Response to Reviewer #2:

I read with interest the paper entitled "Locality-based 3D multiple-point statistics reconstruction using 2D geological cross-sections". The paper describes a new methodology to build 3D realizations constrained by 2D sections that act as local training images and hard data for MPS algorithms. The method seems to give realistic results and perform better than existing algorithms. Globally, the paper and the presented results are convincing. However, I would like the following general comments to be addressed by the author:

Thank you very much for your positive and constructive comments and suggestions. We have corrected all the issues you raised in the revised version. The following is a point-by-point response according to your comments.

1. The introduction is a little bit messy, giving an overview of many papers related to MPS, but missing the point of highlighting the specific issues tackled by the paper. For example, the 2nd paragraph (P2L15) makes the history of MPS. This is not the point, you should rather insist on the importance of the TI (what you do in paragraph 3) and the description of the state of the art (relatively vague in the current version). Many sentences in the introduction are too vague such as "some assumptions have been implemented to reconstruct 3D models" (P3L14, which assumptions?) or "A promising reconstruction method … by adapting the DS algorithm. However, large-scale" (P3L16, the "however" refers to something that is not explained, the problem should be clearly stated).

   Thank you very much for your constructive suggestions. We rewrote most of the introduction to highlight the specific issues tackled by our paper. Specifically, the history of MPS is compressed in the revised manuscript (see P2L4-L13, L22; P3L4-8); vague descriptions have been corrected (see P3L16-22); and the limitations of the existing have been depicted clearly in the new version (see P2L26-32, P3L25-26, P3L29-34, and P4L1-2).

2. I found the methodology section difficult to follow. Indeed, the proposed approach borrows some techniques from existing algorithms (mostly direct sampling), so that part of the methodology is described in other papers. Although, some parameters are common with DS, the philosophy is quite different as DS is never explicitly calculating cpdf. In that sense, the proposed methodology is closer to classical

approaches such as snesim (except that the cpdf are not stored). I would therefore recommend to explicitly describe every part of the algorithm, without (too many) references to previous publications. Doing so, the methodology would be self-sufficient. Part of the methodology (dissimilarity metrics and MDS) is introduced in the result section and should be moved to the methodology.

We are so sorry for that we did not describe the methodology section clearly. In the revised manuscript, we have reorganized the methodology section. Some existing information but used in the following sections was moved to a new section Background Information, such as the pattern distance (see section 2.1, P5L8-17) and the existing probability aggregation formulas (see section 2.2, P5L18-26 and P6L1-11). The description of dissimilarity metrics and MDS were also move to the section Background Information (see section 2.3, P6L12-17).

3. In the parameter sensitivity, it is argued that some parameters are similar to DS, and thus the paper focuses on the new parameters. Nevertheless, although the proposed method borrow some ideas from DS, it is clear that the approach is different (In DS, you stop searching as soon as you find an occurrence with distance below the threshold, here you continue scanning to get the cpdf). Therefore, we cannot assume without checking that some parameters such as the threshold, the search neighborhood or the fraction of the TI that is investigated will have the same influence in DS and in the proposed approach. In addition, some interaction between the parameters is expected. For example, between the threshold and the maximum number of occurrence, or the number of available sections, some interactions can be expected. Indeed, if you increase the threshold, you will reach faster the maximum number of occurrence.

Thanks a lot for your insightful suggestions. The interaction between two important parameters of DS (distance threshold t and fraction of training image to scan f) and two new parameters presented in our method (number of cross-sections N_{cs} and maximum number of matched patterns from each training image N_{max}) has been added (see section 4.1.4 and Figure 8, P19L5-18 and P20). The results show that these two parameters have similar effects as in DS, so we did not discuss them separately. In addition, it will be repeated with the contents of Meerschman et al. (2013) in which the parameterization of DS was tested thoroughly.

4. The application example is not a real application, but another synthetic benchmark using a real analog. Indeed, there is no verification data or specific application of the model. I would therefore merge section 3 and 4.

Because section 3 (section 4 in the new version) mainly focuses the parameterization and performance analysis of the proposed method, and section 4 (section 5 in the new version) aims to present a complete example in hydrology, so we kept this section, but changed the title to "Synthetic Example: 3-D Reconstruction of Hydrofacies" in the revised manuscript (see P26L5).

Specific comments:

5. P3L6-25. You don't discuss the paper by Gueting et al. (2017) in the introduction, although it is a recent paper on the topic and you borrow some ideas later in the field example. You should reconsider this paragraph to describe with more details previous approaches and how your method is new.

The description of the paper by Gueting et al. (2017) has been added in the introduction (see P3L32-34 and P4L1-2). In order to clearly explain the problems addressed in our paper, we added detailed descriptions of the limitations of existing methods in the revised version (see P3L25-26, P3L29-34, P4L1-2 and P4L19-23).

6. P4L29-30. This sentence does not necessarily refer to the approach proposed by Comunian et al. (2012), but more globally, to any method assuming stationarity. It is always possible to use auxiliary variable to account for non-stationarity, as it is done by the s2Dcd approach in the application example.

Thanks a lot for pointing it out. We have rewritten the corresponding sentences in the revised version (see P7L3-6).

7. P5L3-8. For reconstruction algorithm, there might be a confusion between training image and hard data. Here, the sections that are used are both training images and hard data. To some extent, using the whole section as TI and the sub-sections as hard data is already a good way to locally constrain the simulations. The s2Dcd algorithm is already performing well in that sense.

The sections that are used in our work are regarded as both training images and hard data, but it is different with s2Dcd. Obviously, s2Dcd uses a series of 2-D simulations to fill a 3-D domain, but a random simulation path containing all uninformed locations is used. The corresponding descriptions have been added in P4L19-22, P7L15-17, and P7L19-21.

8. P5-L9-10. The choice of the subsections within domains is not necessarily selecting the most local information. Indeed, this depends on the location of the node within the subgrid, some other sections might be closer, for example when close to a domain boarder. Since the approach does not store any cpdf, you could center the sub-domain on the node and the TI would change for each node. This would avoid the issue of case 2 in Figure 2, where many nodes closer to the node to simulate are actually out of the TI.

A great idea! But if we adopt this idea, the whole method proposed in this paper has to be changed. In fact, a 3-D domain is divided into several local parts according to the spatial relationship of the cross-sections, and then the MP statistics will be captured from the local surrounding sub-sections for a node to be simulated. This is a very good idea, and we will consider it in our future work.

9. P7L18-20. I am not sure it is intuitive, you might expect that the parallel sections are somehow correlated (except in case of strong non-stationarity), the multiplication of probabilities could then make more sense... and otherwise for perpendicular directions (if the field is highly anisotropic, the orthogonal directions are bringing totally different information)...

Because these two parallel sub-sections often contain similar patterns, and we just expect a larger number of samples and thus more robust pdf by uniting both of them, so we firstly use the additive method to aggregate the parallel ones, then a multiplicative method is used to combine the orthogonal pdfs. In order to depict it more clearly, the corresponding descriptions have been added in P9L15-20.

10. P8L15-18. In short, you compute the cpdf using all the neighborhood whose distances are below the threshold, right?

Yes, we compute the cpdf using patterns whose distances are below the threshold t. But we used a parameter N_{max} to control the number of matched patterns since if this number is large enough, scanning to training images will be not necessary (see P13L1-3 and section 4.1.2).

11. P9L1-3. Multi-grid approach. The description is not clear. How do you divide the data event within several grids? I thought you were selecting all the data and previously simulated nodes within the radius. Are you only considering neighboring nodes that are on the current grid? Or you just mean that for the first grid there are less points simulated? In practice, everything depends on the radius. Please clarify.

We are so sorry for the unclear description of the multiple-grids used in our work. In the revised version, we added the corresponding description (see P10L24-26). In fact, the neighboring nodes (hard data and previously simulated nodes) around the central node on the current grid are selected to build a data event according to the radius R and the maximum number of points in the neighborhood. Therefore, a large data event is divided into several small parts placed on the different grids which results in smaller neighborhoods on each grid.

12. P9L4-5. I understand that you take the "diagonal nodes" (figure 3) for the smaller grid as they are previously simulated in the multi-grid 2, but the other nodes should also be consider (horizontally and vertically) according to your radius, thus 8 nodes in 2D and 26 nodes in 3D, no? The remark about 3D is strange since you are only considering 3 directions in 2D, this is not a 3D neighborhood or am I missing something? How do you consider previously simulated points or hard data that are out of the 2D planes? Are they simply disregarded, or somehow projected on the plane?

Yes, other nodes will be considered in other two directions. Thank you very much for pointing it out. We have deleted this sentence and added the corresponding description to make it more clear (see P10L29 and P11L2-4). In the local search strategy proposed in this work, three planes through the current simulated node in three orthogonal directions are considered. Other nodes out of the 2D planes will be disregarded (see P11L2-4).

13. P18. Figure 9. The red line shows the proportion in the cross-section, what about the "true" proportion?

The proportions of facies in the 3-D reference have been added and marked by black lines in this Figure in the revised version (see P22L4-6 and Figure 10).

14. P19L6-14. You could actually quantify how realizations are realistic using kernel smoothing (to average the information of the different realizations in a metrics), estimating the density distribution of the realizations around the reference. You can refer to the already cited paper of Hermans et al. (2015) for an application of MDS in the context of 3D MPS in hydrology.

Very useful comment! We have used kernel smoothing to estimate the density distribution of the realizations of three different MPS approaches around the reference (see P24L1-5). Moreover, the related information about kernel smoothing has been added in section 2.3 in the new version (see P6L18-22).

15. P20L15-16. Still, in DS no cpdf is computed as the first matching sample is selected. Here, you continue scanning, so the effect of those parameters is not necessarily similar. For example, an interaction between t, the number of sections and Nmax is expected.

The interaction between t , f , N_{cs} and N_{max} has been added in the revised version (see section 4.1.4 and Figure 8, P19L5-18 and P20).

16. P22L6. You say that the number of sections is insufficient, but you don't provide guidelines about a sufficient density to use your algorithm. What if the sections are not oriented in orthogonal directions?

A recommended density for the number of sections has been given in P14L18-19. If the sections are not straight or orthogonal, we need to project them in orthogonal directions. The detailed description has been added in the revised manuscript (see P31L6-10 and P32Figure19).

17. P23L2-3. A short description of the auxiliary variable would be welcome. I guess it describes the proportion of facies in the different zones along the vertical direction. It does not have to be long.

A short description of the auxiliary variable has been added in revised manuscript (see P27L6-8).

18. P26L2-3. "partial lower dimensional data" is not true. You can only use 2D orthogonal sections in a sufficient amount. Borehole or analogs cannot be used since you argue that local information is important.

In order to illustrate how partial lower dimensional data are used in our work, we added a figure and the corresponding description in the revised version (see P31L6-10 and P32Figure19).

19. P26L7. But you need a lot of 2D sections, which is a clear drawback of the method.

When there are very few or no sections in a direction, a feasible solution has been suggested by Gueting et al. (2017) where sequential 2D simulations are performed to obtained some sections first, and then both the original informed data and the obtained sections are used to reconstruct the model of the entire 3-D domain (see P14L19-21).

Technical comments:

20. P12L6-7. Does the “maximum search distance” correspond to the Radius of previous sections?

Thanks a lot for pointing it out! We have used consistent description for this in the revised manuscript (see P14L6 and P21L7)

21. P21L9. You previously mentioned (P17L15) that s2Dcd was using DS with 4 processors. Please check.

It has been corrected in the revised version (see P25L14-19).

22. P23L11. I think it should be Figure 17 instead of 16.

Thank you for pointing it out! It has been corrected in the revised version (see P28L3-4).