Hydrol. Earth Syst. Sci. Discuss., 10, C6249–C6254, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C6249/2013/
© Author(s) 2013. This work is distributed under
the Creative Commons Attribute 3.0 License.

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "The effect of training image and secondary data integration with multiple-point geostatistics in groundwater modeling" *by* X. He et al.

Anonymous Referee #1

Received and published: 9 December 2013

General comments ——————————

The paper provides a case study with a very interesting data set but at least one of the major conclusions is not well supported by the results, there are methodological problems at different steps, and therefore I suggest that the paper should be significantly revised.

Not well supported conclusion ——————————————-

Page 12, lines 16-17. "Comparison of groundwater heads indicated that a training image with higher accuracy helped improving the model simulation".

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

The main point that I see as a problem is that the hydraulic conductivity used in the set of simulations S3 and S4 is coming from a calibration based on a different geometry. I think that it is then not possible to conclude that this training image (TI) is less acceptable than the other based only on this argument.

To explain more precisely why I believe that this argument is not sufficient, I will try to rephrase how I understand the reasoning of the authors and what I consider incorrect. To start, the multiple point statistics simulations in this paper are generated using two training images (TI1 and TI2). The first training image (TI1) is obtained by thresholding the 3D geophysical data set which is covering the whole study area. The second training image (TI2) is made through a boolean simulations of ellipses.

To compare the performances of the two training images, the authors compute the error between simulated heads with the different models against measured heads in the field. In all the models, the hydraulic conductivities of the lithofacies are kept constant and equal to the values obtained through a PEST calibration of another reference model. This reference model is not shown in great detail but it is explained that its geometry is derived via an inversion from the geophysical data. I therefore expect that its geometrical characteristics are closer from the simulations S1 and S2 than those S3 and S4.

When using these conductivites in the geometries simulated by MPS using TI1, it is clear that the fit should be better than if the same conductivity is used in rather different geometries such as those generated using TI2 as a training image. It does not mean that equally good fit cannot be obtained if the values of the conductivites would be calibrated also using PEST. I do not have any proof of that, but knowing that the inverse problem is usually ill posed, it is very likely that this is possible. If this is true, how the authors could defend their conclusion?

For this reason, I am not convinced by their arguments and think that they should improve it.

HESSD

10, C6249–C6254, 2013

Similarly, when the geophysical data are used as secondary information (through probability maps) they tend to let the geometry of the simulations be closer to the one on which they calibrated the hydraulic conductivities. It is therefore normal that the misfit reduces. But is not a proof that a training image is more or less appropriate than the other.

I am insisting on this point because the other conclusions are pretty obvious for people working in the field of MPS and therefore (and sadly) the authors do not bring something substantially new in this paper.

Methodological issues with the MPS approach ——————————————— A training image is normally a conceptual model of the geology. It is usually based on an analysis of the type of deposits that are expected at a given locations knowing the geological environments or analog sites. Here the authors use two training images which are not of that kind.

The first one is directly derived from a geophysical survey. They basically made a thresholding of the geophysical attribute and consider this as an image of the reality. I have some doubts about the validity of this assumption. Geophysical measurements are usually averaging the true properties of the underground through a complex process that depends on the type of instruments, their characteristics, etc. High frequency heterogeneities in the underground are often smoothed out and the resulting images require interpretations. These hihg frequency structure may be very important for flow and transport processes. I understand that the proposed approach may be useful and may help in some cases, but it really needs more discussion. This is not as obvious as the authors seem to state.

The second training image is obtained using TIgenerator, which is basically an unconditional object based simulator. To build their training image, they assume that the objects have an ellipsoid shape but they do not justify it. I am also concerned about that. Why using this geometry and not half ellipsoids or channels, or whatever? There

should be some justifications and discussions. All the strength of the MPS approach is to provide a vehicle between geological interpretation and stochastic simulation. If there is no geological interpretation at all, then why using this technique?

Again, with the proposed geometries for the two TIs, I am not even sure that MPS has a value at all. I am pretty sure that results of similar quality would be obtained with a very simple SIS method. What is then the justification and advantage of MPS in that case? This is not clear to me even if I am very much interested to see it applied. I am just concerned that by doing this type of application, people will just not see the point.

I am very sorry to be so negative, but I also think that there is an issue with the method used to define the parameters of the object based model. The proportions are constrained so that they are similar for both TIs and this is ok. But the procedure used to define the dimensions of the ellipses seems to be incorrect. It is not described in detail and therefore I may be wrong, but what I understood is that the authors measured on TI1 the lengths of the objects by scanning the TI in the different directions. These lengths were then used as input in TIgenerator. However, the fact that ellipses can overlap implies that the final lengths of objects in TI2 may be different from the input parameter. Did the authors checked with the same algorithm that the statistics are identical on both TIs ? The normal procedure should to adapt the input length by trial and error until the output statistics fit with the targets computed with the same algorithms on TI1. More details are required to convince the reader that the inference has been done properly.

Finally, MPS simulations are very sensitive to input parameters. We can even consider that the values of the parameters (neighborhood, number of multigrids, synprocessing, etc.) are part of the random function model. This is not a detail and this information must be provided to ensure that the results are reproducible.

Detailed comments ——————

Abstract, lines 19, and 21 : what are the criteria used to define if a TI image is accept-

able or lead to better simulations. The text would be better if more explicit.

Page 2, line 27: Other authors did 3D MPS simulations from 2D TI images. For example:

Okabe, H. and M. J. Blunt (2005). "Pore space reconstruction using multiple-point statistics." Journal of Petroleum Science and Engineering 46(1-2): 121-137. Okabe, H. and M. J. Blunt (2007). "Pore space reconstruction of vuggy carbonates using microtomography and multiple-point statistics." Water Resources Research 43(12). Comunian, A., et al. (2012). "3D multiple-point statistics simulation using 2D training images." Computers & Geosciences 40(1): 49-65.

These methods are not always as complicated as the authors suggest.

Page 3, line 3, Is equifinality really analyzed in this paper ?

Page 5, line 4: “The critical step of sequential simulation is the conditional probability” – needs to be rephrased, “the conditional probability” is not a step.

Page 5, lines 5 to 14. The explanation of snesim needs to be clarified. The terminology is not described in proper order in the text.

Page 6, lines 8 to 10. This part is important. I suggest to extend the explanation. One key aspect is that Krishnan (2008) has shown that the value of tau depends on the values of the conditioning data as well as the patterns. In general it is not a constant. In practice, setting the value of tau is complex, and this technique is not always ensuring that the ensemble of simulations will respect the probability maps that are given as input. This is discussed in detail for example in Allard et al (2012) or Schaeben (2012). One thing that can be done is to adjust by trial and error the value of tau and check the quality of the results. Here, the value seems to have been set a priori. The point is slightly discussed later on but as the authors claim that this is one of the first application of MPS constrained by geophysical data, it would be good to provide more details. It would also be good to show probability maps to check that they are close to the target.

Allard, D., et al. (2012). "Probability Aggregation Methods in Geoscience." Mathematical Geosciences 44(5): 545-581. Krishnan, S. (2008). "The Tau model for data redundancy and information combination in earth sciences: theory and application." Mathematical Geosciences 40(6): 705-727. Schaeben, H. (2012), Comparison of Mathematical Methods of Potential Modeling. Mathematical Geosciences 44 1 : 101–129. doi:10.1007/s11004-011-9373-2.

Page 7, line 5, I suggest to rephrase the sentence. It is not clear what is a “geostatistical descriptive map”.

Page 8, lines 1-5. I think that the method is not correct. See General comments above.

Page 8, lines 7-19. The flow and transport model is not described accurately enough nor a citation is given to provide the details. This is not sufficient.

Page 8, line 24. What does it mean “head tends to be stable”. Please provide a more accurate statement.

Page 9, lines 23 and 24. The notation S1, S2, etc. should be introduced in the text and not only in Table 1.

Page 10, lines 20-29. A figure showing the probability maps and allowing to compare them with the target probability derived from the geophysical data would help understanding the results.

Page 12, lines 16-17. “Comparison of groundwater heads indicated that a training image with higher accuracy helped improving the model simulation”. What does “training image accuracy” means here?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 11829, 2013.