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Interactive comment on "Simulated high-latitude soil thermal dynamics during the past four decades" *by* S. Peng et al.

Anonymous Referee #2

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The manuscript presents a comparison of soil temperature trends simulated by nine numerical models. Texts are well organized and clearly written, and tables and diagrams are effective in showing the variability among simulated soil temperatures. It is an interesting exercise to demonstrate the large uncertainty in soil temperature simulation in the permafrost region. However, I am not sure what scientific advances we gain from this exercise. If I am not mistaken, the nine models are driven by different climatic forcings, and have vastly different structures and algorithms (Table 1). Therefore, the variability in model results is due to the variability in both forcing and model algorithms. As a result, the reader is left wondering what the results of this exercise really mean. For example, CoIM and ISBA show completely different patterns of soil temperature trends (Figure 4). Is this caused by model algorithms or climate forcing?

As someone who has done much work on the comparison between permafrost model simulations with field observations, I am keenly aware of model sensitivity to subtle changes in surface variables (e.g. vegetation parameterization), subsurface variables (e.g. soil moisture), and boundary conditions (e.g. magnitude of geothermal flux). Permafrost models are also sensitive to the initial condition, as well as the thickness of the model domain. In order of the reader to understand the meaning and implication of model results, it would have been much more meaningful to conduct the model comparison exercises using a common set of forcing variables. For these reason, I cannot recommend publication of this manuscript in its present form. I suggest that the authors re-design and conduct new model comparison exercises, or present more meaning explanation for differences among the present simulation results and discuss how what causes these differences.

In addition to the fundamental comments above, the following is specific comments.

Page 2305, Line 11-16. What are the scientific objectives of this work? The objectives (1)-(3) cannot be meaningfully achieved, if model simulation results are strongly dependent on model algorithms and structure.

Page 2305, Line 19-23. In addition to surface forcing, the forcing from the bottom boundary of models needs to be explained clearly. Energy input in the form of geothermal flux has strong effects on soil temperature.

Page 2305, Line 23-24. If I have understood correctly, three out of nine models do not consider the “effects of water in soil on phase change”. Does that mean these three models do not simulate the freezing and thawing of soil water? Since permafrost is the phenomenon of pore water freezing and thawing, I am not sure if these three models are even suitable for the purpose of this exercise. Clear justification is needed for the inclusion of these models.

Page 2306, Line 8-10. Why were different forcing data sets used for different models? Clear justification is needed in relation to the scientific objectives of the study.

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Page 2307, Line 18-22. The thermal condition of top 3m is strongly dependent on the presence of absence of permafrost in the underlying zone. In some regions permafrost is more than 10-20m thick. Table 1 indicates that some of the models are not sufficiently deep to represent the effects of underlying permafrost.

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