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

Lin Zhu et al.

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When reading the review from anonymous reviewer #3, it clearly arises that he did not like our contribution at all. He did not find even a single merit and just provided a long sequence of extremely negative judgments. For example, "claim . . . unfounded", "previously published", "not original", "not scientific progress", "lacks scientific completeness", "consumed with mundane documentation", "not constitute good science", "important qualities are lacking", and others. This made us astonished, also in the light of the generally positive comments provided by reviewers #1 and #2. Why such a tremendously severe evaluation? Except for a couple of specific issues given in comment 3 "Presentation Quality", all the other comments by reviewer #3 are extremely

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general and no suggestion is provided on how the work can be improved.

We strongly disagree with this reviewer and with this way of reviewing a manuscript. Hence, a detail rebuttal of the majority of reviewer #3 comments is given in the following.

1) Scientific Significance - Poor

Rev.#3 : 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 assignment, not scientific progress.

Response: This comment is a mixture of obviousness and general statements but the supposed lack of originality is not supported, for example, by specific references. We are obviously conscious that semivariogram, geophysical modelling to infer hydraulic parameters, etc. have been used since a long time. We do not claim originality for this. What is original is the way we integrate a large number of inexpensive and fast VES surveys (properly calibrated through a few more-detail investigations as well logs) with a facies model developed from borehole lithologic data to simulate the log10(K) continuous distribution in multiple-zone heterogeneous alluvial megafans. Moreover, also the application site is anything but worthless, as the Chaobai fan is used to supply the majority of the potable water to one of the most important city in the World. We have specified more clearly the novelty of our contribution in the abstract (lines 16-19) and Introduction (67-71).

2) Scientific Quality - Poor

Rev.#3 (a): 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 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.

Response: The major part of this comment is soaked with philosophical argumentations. For example, "The authors seem to be advocating that an "original method" constitutes science", or "the paper is consumed with mundane documentation of its observations and methods", and also "The paper could have tested whether its methods are actually vital for an optimal use of the limited water resources in this area". We think that a reply to such a type of comments is worthlessness. The only two technical aspects detectable in this part of the review are "what the hypotheses of the paper are" and "what the experiment will actually be". About the former, we have clearly stated in the abstract, introduction, and section 2.4.3 that local stationarity of log10(k) is the basic hypothesis of or investigation. The changes of the variance characterizing the composited semivariogram between the three zones support the utilization of the local-stationary assumption (lines 298-305). Concerning the latter, we have reported in the conclusive section how the outcome of this study will be used in the future: “This result provides valuable insights for understanding the spatial variations of hydraulic conductivity and setting-up groundwater flow, transport, and land subsidence models in alluvial fans” (lines 357-359). As hypothesized by the reviewer, we have planned performing these dynamic simulations in a next step. As supported by reviewers #1 and #2, we believe that the definition of the static model of a complex heterogeneous megafan as that of the Chaobai river is worth to be published.

Rev.#3 (b): 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).

Response: These are the only scientific inquiries in this review. Curiously, they are the same comments provided by Reviewer #1 in “General comments” #3 and #6. The reviewer can refer to the responses provided to Reviewer #1.

Rev.#3 (c): 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.

Response: Sorry, but we are not able to understand what are the “methods of geology”.

3) Presentation Quality - Poor

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Rev.#3 (a): 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.

Response: The same response provided to comment 2(a) holds also in this case. What means "The claim of Fusing multiple-source data isn't necessarily science"? We never claimed this! We simply wrote in the Introduction "Recently, data fusion techniques have been developed for coupled inversion of multi-source data to estimate K distributions for groundwater numerical modeling" and add a number of recent references where integration of different data are used to characterize hydrogeological systems. As written above, these are very general suggestions to which it is impossible to provide worthful replies.

Rev.#3 (b): Figures 5 & 6 were never referenced in the text.

Response: The references have been added (lines 268 and 293, respectively).

Rev.#3 (c): Figures 4-6 are difficult to interpret without labeling of y-axis units and use of variable scales in Figure 4 & 5.

Response: The unit of y-axis is already provided in Figure 4 ("Resistivity"), Figures 6 and 7 ("Variance") (previous Figure 4 and 5). Concerning these two latter, we preferred to put the axis units in the first sub-panel and omitted in the others for picture clarity. Concerning the "variable scale" in Figures 6 and 7, in the caption of Figure 6 we already reported "Notice that the range in the y-axis differs for sands and gravel lithology in Zone 2 and Zone 3". The choice is done to improve the clarity of the subpanels. We have updated the caption to "Notice that the range in the y-axis differs for gravel lithology" and add the same note in the caption of Figure 7 too.

Rev.#3 (d): Figure 7 has no scale.

Response: Updated

Rev.#3 (e): 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).

Response: We disagree with the reviewer. We believe that such a negative judgment cannot be based only on the couple of minor figure improvements suggested by this reviewer. Finally, similarly to all the other comments, a negative but extremely general sentence is given for the English form. Any specific example is suggested to improve the English form. Anyway, English language has been updated following the specific recommendations by reviewers #1 and #2.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-373/hess-2016-373-AC3-supplement.pdf

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