Point-to-point responses to RC3

This manuscript addresses an important question: the interpolation of sparse data in a non-stationary context. It develops a framework that is seen as generic and demonstrates it in the specific context of interpolating subglacial bedrock topography. The novelty is the use of a large number of training images coming from a variety of deglaciated areas, and to devise a scheme to select a subset of them that is specific to each simulated region.

My overall assessment is that the manuscript is scientifically sound and will have an impact in the glaciology community, and also possibly more generally in other applications of MPS to large-scale problems, including space-time simulation. The tests in section 4 clearly show the value of the approach. Importantly, this study improves our understanding of the usage of MPS in the case of very large models that are fed by lots of data and training images. It demonstrates the need to select a specific subset of training images for each region rather than using the entire training image database for all areas. This strategy provides gains in terms of quality and computation, which is very valuable. Also, it could fuel the discussion on using machine learning approaches to address such problems (e.g. GANs) which would require a new training for each sub-area, making them inefficient.

This said, I do have remarks on some technical aspects of the proposed approach that I outline below. Despite these comments, I find the method convincing and I believe the manuscript deserves publication in GMD.

Response: We highly appreciate the reviewer's positive evaluation on our paper. The remarks and suggestions are also very constructive to improve the paper. They therefore have been incorporated into the newly revised manuscript. Changes are highlighted in red in the revised manuscript. Please see below our point-to-point responses.

- While the approach is novel, some of the existing literature was missed. For example, on l.63-54, it is mentioned that this is the first application of MPS to subglacial topography. A recent paper doing just this appeared in The Cryosphere: https://tc.copernicus.org/preprints/tc-2021-161/

  Response: We have added this new paper to our reference list and revised the manuscript accordingly.

- The proposed method involves a number of modeling steps and choices. This is fine when justified, but here I did not see a clear justification for many of the choices made. I detail some instances of this below:
  - I do not see a clear motivation for using a modified Hausfdorff distance in eq. 1. Why using a distance between patterns rather than some statistical similarity between patterns, e.g. in terms of integral scales?

    Response: This is a very good question. The reason is that we need a simple method to measure distance between TIs' representative patterns. Using statistical similarity approaches would require two steps 1) specifying statistics 2) specifying distance.The Hausdorff distance captures both in one because it measures difference between shapes, which is relevant to this application. But yes, the approach suggested by the reviewer will also work.

- There exists a literature on methods to select one or more TIs based on conditioning data (e.g. Pérez, C., G. Mariethoz, and J. M. Ortiz (2014), Verifying the high-order consistency of training images with data for multiple-point geostatistics, Computers and Geosciences, 70, 190-205, 10.1016/j.cageo.2014.06.001, or Abdollahifard, M. J., M. Baharvand, and G. Mariéthoz (2019), Efficient training image selection for multiple-point geostatistics via analysis of contours, Comp. & Geosci., 128, 41-50, https://doi.org/10.1016/j.cageo.2019.04.004). This is the same problem that is addressed here with the probability aggregation approach. Such methods are not considered or mentioned. It is fine that the authors propose a new methodology, but the reason for not using existing approaches should be given.

  Response: We thank the reviewer for providing the above TI selection methods. We agree that they share similarity with the TI selection problem in our paper. However, the unique contribution of our paper's approach is that we quantify the posterior probability, then sample from it. We have added these papers to our references to Section 3.7 and explained our reasons in the revision.

- The general approach proposed is rather sophisticated (probability aggregation - PSO optimization – kernel density estimation), which complexifies the implementation. From a user point of view, these steps should be justified. Basically, it is pretty clear how things are done but the why is not always explicit.

  Responses: The methodology framework may be complex, but the execution is simple from a user's perspective. Also, from users' perspective, we provided the implemented code as supplementary material. The codes only require the user to provide the line data, define local areas, and a few DS parameters to run it. They don't need to reimplement the theoretical details.

- 303-304: this is a strong assumption because these distances are considered in a high-dimensional space. Can it be justified?

Response: Yes, it can be justified. The MDS plot of Figure 5 justifies this assumption. It shows the nearby TIs in MDS space are very similar to each other and tend to have the same geological features of fjords, mountain ranges, etc. They will thus have a similar probability of being assigned to the same radar data subarea. We added a line to explain this.

- The procedure for the choice of the TIs is rather complex. This is justified by the dependence between neighboring TIs which is modeled by probability aggregation. However, since there are local data everywhere in the domain, simply selecting a set of TIs statistically similar to the data might also perform well. Has this been tested? For instance, looking at figure 10, I am wondering whether a similar ranking could be obtained with a simpler approach, for instance considering similarity in terms of histogram and variogram similarity.

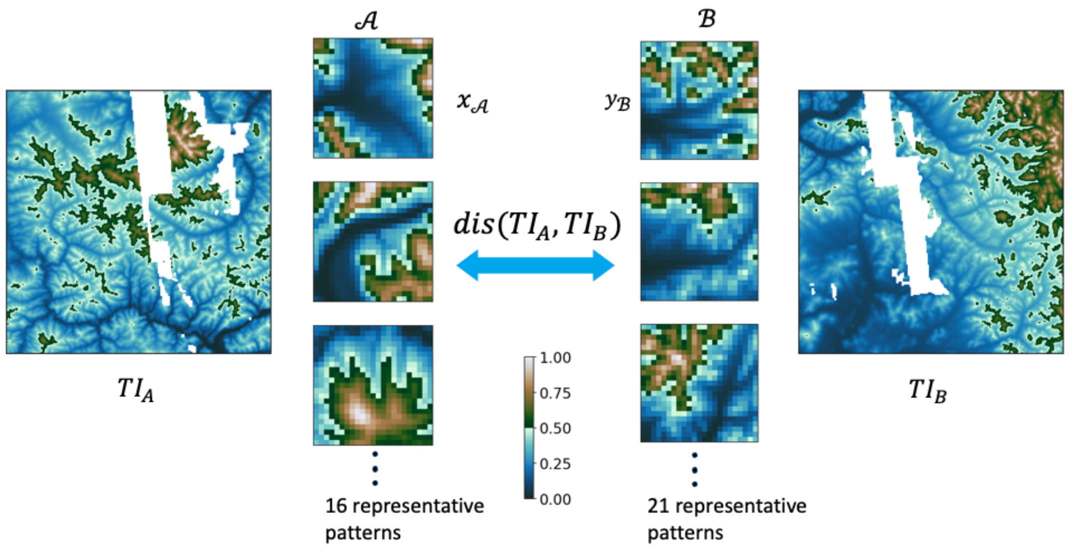
  Response: That has not been tested. Our approach is general, it will work in cases where areas with dense data border areas with sparse data, where the TI selection of the sparse data area may lead to conflict with the dense data area. Secondly, the approach also creates multiple realization of possible pairs of TI in neighboring zones, thereby possibly create a fuller sampling of the possible TI selection in any two neighboring areas

- It is likely that the training images need to be optimally oriented prior to simulation. Or possibly, one could use a rotation-invariant distance. Has this been tested?

  Responses: Yes, we tested to rotate the TIs with different angles. It turned out to make the DS computationally demanding when including more rotated TIs, but it produces similar topography results to non-rotating.

- Section 3.1.3 and the legend of figure 4 are quite unclear to the uninitiated reader and should be improved.

  Responses: we have re-plotted the legends of Figure 4 to make it clearer. Below is the new figure:



- 227: This statement about boundaries seems incorrect. DS is affected by the boundaries of the TI, as the edges of the TI cannot be sampled, especially for large data events.

  Responses: Here we didn't mean the TI boundaries, but cross the specified radar line regions. Thanks for pointing this statement out. We have rephrased this sentence to avoid confusion.

Minor comments/typos:

l.22: sentence grammatically incorrect

Corrected

l.23: provides

Corrected

l.24: flight lines data

Corrected

l.86-87: here the work by G. Pirot could be mentioned as it does exactly what is mentioned in this sentence (https://doi.org/10.1016/j.geomorph.2014.01.022, and also doi:10.1002/2015WR017078).

The two references are added.

l.96-97: Awkward sentence, rephrase

Sentence has been rephrased.

l.115: remove extra parentheses

Corrected

l.149: section 0 is a mistake

Corrected

l.182: verbatim copy should be explained

We have rephrased this line.

l.204: remove word expressed

Corrected

l.286.287: this reference seems out of place. This is the TI selection, not DS simulation

Corrected

l.287: erroneous reference: there is no section 3.13

Corrected

l.292: parameterization … convergence

Corrected

l.293: make PSO

Corrected

l.320: “is to reflect” -> should be rephrased

Rephrased to “is to quantify”

l.370: section 0 doesn’t exist

Corrected

l.397: rewrite sentence (diving ->dividing)

Corrected

Figure 18: problem of color scale where white areas appear in the simulation domain

We have changed the white area to black.

l.424: density of water is 1000 kg/m3

Corrected

Check the citations: several are duplicated.

Duplicated citations are removed