

Interactive comment on “Conceptual Model to Simulate Long-term Soil Organic Carbon and Ground Ice Budget with Permafrost and Ice Sheets (SOC-ICE-v1.0)” by Kazuyuki Saito et al.

Anonymous Referee #2

Received and published: 16 September 2020

General comments:

In the manuscript (MS), Saito et al. developed a numerical soil organic carbon–ground ice budget model (SOC-ICE-v1.0) to compute long-term evolution of soil organic carbon (SOC) and ground ice (ICE). The model was developed for the last 125 thousand years for areas north of 50°N. Based on the authors, the simulated results successfully (i) reproduced temporal changes in northern SOC and ICE, consistent with current knowledge and (ii) captured regional differences in different geographical and climatic characteristics within the circum-Arctic region. Moreover, the authors considered that the resulting circum-Arctic set of simulated time series can be compiled to produce

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snapshot maps of SOC and ICE distributions for the past and present assessments or future projection simulations. Saito et al. concluded that the model provides substantial information on the temporal evolution and spatial distribution of circum-Arctic soil carbon and ground ice. However, model improvements in terms of, e.g., forcing climate data and choice of initial values are required in the future.

It is evident that the authors have addressed a topical issue, spatiotemporal prediction of soil organic carbon and ground ice across the circumpolar permafrost area. Moreover, the period of time is notable, the last 125 ka years. To my opinion, the topic of the MS fits well to Geoscientific Model Development (GMD). In general, I consider this MS to be relatively concise and well-written. However, I have two major concerns and some suggestions to improve the work.

First, there seems to be overlap between this MS and Saito et al. (2020) published in *Progress in Earth and Planetary Science*. Please clarify the novelty and added value of this MS. Second, how reliable are the results of SOC and ICE for areas covered by glaciers (e.g. continental ice sheets)? How these results relate to the fact that, for example, the site in northern Europe (Kevo) was covered by continental ice sheet until ca. 10 ka? The model seems to produce substantial variation in SOC despite the presence of glacier ice cover.

Saito, K., Machiya, H., Iwahana, G., Ohno, H., & Yokohata, T. (2020). Mapping simulated circum-Arctic organic carbon, ground ice, and vulnerability of ice-rich permafrost to degradation. *Progress in Earth and Planetary Science*, 7(1), 1-15.

Specific comments:

Title: Please reassess the use of 'conceptual' in the title. I would see the model as 'numerical' rather than 'conceptual'.

In the Abstract (and elsewhere), you use 'a conceptual and a numerical...'. For me a conceptual model differs from a numerical model but here the presented SOC-ICE-

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v1.0 is both. Could you please clarify the motivation for the combination of conceptual and numerical?

It would be nice to have information on the spatial resolution of the model outputs somewhere in the Abstract. This could be relevant also in the Introduction or in the beginning of the section 2.

Introduction: maybe it would good to include definition of permafrost.

Lines 37-39: You state that ‘...well-recognized and widely examined using...’ but refer only to one paper. Maybe few references more?

Lines 46-52: I would present the ‘second pathway’ and ‘third pathway’ in reverse order. The third is more significant pathway?

Line 59: Could Hugelius et al. (2020) published in PNAS be relevant here?

Hugelius, G., Loisel, J., Chadburn, S., Jackson, R. B., Jones, M., MacDonald, G., ... & Treat, C. (2020). Large stocks of peatland carbon and nitrogen are vulnerable to permafrost thaw. Proceedings of the National Academy of Sciences.

Lines 72-74: Need for so many references here?

Line 177: Why the warm period was set to start at 14 ka? For example, Holocene began ca. 11,5 ka before the present.

Sections 3.1, 3.2.1 and 3.2.2: I find it problematic to include references in the Result sections (results of this MS can be confused with published ones; look like discussion).

Lines 474-476: Do the literature support the mostly negative balance (accumulation rates) across the permafrost region for the past 12 ka? Please consider this in the Discussion.

Line 480 (also in the Abstract and Conclusion): You highlight the possibility to produce snapshot maps. Please provide some maps as examples in the MS.

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Section 4.2: You focused on soil carbon in this section. How to improve the model outcomes related to ground ice?

Lines 551-552: The sentence ('One of these...') should be removed (not relevant here).

Table 1: In Eg. (4), why there are same figures for sand and clay? Their hydrological properties are different.

Table 2: There is no information for the 'Ta' and 'Pr' in the caption? If these area air temperature and average precipitation, please give information from what period they are? At least, some of the figures seem to be odd for modern annual averages.

Technical corrections:

Line 15: You could add 'permanently' (...permanently frozen...)

Line 18: You could add 'ground' (...and ground ice...)

Line (and elsewhere): Should the references be in chronological (or alphabetical) order?

Lines 58 and 64: Please correct Gorham 19991.

Line 107: Need to add 'soil' (soil carbon) and 'ground' (ground ice)?

Line 169: Should 'annual mean temperature' be 'MAAT'?

Lines 455-457: I would remove the sentence 'The lowest and highest whiskers of the box-whisker...'. This is good in caption but not needed here.

All abbreviations in the Figures and Tables should be spelled out in the captions.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-80>, 2020.

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