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Interactive comment on "Automated Monte Carlo-based Quantification and Updating of Geological Uncertainty with Borehole Data (AutoBEL v1.0)" *by* Zhen Yin et al.

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General comments

This paper proposes an application of the "Bayesian Evidential Learning" approach to the problem of uncertainty assessment in integrated reservoir modeling. Although the general approach was previously described in Scheidt et al (2018), this paper contains significant new elements, applications and discussions, which are very interesting for the community.

In terms of form, the paper is very well written and clearly presented, apart from minor

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issues. It includes a link to a Jupyter notebook implementing the methodology and applying it to the reservoir thickness. The implementation works fine, after some twidling to install scikit-fmm. The overall structure of the code seems to allow for managing facies (some unused functions for facies modeling are present in the code), but the demo notebook assumes porosity to be 1 and water saturation to be zero. Even so, the reproducibility is much better than in most similar papers on this topic.

Overall, I congratulate the authors for the very interesting approach which represents a paradigm shift as compared to the current practice. I have, nonetheless, several comments, questions, and suggestions, which I hope will help to improve the paper. My recommendation is to proceed with minor/moderate revision.

Specific comments

- Overall, the introduction makes a good job introducing the general problem, but more precise explanations about the exact contributions of the paper would be welcome at this stage (in particular with regard to the other recent contributions of SCREF).
- The borehole data are generally at much higher resolution than the reservoir grid data. However, as in most reservoir modeling approaches, this paper assumes that the borehole data has been upscaled to grid resolution, a process which is source of inaccuracies in reservoir models. One of the points of the proposed approach is that the falsification step (Section 2.1.3) could in principle integrate the scale change. Comments on this would be welcome. Also, some additional precisions about the management of categorical variables during falsification would be welcome (in addition to the last sentence on page 5). From Eq. (8), it seems that the robust Mahalanobis distance accounts for spatial redundancy; please confirm.
- Overall, I am not fully clear about the falsification step. As this is a key aspect

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of the proposed methodology, it would be good if the authors could insist on the consequences of this approach as compared to the conventional method which creates models sampling exactly the borehole data. Errors between model and data may be acceptable for some applications such as hydrocarbon in place, as they will average out, but would they yield reliable forecasts of porous flow and transport if borehole data are not exactly honored by some model realizations?

- Although this is not the main point of the paper, some parameters for generating the prior models could be described more precisely and discussed. For example, I am a bit puzzled by uniform distributions taken for the facies 1,2,3, which suggests that facies 0 will adjust so that the total is equal to 1, which may be a source of bias (see Haas Formery, Math. Geol. 2002, or compositional data analysis literature). What are the variogram ranges for facies modeling? In the figure, there seems to be a facies trend, but what are the parameters of this trend? Are the variogram models isotropic? Is the variogram of porosity the same for all facies?

- Overall, I get the overall idea for facies, but I am not fully clear about the consequence of the facies processing. The signed distance is mentioned, and then the Truncated Gaussian. I first understood that the TG was used in the generation of prior models, and the signed distance for the workflow steps (which would mean in general 3 distance fields for 4 facies). But I was then puzzled by Fig 12 which suggests that maybe one single scalar field is used. So at the end, I am not sure about what was done exactly. Clarification of this would be needed in the paper.

- I looked at the code to try to understand the facies management, but it is not fully integrated in the high-level functions. Adding facies management in the code would improve reproducibility. If not possible, then please explicitly mention in Section 2.3 that the provided python code illustrates the workflow for thickness only.

- I have some doubts about the back transformation process when not enough

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PCs are retained. Some artifacts are visible in the realizations on Fig. 19 and on the poro/perm plots of Fig. 20 (breaking the consistency between petrophysical features within each facies). Could this also break the match to borehole data if not enough PCs were selected?

- As the aim of the paper is to “minimize the need for tuning parameters” it would be good to summarize the updated parameters as in Table 1 to help the reader assess to what extent the global model parameters have been updated by the process. I cannot help but wondering about how the updating of global parameters such as facies proportions or variogram range would compare to a classical process where statistical inference would be repeated by domain experts as new data become available.
- Performance: I assume the 45 minutes do not include the prior model generation? Please clarify.
- Discussion: the discussion in its current form highlights the main points of the method and some challenges, but does not really discuss some aspects which are often considered critical in subsurface models, such as the match of individual realizations to borehole data, or the preservation “geological consistency” such as the petrophysical distributions for various rock types. I suspect some moderate violations do not really matter for the accumulation problem considered in this paper, but I have more doubts about what would happen for modeling objectives involving highly non-linear physics, such as flow simulation. SOme balanced discussion on these aspects would probably be useful. Another question is whether there are any guidelines about the various sensitivity / confidence levels involved in the method, as these parameters likely impact the results.

Technical corrections

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In several places: the term "data-scientific" looks hype but I don't get the exact meaning. Please define or use another term.

Also, the term "constraint" used in several places could probably be replaced with more accurate terminology.

p.1, l.19: "A generalized synthetic data set motivated by a gas reservoir": please rephrase. Seems to me this is a gas reservoir study which has been simplified.

p.4, Eq. (3): Unless I am missing something, the notation could be simpler using χ_{sp}, χ_{gl}, etc.

p.6, l.10: Please define G (and make it bold?). Fix typos in Eq. (10): "proir" should read prior.

p.6, l.24-25: Please rephrase the sentence for more clarity. This is more an explanation about why it works in practice than an actual "truth".

p.7, l.1: Instead of "model grid cells", I'd suggest for generality: model parameters

p.7, l.3-6: I'd recommend to factually summarize the DGSA approach rather than summarizing its advantages (as compared to what?).

p.7, l.11-20: The notations of Eq 11 are unclear to me. I get the point of the sequential updating, but I am not sure it is correct to write "the posterior model of χ becomes prior model for ", as both variables are different. Maybe using the subscript such as $\chi_{posterior}$ in Eq (11) would help make the point clearer.

P.7, l. 21-25: PCA on an image can be achieved in a variety of manners. I suspect that in Figs. 1 and 2, the PCA factors are linear combinations of image columns, but please explain so that the reader does not have to guess.

P.7, l. 27: I'd suggest to use ψ_s, as it relates to the distance to facies s. I also think that $\mathbf{x}_\beta$ should be the closest location equal to (and not different from)s in the second term (otherwise, the definition enlarges the facies by 1 voxel). Or maybe just the distance to

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the boundary of facies s?

p.8, l. 7: Typo: the prior uncertainty models *have*

p.9, l.1-3: Please check convoluted sentence.

p.9, l.13: Did you mean “the reservoir rocks deposited at shallow marine environments”?

p.10, l.2: Confusion between in-place resources (GIIP) and recoverable reserves, which also depends on flow behavior.

p.10, l.12-13: Please check syntax.

P.10, Fig.5: a scale would be needed so that variogram ranges provided later in the paper can be related to model size and well spacing.

p.11, table 1: Typo in “gammy ray”

p.12, l.2: In addition to resolution, velocity is a significant source of thickness uncertainty.

p.13, l.10-15: h is the height above the free water level, not the reservoir depth.

p.13, l.19-20: Please fix sentence.

p.16, l.11: The independence between thickness and facies is stated as a fact. In my view, it is a working hypothesis (probably a reasonable one), but not a general truth.

p. 18, Fig.10: Would d_{obs} correspond to a line? I guess it should have some thickness due to data noise and to PCa projection.

p.19, l.1-2: “the uncertainty... their prior”: Please chech grammar.

p.19, Fig.11: Please tell what the dash lines correspond to (kernel density?)

p.20, Fig. 12: The visualization for posterior facies distributions gives a qualitative hint about what happens, but I am not sure about what we are exactly looking at. It is the

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blending of discrete color maps or the average of the underlying Gaussian field (but then, facies threshold change depending on facies proportions)?

Fig. 15: Typo in legend

p.25: Please remove mention to reserves, as no recovery factor is involved.

p.25, l.17: The cross-validation tests on wells 5 and 6 seems a bit optimistic, as vertical averaging on the 75 layers essentially amounts to making a two-dimensional model. Again, erors average out, so the reduction of uncertainty in such a case is no proof of the actual forecasting ability of the model in three dimensions.

p. 24, Fig. 20: The correlation coefficient is not really meaningful on such multi-modal distributions, even more so as the facies proportions likely change from one distribution to another. It would make more sense to examine the bivariate statistics per facies.

P.26, Fig. 23: please explain what we see: the curves are densities, but what does the point horizontal spread mean? And again, what is the facies value?

p.27 l.12, l.15, l.24 : "results an extreme fast computation"; "be able predict", "do not full match": Please fix English

p.28, L.11-21: This paragraph nicely explains the problem in simple terms, so I think it would be better integrated in the introduction than in the discussion.

p.28, L.16: this sentence mixes the falsification of parameters and falsification of a methodology, which I think are very different. I suggest rephrasing.

p.28, l.30: Not sure I understand the references in this context.

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