Review on the manuscript "Simulating Reservoir Lithologies by an Actively Conditioned Markov Chain Model"

(Invited for review on May 30, 2017 by Computational Geosciences; submitted on July 29, 2017)

In this manuscript, the authors suggested using a small tolerance angle for conditioning to the future state in the coupled Markov chain (CMC) model suggested by Elfeki (1996) and Elfeki and Dekking (2001) so that a "future" datum of the same state not exactly located on the same lattice row can be used to condition the simulation of a layer. Such a minor technical change is problematic. The case studies were conducted unreliably and the results were presented improperly. The manuscript was written in a very misleading manner with mistakes. I don't think it is suitable for publication.

MAJOR PROBLEMS:

1. The CMC model has been proved to have obvious defects both in simulation algorithm and in theory. The defects of the CMC model were also displayed more or less by Elfeki and Dekking (2001, 2005). The Markov chain random field (MCRF) model (Li 2007, Li and Zhang 2008) has explained and solved the small class underestimation problem. The value of the CMC model was recognized by Li and his coauthors as a pioneer study and contributor to the MCRF approach. I don't think a minor change on the previous simulation algorithm of the CMC model can bring more credit to the CMC model. On the contrary, hanging on the CMC model without admitting its defects and properly recognizing others' progress is not only misleading, but may damage its value as a pioneer study that inspired later researches. I believed that the CMC model inspired not just the research of Li and his coauthors in Markov chain geostatistics. However, it seems that only Li recognized the contribution of the CMC model to his research.

2. The authors presented the previous CMC model in the article text, but improperly presented the proposed A-CMC in an appendix. In the CMC model, chain A and chain B are two 1D Markov chains. But in Appendix A, the horizontal chain A in the A-CMC is not a 1D chain anymore, and 2D probability terms occur in chain A. First, while the two "future state" cells are so close in a well due to the small tolerance angle, how can they be independent of each other? According to the authors, in the A-CMC model, there is still only one future state to be used. However, the equation derivation in Appendix A considers two future states (see equations (4, 5, 7, 8, 9, 10, 11)) or even n multiple future states (see equation (16)). Is it a conflict? Second, the authors here derived the model of the horizontal chain A by following the derivation process of the single-chain-based MCRF model proposed by Li (2007), but did not understand why Li derived the MCRF model in that way and what kind of neighborhoods Li applied the conditional independence assumption to. If this is proper, then why did the authors here have to first define a CMC model in equation (4)? Isn't it simpler and more correct to directly derive the whole CMC model by following the derivation method of the MCRF model (then the CMC model becomes the MCRF model)?

3. The case studies were not conducted and described properly. In the first example (Book

Cliffs Model I), there are clear small class (clay) underestimation and major class (FS) overestimation, but the authors ignored them. The data of transition probability matrices provided in Table I have problems. Through calculating the equilibriums of transition probability matrices, I can easily get approximate class proportions. From the horizontal matrix, the class proportions can be obtained as: 12% (MS-non), 10% (FS-non), 33% (FS), 19% (VFS), 20% (SS), and 6% (Clay); but from the vertical matrix, they can be obtained as: 11% (MS-non), 4% (FS-non), 13% (FS), 43% (VFS), 11% (SS), and 17% (Clay). The class proportions from the two matrices should be similar. However, the differences for some classes here are too large to be proper.

Horizontal sampling interval = 25 m							Vertical sampling interval = 5 m						
Horizontal transition probability matrix							Vertical transition probability matrix						
State	1	2	3	4	5	6	State	1	2	3	4	5	6
1	0.9768	0.0001	0.0132	0.0066	0.0032	0.0001	1	0.7496	0.2500	0.0001	0.0001	0.0001	0.0001
2	0.0262	0.9734	0.0001	0.0001	0.0001	0.0001	2	0.0001	0.3333	0.0001	0.0001	0.6663	0.0001
3	0.0001	0.0001	0.9941	0.0001	0.0028	0.0028	3	0.1111	0.0001	0.6664	0.0001	0.2222	0.0001
4	0.0001	0.0001	0.0001	0.9841	0.0098	0.0058	4	0.0001	0.0001	0.1000	0.6997	0.0001	0.2000
5	0.0001	0.0132	0.0001	0.0022	0.9843	0.0001	5	0.1250	0.0001	0.0001	0.3750	0.4997	0.0001
6	0.0001	0.0001	0.0049	0.0294	0.0001	0.9654	6	0.0001	0.0001	0.0001	0.4996	0.0001	0.5000

Table 1. Input dataset for the Book Cliffs model I

4. The largest problem lies with the second example (Book Cliffs Model II). The proportions of the five classes are obviously different both in the original image (Fig. 9) and in the three wells (Fig. 10), and the minor class Clay-non can hardly be seen in the original image and even did not appear in the three wells. However, the parameters (i.e., transition probability matrices) provided in Table 2 mean thoroughly different things: (1) The data of the horizontal transition probability matrix imply that the five classes have the same (horizontal) autocorrelation and equal proportions (20% for each class). Assuming the data are stationary first-order Markovian, the horizontal autocorrelation range of each layer class can be estimated to be about 9km, much longer than the longest layer length, that is, the lateral extent of the simulation area - 5km (because the real data usually are not stationary first-order Markovian, the horizontal autocorrelation range for each layer class is longer than 9km). How much rationality is there for the authors to assume the hardly-seen minor class (Clay-non) has a lateral autocorrelation range of 9km and an area proportion of 20%? (2) The data of the vertical transition probability matrix imply that four classes have similar (vertical) autocorrelation ranges (about 20m) with different proportions and the minor class Clay-non has a relatively shorter (vertical) autocorrelation range (about 6m). In fact, the vertical autocorrelation range of the minor class should be close to 0. The proportions of the five classes implied by the vertical transition probability matrix are 33% (FS-non), 37% (VFS-non), 9% (SS-non), 0.2% (Clay-non), and 21% (Coal), rather than the 20% implied in the horizontal transition probability matrix. Because the horizontal correlation ranges (about 9km for all classes) are much longer than the vertical correlation ranges, the horizontal

transition probability matrix has a major control on simulated results. If the authors did not have used such "carefully chosen" irrational transition probability parameters, the minor class should have no chance to occur, and the simulated images should have been very different.

Horizontal sampling interval = 25 m					
Horizontal transition probability matrix					
State	1	2	3	4	5
1	0.9900	0.0025	0.0025	0.0025	0.0025
2	0.0025	0.9900	0.0025	0.0025	0.0025
3	0.0025	0.0025	0.9900	0.0025	0.0025
4	0.0025	0.0025	0.0025	0.9900	0.0025
5	0.0025	0.0025	0.0025	0.0025	0.9900
Vertical sampling interval = 0.4 m					
Vertical transition probability matrix					
State	1	2	3	4	5
1	0.9622	0.0236	0.0094	0.0024	0.0024
2	0.0100	0.9698	0.0001	0.0001	0.0200
3	0.0127	0.0318	0.9553	0.0001	0.0001
4	0.2500	0.2500	0.0001	0.4998	0.0001
5	0.0354	0.0001	0.0044	0.0001	0.9600

Table 2. Input Dataset for Book Cliffs model II

5. The above unreasonable and self-conflicted input parameters used in the study mean that the case study results in this manuscript are unreliable.

MINOR PROBLEMS: …..

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