

## ***Interactive comment on “Multi-objective calibration of a distributed hydrological model (WetSpa) using a genetic algorithm” by M. Shafii and F. De Smedt***

### **Anonymous Referee #2**

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In this paper, two methods are compared for calibrating a hydrologic model, *i.e.* a genetic algorithm (NSGA-II) and the classical PEST. Therefore, the WetSpa model was calibrated using discharge data from the Hornad River. For this distributed model, 11 parameters were selected for calibration. During this calibration three indices are minimized, *i.e.* CR1 (which is linked to bias, but which is wrongly called bias in the paper!), CR2 (Nash and Sutcliffe) and CR3.

The paper discusses the problem as being a Pareto problem, however, I do not see CR1, CR2, CR3 as a set of mutually contradicting objectives. *I.e.*, I doubt that minimizing CR1 will result in increasing CR2 or CR3 and vice versa! Therefore I doubt that

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the term Pareto front and Pareto optimal are correctly used! I believe the whole problem should better be described in the framework of equifinality! (also the left panels in figure 3 are not really convincing me of being a pareto front!)

I believe that paper would merit from better describing the different techniques used (i.e. NSGA-II and PEST), mainly focussing on their differences: a non-linear and global search algorithm versus a linear local search technique is quite different. Furthermore, the comparative study that has been conducted is not really fair as NSGA-II is able to use multiple criteria, whereas PEST only uses one. Therefore, it would be best to compare both techniques in an exercise where only CR2 is minimized. Furthermore, the results obtained should be better compared to other compative studies that have been published (e.g. Tang et al., HESS, 10, 289-307,2006; Wöhling et al., Soil Sci. Soc. Am. J., 72, 305-319, 2008).

The paper also concludes that NSGA-II better performs than PEST, however from the text, this conclusion is not really convincing. This should be demonstrated in better detail. The discussion that tries to demonstrate this is rather weak, and some strange statements are made: e.g. "the solution with PEST is not Pareto optimal", it cannot be Pareto optimal since it only uses one criterium in for calibration.

I believe the paper could be much improved by discussing more in depth the techniques used and by mentioning what type of minimization technique (global vs local) they are and what effect this will have on the calibration exercise! (e.g. since PEST is a local search algorithm, it should be repeated several times with different initial parameter sets). Furthermore, an in-depth discussion with respect to other papers that compare similar calibration procedures would definitely be beneficial to the paper. I believe, the current length of the paper definitely allows to elaborate more on these issues.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 243, 2009.

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