Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-567-RC1, 2016

Hydrology and Earth System Sciences Discussions
Open Access

Interactive comment on "Multiple-point statistical simulation for hydrogeological models: 3D training image development and conditioning strategies" *by* Anne-Sophie Høyer et al.

Anonymous Referee #1

Received and published: 9 December 2016

This manuscript is a well-written and clear case study on the application of MPS to a very large domain. As such, it will be valuable for a range of researchers. While I recommend eventual publication, I also have reservations that should be addressed.

Regarding the content of the study, I appreciate the overall methodology and the emphasis, throughout the discussions, on the fact that the training image and the simulation algorithm are all elements structuring the final models, and as such the evaluation should take place on unconditional realizations.

However, I also found that the conclusions would be much better supported by adding a few elements:

Printer-friendly version

Discussion paper

1) Currently only a single realization is used for each setting. This is clearly insufficient. On top of p. 12 it is argued that the simulation is considered representative, however I don't agree with this statement. Multiple realizations are needed to quantify uncertainty. It is possible that the single realization is representative, but the only way to find out is to compare with a set of other realizations and decide whether the inter-realization variability is small enough, according to a given criterion (e.g. flow, transport, etc). On p.12 (l.11-12) it is also argued that the methods to use multiple realizations do not exist, which is clearly not the case.

2) The assessment of the results is mostly qualitative, both regarding the patterns produced in the model, and to assess the proportions variability (top of p.8, top of p.9). The tools to do exist and should be used. Also, quantitative comparison of the modeled patterns and the patterns in the conditioning data would be a good validation.

3) The literature review part of the introduction is quite incomplete, missing a number of studies that have looked into 3D MPS models. On p.2 l. 25 it is said that not many studies have looked at 3D TI-based models. I disagree, with for example Ronayne et al (2008), Jha et al (2014), Perez et al (2014) to name a few, and a lot of other studies in reservoir engineering as well. For non-stationarity also, there are Cuhgunova et al (2008), Straubhaar et al (2011), and possibly others, who made important contributions.

Regarding the structure of the document, I also have 2 remarks:

1) There is an imbalance between the description of the data and methods, which is quite short (6 pages), and the discussion/conclusion, which is 5 pages. There is clearly too much material in the discussion, including elements that could be removed or moved to other sections. Here are some suggestions:

- P.7, l.15-20: this could go in section 5.4.

- P.9, from l.19: this could go in the introduction

- P.10, l.22 to p.11, l. 2: This is not related to the purpose of the paper and could be removed.

- P.11, l.3-10: The method for the conversion of boreholes to probabilities should be described in the methodology section, not here.

- P.11, l.11-19: There could be a separate section on non-stationarity because it is mentioned often.

- P.11, l.20-34: This is a long paragraph on something that is not done. It could be removed.

2) Sections 2, 3 and 4 could be grouped together as they all relate to the description of the study site.

Other remark:

Figures 7 and 8: the green-purple color scale is very subjective and seems to highlight values around 0.4. It creates artificial discontinuities. A usual continuous color scale (rainbow or grayscale) would be better.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-567, 2016.