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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Modelling 3D permeability distribution in alluvial fans using facies architecture and geophysical acquisitions" *by* Lin Zhu et al.

Anonymous Referee #3

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1) Scientific Significance Does the manuscript represent a substantial contribution to scientific progress within the scope of this journal (substantial new concepts, ideas, methods, or data)?

Poor

Concepts, ideas, and methods are not new. The claim of an "original method" by the authors is unfounded. Every method used has been previously published and implemented: Dividing a domain into zones to do geostatistical modeling is not original; use of geophysical data to derive facies or hydraulic parameters is not original ; assumptions of "K distributions are local stationary" and computing the log10(K) semivariogram are decades-old concepts. The paper generally reads like documentation of a work as-

signment, not scientific progress.

2) Scientific Quality Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)?

Poor

In judging scientific quality, consider the scientific method: systematic observation, measurement, and experiment, and the formulation, testing, and modification of hypotheses.

Granted, the paper does implement some systematic observation and measurement and proceeds to set up an "experiment" of sorts by producing geostatistical realizations of hydraulic conductivity. However, it is not clear at all what the hypotheses of the paper are and what the "experiment" will actually be: A flow model or a transport model for what use? A calibration/validation exercise to what observations? The paper simply lacks scientific completeness in formulation and testing of hypotheses.

The authors seem to be advocating that an "original method" constitutes science or, perhaps, a hypothesis of being "original" is science, but even that is not necessarily true especially since the authors' claim of being "original" is debatable. The closest statement to a hypothesis I could find is given at the end of section 2.1:

"The characterization of the distribution and spatial variability of hydraulic conductivity is vital for an optimal use of the limited water resources in this area."

This statement isn't new or "original" except perhaps at the particular area of study in China. More importantly, this hypothesis is not tested in the paper! Instead, the paper is consumed with mundane documentation of its observations and methods and preparation of an experiment that is never executed. The paper could have tested whether its methods are actually "vital for an optimal use of the limited water resources in this area" (e.g. by flow or transport modeling with comparison to water level or chemistry data, i.e. observations). A scientific result would be proof that the author's methods are better than a typical effective-K model for determination of some "vital" information about the aquifer system. Perhaps the authors plan to do this in another paper, but that does not matter. The existing paper just does not constitute good science on its own.

From a hydrogeologic perspective, important qualities are lacking in the representation of alluvial fan heterogeneity: (1) there is no directional non-stationarity (e.g. no radial variability of the depositional major axis; no stratigraphic dip), (2) there are abrupt, unrealistic discontinuities between zone (e.g. facies occurrences abruptly terminate and the edge of a zone, like a fault), and (3) the zonal approach leads to unrealistic transitions in geometrical properties (e.g. thickness of gravel deposits). For all the claims of being an "original method" by combining different methods, the paper does not seem to pay close attention to methods of geology.

3) Presentation Quality Are the scientific results and conclusions presented in a clear, concise, and well structured way (number and quality of figures/tables, appropriate use of English language)?

Poor

Again, the paper is really lacking in actual scientific results (i.e. results of hypothesis testing). The paper is full of documentation of what was done to analyze data and make geostatistical realizations, including re-hashing of old methods with obvious weighting to referencing of the authors' previous publications. Even if the conclusion that "it is worth highlighting we depicted an original method..." were true, this does not constitute good science on its own. The claim of "Fusing multiple-source data" isn't necessarily science on its own since it is routine practice in the earth sciences.

Figures 5 & 6 were never referenced in the text. Figures 4-6 are difficult to interpret without labeling of y-axis units and use of variable scales in Figure 4 & 5. Discussion of dip and strike direction model parameters other than variance is lacking. Figure 7 has no scale. These are key elements to geostatistical modeling, yet this information

was poorly presented.

In terms of documenting what the authors did, the paper is a reasonable piece of communication of the caliber of an institutional report (which would need further revision in regard to Figure 4-7 as noted above and use of English language).

For final publication, the manuscript should be

Rejected

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