemble minimum for annual maximum thaw depth... ' and so on.

The figure was adapted accordingly (see below). **Caption: Simulated permafrost, vegetated fraction and inundated areas in the year 2000: a)** Ensemble mean of the annual maximum thaw depth, corresponding to the year 2000 (1990 - 2010 mean). Grey areas indicate grid boxes in which the annual maximum temperatures throughout the top 3 meters of the soil exceeded the melting point for more than 10 years in the period 1990 - 2010. These are considered to be unaffected by near-surface permafrost and are not taken into consideration in the study. **b)** Same as a but for the difference between ensemble minimum and mean. **c)** Same as a but for the difference between ensemble maximum and mean. **d)** Ensemble mean vegetated fraction. Note that this is the maximum grid box fraction that can be covered by vegetation, while the actual vegetated cover depends on the current state of the vegetation and can vary throughout the year. **e)** Same as d but for the difference between ensemble minimum and mean **f)** Same as d but for the difference between ensemble maximum and mean. **g)** Annual minimum inundated fraction for the year 2000. Shown is the ensemble mean. **h)** Same as g but for the annual mean. **i)** Same as g but for the annual maximum inundated fraction.

References

Eliseev, A. V., Demchenko, P. F., Arzhanov, M. M., and Mokhov, I. I.: Transient hysteresis of near-surface permafrost response to exter-nal forcing, Climate Dynamics, 42, 1203–1215, https://doi.org/10.1007/s00382-013-1672-5, https://doi.org/10.1007/s00382-013-1672-5,2014.

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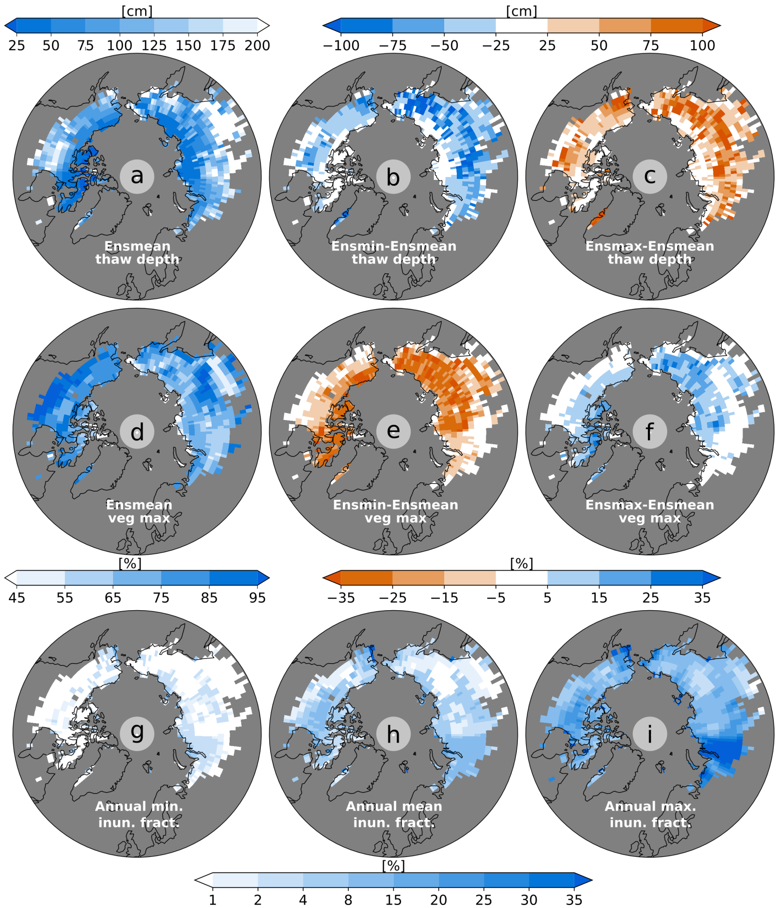

Fig. 1.

Interactive comment

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Discussion paper