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Geoscientific Model Development Discussions

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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 #1

Received and published: 28 August 2020

General comments:

The authors present a very timely and necessary modelling framework for assessing the spatial distribution of soil organic carbon (SOC) and ground ice (ICE) across the circumpolar permafrost region between the 50th and 70th latitudes. Moreover, the presented SOC-ICE-v1.0 model can be used to produce maps of these distributions at any time point during the last 125,000 years. This is obviously an ambitious task to initiate with, but the authors accomplish in providing modelling tools that have potential to inform about the history and future of permafrost-affected soils. In their recent manuscripts and published works the authors have already assessed future develop-

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ments and published snapshot maps using outputs from SOC-ICE-v1.0.

Despite the simplified consideration of some relevant factors for SOC and ICE dynamics (very coarse representation of soil properties, only one ice core to force past circumpolar climate deviations), the models show promising performance in reconstructing SOC and ICE histories. What I find impressive is the model's ability to account for the role of continental ice sheets and changing sea level in the reconstructed time series for SOC and ICE. The manuscript is well written. Results are well presented and likely reproducible, although some of the performed pre-examinations are mentioned in a cursory manner.

Concerning the results, the time series over the last 125 ka appear mostly realistic, although the lack of validation data especially for ground-ice accumulation history hinders model evaluations. Despite the comparisons using observations from 8 locations across circumpolar north, I remain rather uninformed about the model's capability to reliably produce the actual spatial SOC and ICE variability. The authors state that the modelled SOC and ICE foremostly paint a picture of relative contents, that is, in relation to other grid cells across the study area and not absolute in situ contents. Rather coarse spatial analysis resolution and very coarse representation of soil properties additionally reduce the model's potential to address the local to regional consequences of organic carbon cycling to the atmospheric GHG or ground subsidence due to ground ice melt. Nevertheless, I consider that at the present SOC-ICE-v1.0 constitutes a fair step towards these goals.

I applaud the authors for their explicit explanations of the performed parameterisations. However, coming from a different modelling tradition, I have recognized and pointed out several places where I believe the methods would benefit from further clarification. Moreover, I have several specific comments and suggestions for the authors to consider before consideration of publication in GMD.

Specific comments:

lines 30-34: As ground ice is the other studied property, I would suggest adding a very brief note on what consequences its melt may have.

lines 39-40: While yedoma is a prominent type of ice-rich permafrost, all ice-rich permafrost is not exclusively yedoma but other types of ice-rich permafrost occur.

line 42: I would suggest avoiding the term “buried ice” in the context of ice wedges, as buried ice typically refers to ice accumulated on the ground surface (e.g. glacier, lake, river or sea ice) and later buried by sediments. See, e.g., Permafrost Subcommittee: 1988, Glossary of permafrost and related ground-ice terms, Associate Committee on Geotechnical Research, National Research Council of Canada, Ottawa, Technical Memorandum No. 142, 156 pp.

line 51: Please elaborate. What is "the aerial extent of ice-rich permafrost"?

I wonder if the whole introduction part would read more clearly if the descriptions of SOC accumulation history and research tradition (around the lines 71-84) would be embedded in section 1, and if section 1.1 would then solely focus on general descriptions of the model? Moreover, I am sure that the authors have become aware of a very recent study by Hugelius et al. (2020 PNAS), which appears to have provided notable advances in mapping the circumpolar C distribution. Consider updating parts of the review of current knowledge at lines 57-64 with the information provided therein. Hugelius et al. (2020) Large stocks of peatland carbon and nitrogen are vulnerable to permafrost thaw. PNAS 117 (34) 20438-20446

lines 90-93: The authors say that they incorporated a key parameter that represents temporal and spatial variations in climatic and topo-geographic conditions. This is related to the whole issue of external, or allogenic, factors, which are referred to in a bit inconsistent way by using terms, such as “climatic or environmental conditioning” (lines 517-518), “climatic, topographic and/or land composition” (399-400) or “climate, hydrology and topography” (77). I wonder if it would be possible to more explicitly describe what this parameter represents in this study. As far as I understand only continentality (distance to the closest ocean body) and cover ratios of land, water, ice sheet and its thickness were specifically parameterized. DEM-based topographic conditions were only used in the authors' recently published paper (Saito et al. 2020 Progress in Earth and Planetary Science) to downscale the outputs of here presented model.

line 91: What curvatures? Terrain?

line 142: Can the authors very briefly clarify what they mean by stating that the Mosaic model outputs showed the best settings and results for regions north of 50°? No detailed explanations of the preliminary analyses are needed but please elaborate "settings and results".

lines 183-184: Does altitude data here refer to the thickness of an ice sheet or a digital elevation model? Related to this, clarifications on the used elevation data (if any) is needed. In the reference list the authors have Amante and Eakins (2009) and Tarboton (1989) related to DEM's but they are not cited in the text. Having read the authors' recent paper (Saito et al. 2020 Progress in Earth and Planetary Science) where they produced maps using SOC-ICE-v1.0, it appears that the related DEM was therein used to downscale model outputs.

Figure 3: Consider adding an explanation of the presented subsurface layers. Does the brown box refer to permafrost or impenetrable surface in general?

lines 295-297: Could this examination of "overall goodness of the reproduced time series" benefit from an elaboration or a reference?

Figure 4: The caption suggests that results from litter fall diagnosis are shown but they are missing. lines 315-318: Supplementary Fig. 2 could benefit from a more detailed caption (naming the 6 models, explaining the symbols).

Figure 5: Please align the panels to the same level and maybe label latitudes and longitudes.

lines 337-338, I had problems understanding this sentence. In the Discussion (lines

527-532), the authors provided a clear account on how the initial values for the spin-up were derived. I recommend presenting that piece of text in the Methods, so the spin-up is easier to understand. Please also consider elaborating what “5000 yr” means in this context – point in time or a period for which the model was spun up? Spin-up may also not be familiar for all readers, so maybe open that a little.

lines 368-369: Check language, some words seem to be missing from where the permafrost zone for Kevo site is mentioned.

line 371-372: Please revise the statement/language that Anaktavuk and Yakutsk locate “in areas that include the ice-rich permafrost (Yedoma) region”. Yedoma, or other ice-rich permafrost regions, are not confined to these areas.

The results section has some sentences that would be better situated in the Discussion (e.g., lines 397-401, 407-411). Would it make sense to title this section Results and Discussion? The current Discussion is relatively short in comparison to the Results.

Chapter 3.2: In this chapter (more precisely, in section 3.2.3.), the authors also examine the simulated results of ice accumulation and dissipation, so it could be mentioned in this preamble (lines 430-436).

Chapter 3.2.1: I think that the authors do good job in discussing the possible reasons for the discrepancy between observed and simulated basal age. For example, the used climate data reconstruction from one ice core anticipatedly affects the results as the authors later discuss in 4.2. Related to this, in some point of the manuscript it would be beneficial to provide a brief reasoning behind using only one ice core and why it is suitable in the present purpose.

lines 472-473: I wonder about the large melting of ground ice during 14-15 ka, given that at least Kevo was under the continental ice sheet at that time. Is the anomalous melting related to glacial dynamics or warming climate, and also around 11 ka when the ice sheet finally retreated from the area? Could the authors say something more

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precise about past glacial/ground-ice dynamics here in order to assess the reliability of the model as no independent observation-based validation data is available?

lines 501-502: Please revise the sentence. It could be made more readable, e.g. the expression "locality-prone profiles".

lines 527-532: Explaining this procedure would have seriously helped to understand the initial forcing values first presented in the beginning of the Results section. I thus suggest relocating this text to the Methods. Please also see my comment for lines 337-338.

line 536: I guess that by “relative” risks the authors here may refer to their earlier statement on how the model results do not necessarily represent the absolute SOC or ICE at a grid cell but rather their amounts relative to other grid cell? However, I think this is not clear in the first sentence of the Conclusion, and thus here “relative” could be removed.

lines 551-552: Do the authors here refer to another study using their model?

Technical corrections:

line 10: Is “relative” relevant or understandable without context?

line 39: "ice-rich-permafrost" to "ice-rich permafrost"

line 54: is “relative” needed?

line 58: Please correct “Gorham 19991”

line 114: In the abstract, the authors write "A conceptual and a numerical soil organic carbon–ground ice budget model". Are they separate models or one model as stated here (“The developed conceptual numerical model. . .”)? Please be consistent throughout the text.

line 142: Rodell and Beaudoing . . .., publication year missing lines 175: “closest ocean, distance from the coast of the closest ocean” Is “closest ocean” redundant?

Table 1: kpice to kice

Table 2. Please consider explaining in the caption what the tau symbol denotes. Ta and Pr could also be explained. What does “Simulated ground ice is in meter” mean in footnote d?

line 394: Saito et al. 2020, not in review anymore.

line 419: Anaktavuku to Anaktavuk

line 420: length of the thawed layer?

lines 454-457: This information (starting from “, and then sorted to. . .”) is found in the caption for Figure 8, and thus not necessary here.

At line 481, could the authors repeat the temporal resolution, i.e., for how long a period the snapshot maps can be compiled.

line 482: Saito et al. 2020 now published

line 491: There are Yokohata et al. 2020 a and b in the Refences, which one does this cite to? Is it published?

line 499: I think the last sentence; “Below is a list. . .” is not necessary here.

line 503: Thence to Hence

line 508: Do the referred timings of initiation refer to the results here, or by Morris et al. 2018?

line 513: “may improve function” could be clarified/said in a different way

line 532: I suggest editing; “less than a dozen” to “eight”

At least the following listed refs are not cited in the text:

- Amante and Eakins 2009

- AMAP 2017

- Biasi et al. 2005 (Biasi et al. 2013, however, is cited but not in the references)

- Bradley 1999

- Tarboton 1989

The following, in turn, not found in the References:

- Yu et al. 2008

- Brown et al. 1998

Please check all citations and references.

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