

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2021-297

John Goff (Referee)

Referee comment on "Mapping high-resolution basal topography of West Antarctica from radar data using non-stationary multiple-point geostatistics (MPS-BedMappingV1)" by Zhen Yin et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-297-RC1>, 2021

This paper describes an interesting and potentially very useful methodology for realistic interpolation of sub-glacial topography (one of many potential applications). Overall I think that this is solid and important contribution to the field. The methodology is quite complex, however, with many steps involved. The text is a bit dense with undefined jargon, and I feel the authors could do a much better job at explaining these steps, and particularly in explaining basic concepts. For example, I was never sure what the authors meant by "distance" between two training images, and that set me at a big disadvantage in trying to comprehend the rest of the methodology. Another example: the authors never define how the values of MDS1 or MDS2, key parameters in the methodology, are determined. There are many more such examples noted in my marked-up pdf file.

I hate sounding like the aggrieved reviewer, but really, the authors scant mention of my own paper on the conditional simulation of nearly the same data set had me at a loss. The two methods are extremely different, but the ultimate product and goals are identical in trying to produce a realistically rough surface conditioned on existing radar soundings and accounting for a high degree of spatial heterogeneity. Of particular note, my method spent a considerable effort on ensuring the continuity of fjord-like channels beneath the glacier, which are obviously very important factors in flow simulation and likewise are poorly reproduced by standard interpolation schemes like kriging. How does this method perform in that measure? I suspect it actually does quite well – that the highest probability deglaciated terrain training images do a good job in conditioning the data interpolation to that geometry. But the authors do not explore that property. The authors also did not do a good enough job distinguishing the superiority of their method over SGSIM. The latter images actually looked quite good.

As noted in my returned pdf, the figures and captions could use some work. A few of the issues: A lot of the training images were just reduced from larger versions, meaning that the annotation was too small to read. On several images the color white is used both to indicate areas of no data and Z values >500 m. This ambiguity needs to be resolved.

Many of the captions were far too brief and failed to explain what is going on in the figure.

Please also note the supplement to this comment:

<https://gmd.copernicus.org/preprints/gmd-2021-297/gmd-2021-297-RC1-supplement.pdf>