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Interactive comment on "Scale dependency of fractional flow dimension in a fractured formation" *by* Y.-C. Chang et al.

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Comment on Scale dependency of fractional flow dimension in a fractured formation by Y.-C. Chang, H.-D. Yeh, K.-F. Liang, and M.-C. T. Kuo

General considerations

In the manuscript the Authors apply the generalized radial flow (GRF) model developed by Barker [1988] in conjunction with the optimization method of simulated annealing (SA) to a data set obtained from aquifer tests performed in 1979 at the Chingshui geothermal field (CGF) in Taiwan. The scope is to investigate the hydrogeological properties of the fractured formation and to verify the scale dependency of flow.

The main problem is clearly stated by the Authors in the introduction at page 1989 "When analyzing data from the hydraulic test, it is difficult to choose an appropriate flow dimension in a fractured formation system." After a discussion of the literature and a brief description of the experimental site, they recall Barker's model [1988] and explain how the simulated annealing method is applied in conjunction with a generalized radial flow analytical solution to obtain from the drawdown data analysis both the hydraulic parameters and the proper dimensionality of the interpretative formula.

From the developed analysis the authors conclude that the joined generalized radial flow and simulated annealing approach "can successfully determine the flow dimension and hydrogeologic parameters for the CGF fractured formation" and "the flow dimension increases with the distance between the pumping well and the observation well" (page 2004).

While the topic is of manifest relevance for all those involved in fractured media pumping test analysis and an interesting approach is proposed to obtain a proper solution, I think that more efforts are required to demonstrate the feasibility and the reliability of the suggested method.

Specific comments

I agree with Scesi's comment and I found quite obscure the physical description of the site given in Section 2, mainly with reference to the spatial interaction of wells with joins and faults. The maps and the section reported in Figure from 1 to 3, as well as the information given in the text, are inadequate to understand the scale of the problem. In other words I'm unable to understand how (in a statistical sense) the boreholes are crossed by fractures and I think that this fact is of paramount relevance in the analysis. This is confirmed by the Authors themselves that at page 2000 state: "The GRF model is therefore applicable to the CGF because it is homogeneous and isotropic based on the field description and anisotropic analysis." The latter statement is based on the analysis of five sets of well combination (Table 2), but "the directions in sets 4 and 5 are

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inconsistent with the direction of prominent set of joints in Fig. 4" (page 2000) and for this reason only the results of sets from 1 to 3 are considered as valid. If the physical description can decide whether the results of the computations are to be accepted or rejected (I generally agree with this attitude), I think that it must be clear without any doubt. Moreover I suggest to give a simple recall of the Papadopulous method, being the "Proceeding of the Dubrovnik Symposium on the Hydrology of Fractured Rocks, International Association of Scientific Hydrology, 21–31, 1965" substantially unavailable to most the readers.

In any case, the aforementioned aspects were widely discussed in the comment by L. Scesi, thus I prefer to spend some words about the joined GRF and SA analysis.

All the results (dimensionality n, transmissivity T, and storativity S) are reported in Table 3, with the standard error of estimate (SEE) as a function of the distance between the pumping well and the observation well. From my point of view the results depend on: i) the choice of the objective function (eq. 6 of page 1997), and ii) the choice of the temperature reduction factor, assumed "constant and smaller than one" (page 1999).

With reference to the point i), I would like to see the effects related to a different formulation of the objective function, as an example by weighing in a different manner early and late time drawdown data and/or by defining as objective function the absolute value instead of the square of the difference between observed and predicted head. This may give an idea of the robustness of the results, which show great differences in the parameter n, T, and S even if the drawdown behavior is quite similar. This situation is shown in Fig. 5 (cases a, b, and d), where the estimated drawdown using a general GRF solution and the Theis (GRF with fixed n=2) solution are compared.

Moreover, as reported in "Press et al., Numerical Recipes, The Art of Scientific Computing, 2nd edition, 1992", the essence of the minimization process is slow cooling and, even at low temperature, there is a "chance for the analyzed system to get out of a local energy minimum in favor of finding a better, more global, one". The thermodynamic analogy is remarked by the Authors themselves, although they don't show any result of a (quite mandatory) sensitivity analysis about the temperature reduction factor. I suggest that this will be done to reasonably ensure about the minimum obtained with the SA method.

Other remarks

I'm unable to deduce from Fig. 1 the distances from the pumping well reported in Table 3.

Why do the Authors spend a large part of the paper to criticize the work by Le Borgne et al. [2004]? The results showed in that paper are not manifestly in contrast with those (quite obvious) presented here. If the scope is to demonstrate that the use of SA is better than the "graphical fitting procedure" adopted by La Borgne, I suggest that the Authors modify the Sections 4.1 by enhancing in the discussion the differences that can be obtained by means of their and other approaches. In this case an application of a different procedure (the graphical fitting?) on their own data is also required.

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