The authors have made an effort to more clearly explain the reasoning behind their method. This should be appreciated. However, new unexplained concepts (e.g. „well-known iterated conditioned mode method", „well known Duchon's energy") are used to support the main reasoning behind posing and solving their minimization problem. The authors write that the conditioned mode method is „well-known". I would argue that this is something very specific and needs more explanations why it is relevant to the proposed method. Especially that the conditioned mode method has something in common with Markov random fields, a concept that does not occurr in the manuscript. I don't recommend any decision (I marked major revision in the system because I have to select something) because I don't do research in functional analysis (note that the authors now cite a theoretical paper from functional analysis Duchon, 1977 for their well known Duchon's energy). So maybe colleagues doing research strictly in functional analysis could say something more decisive.

More detailed comments are given below.

#1 In the response file, the authors write:

We thought the Hermite-Birkhoff interpolation in pure mathematics could be referenced.

Any valid source of information can be referenced. Because you deleted the Bachman&Narici book from the referenced publications, you have acknowledged that it was invalid. Thank you. Now you have Duchon, 1977 which seems to pose a new yet similar challenge – why is this publication relevant?

#2 In the response file, the authors write:

Comparing with explicit modeling, implicit modeling has the efficiency advantage of avoiding a lot of workloads of human-computer interaction. The 3D orientations are usually surveyed on the outcrops of strata.

Since my comment was about comparing triangulation and interpolation methods, I will make a point about the difference in computational time.

Interpolation methods, e.g. kriging (but also in Grose et al. https://doi.org/10.5194/gmd-14-3915-2021), often require solving large systems of linear equations. For example, for kriging equations, the size of the linear systems is proportional to the number of N sampling points. This implies that the CPU time for solving this system on a computer is proportional to N^2 (Mallet, J.-L., 2002).

However, the Delaunay triangulation of n points in the plane can be computed in O(nlogn) expected time, so triangulation is faster to calculate.

Theorem 9.12 *The Delaunay triangulation of a set P of n points in the plane can be computed in $O(n \log n)$ expected time, using $O(n)$ expected storage.*

Proof. The correctness of the algorithm follows from the discussion above. As for the storage requirement, we note that only the search structure $\mathcal{D}$ could use more than linear storage. However, every node of $\mathcal{D}$ corresponds to a triangle

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Fig 1 Scan from de Berg et al.

References:

- Mallet, J.-L., (2002), Geomodeling, Oxford University Press, p. 510.

- De Berg M, Cheong O, Van Kreveld M, Overmars M (2008) Computational Geometry: Algorithms and Applications, 3rd Ed. Springer

#3 In the response file, the authors write:

Inspired by the well-known iterated conditioned mode method, instead, we devise an iterative scheme to optimize potential function f and the gradient magnitudes $\mathbf{l}$ alternatively.

The authors write that they are inspired by „conditioned mode method", however I cannot see any citation for this inspiration in the manuscript. The authors cannot assume that geological readers are just familiar with the concept of „conditioned mode method". In fact, I can see the following definition (https://en.wikipedia.org/wiki/Iterated_conditional_modes):

„In statistics, iterated conditional modes is a deterministic algorithm for obtaining a configuration of a local maximum of the joint probability of a Markov random field. It does this by iteratively maximizing the probability of each variable conditioned on the rest."

You don't use Markov random fields in the manuscript, so why the „conditioned mode method" should be relevant to your method?

#4 In the response file, the authors write:

In the revised manuscript, we have discussed the limitations of our implicit modeling method in Section Discussions.

However, in the Discussions section, I cannot see any new comments about limitations (e.g. use of data from outcrops discussed in the response file). There are only new comments about future work.

#5 The authors explain the three components of the energy function in the response file but not in the revised manuscript.

#6 What is actually the „novel optimizing term" ? (Line 57)

#7 In the response file, the authors write:

„scattered multivariate Hermite-Birkhoff data (i.e., unstructured points and orientations)"

If the definition for Hermite-Birkhoff data is „unstructured points and orientations", then why not writing only „unstructured points and orientations"? I still don't know why the term „Hermite-Birkhoff" appears in the manuscript.