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Hydrology and Earth System Sciences Discussions

Interactive comment on "Bi-criteria evaluation of MIKE SHE model for a forested watershed on South Carolina coastal plain" *by* Z. Dai et al.

Anonymous Referee #2

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General comments

The manuscript presents (in a relatively long extent) some results of the application of the MIKE SHE model on the data of a forested (flat) watershed, located at the South Carolina coastal plain. Although the model performance is rather acceptable, particularly owing to the great amount of data from the small experimental watershed, there are some points in the current research that are somewhat weak, as indicated in the forthcoming paragraphs.

The modelling protocol that has been applied is very basic and although the authors (and the title of the manuscript) claim that a special evaluation has been carried out, previous studies (see for instance Refsgaard, 1997; Vázquez et al., 2002; Madsen, 2003; Vázquez et al., 2008; 2009; Blasone et al., 2008, ...) have already addressed

the evaluation approach described in the paper. Moreover, the sensitivity analysis presented in the current paper is very trivial. More complex sensitivity analyses, more appropriate to the intrinsic spatial distributed nature of MIKE SHE, have been carried out in previous publications (Vázquez et al., 2002; Vázquez and Feyen, 2003; Vázquez et al., 2008), including other model performance evaluation test such as the multi-site test.

Apparently, the authors were not aware of previous publications on the application of MIKE SHE, including one that was published in the HESS journal some years ago (Feyen et al., 2000) when they carried out their reported research.

Thus, for further publishing consideration, I believe that the manuscript should be carefully enhanced to include different modelling topics than what is just mentioned in its current form. Consequently, I recommend that this article is refused for publication, in its current status. Nevertheless, it is up to the better criteria of the Editor to decide on whether a second deeper review should be encouraged or whether this manuscript should be directly rejected.

Specific comments

In case that the Editor decides that a second deeper revision is needed for publication, I am pasting in below some specific comments that should be carefully addressed before a re-review of the manuscript takes place.

P 2 L 15: Equifinality is due not only to over parameterisation but also to various uncertainties in the modelling.

P 2 L 16: Uncertainties are not only due to parameter variability!!.

P 3 L 19-21: This is not correct. The authors cited in their manuscript the reference Vázquez et al. (2008). This study (for instance) deals with the simultaneous consideration of discharge and water table for model calibration and evaluation. Apparently the authors are not aware of previous studies such as Madsen (2003), Vázquez and Feyen

(2003) that considered two system variables for model calibration and evaluation.

P 6 L 6-9: This paragraph should be moved to section 23. The MIKE SHE-MIKE11 assemblage was not done in this study. This study has used this already existing model assemblage. Please rephrase this paragraph.

P 6 L 13: Don't you believe that this resolution is perhaps too coarse for such well monitored and small catchment?. Please elaborate further on your choice for the modelling resolution.

P 7 L 1-4: Please elaborate further on your groundwater model. Until which depth did you consider the soil layer to span?. From which depth the geological material start?. How many aquifers did you model?. The aquifer(s) is(are) confined/unconfined?. Upon the latter, how did you define the hydrologic parameter values? (i.e. Sy and Ss are generally not needed simultaneously!!), etc....

P 7 L 5-14: Drainage parameter values are needed in MIKE SHE to account for the presence of drainage systems in the modelled catchment. Otherwise their use becomes merely an artificial way of getting good hydrograph predictions!!. Which drainage system are you modelling?. No well founded reason is given in your manuscript to initialise a physically based model like the current one with a drainage depth of 50 cm!!, etc....

P 7 L 18-25: I do not like that in a section (2.4) that is supposed to deal with the model parameterisation, information that is given in the manual of MIKE SHE is placed. No indication is given at all about parameterisation of the overland module!!. What was calibrated and was not?. Further, nothing is said on the MIKE 11 set-up, what about boundary conditions of intermittent and perennial streams?, what about initial conditions?, description of cross sections?, etc....

P 8 L 17-20: The reason for assuming identical surface and groundwater catchments sounds to me a bit speculative and as such not convincing. Would you please further

comment on this issue?.

P 8 L 28-29: This is not true. Please see previous related comment (P 3 L 19-21).

P 8 L 29-30: I do not quite well understand this part. How can you calibrate a model through a sensitivity analysis?. Would you please elaborate further on this?.

P 9 L 10-15: I do not understand your choice of performance statistics. Why did you choose R2?. It is a rather bad choice because it is too oversensitive to peak values (see for instance Legates and McCabe, 1999; Vázquez et al., 2002). Further, in Vázquez et al. (2008), a reference that you have consulted, a relationship between E and RMSE is addressed. As such E and RMSE are measuring the same type of error information (i.e. systematic and random; see Vázquez et al., 2008). Thus you should have used either of them but not both of them!!. Moreover, how were mathematically calculated R2 and RMSE?.

P 9 L 22-24: But on pages 6 and 7 you just mentioned the opposite: Kx affects "signikficantly" several flow types. The same was mentioned about Ky. Please clarify on this matter.

P 10 L 13-17: Because this seems to be a very critical modelling factor, I think that a figure comparing the topography of the study site and the spatial distribution of detention storage should be given!!.

P 10 L 23: Shouldn't RMSE have units of measure?. Again, how did you calculate this statistic?, like a relative RMSE (i.e. RRMSE)?.

P 11 L 3-10: Does it agree with the drainage system of the modelled watershed?.

P 11 L 11: I assume that you mean the Striker "coefficient"?.

P 12 L 7-19: These conclusions have already been given in previous publications (se for instance, Refsgaard, 1997; Feyen et al., 2000; Vázquez et al., 2002; Vázquez and Feyen, 2003, etc. . .).

P 13 L 17-20: This problem has been also reported several times for larger catchments (Feyen et al., 2000; Vázquez et al., 2002;. . ..).

P 13 L 21: I do not quite understand why you have done regression analysis between the observed and simulated water table values?. One thing is to use R2 as a (bad) performance statistic, but another completely different thing is to carry out a regression analysis!. To have a good correlation between these variables does not mean to have a good model prediction (see for instance Vázquez et al., 2002).

P 14 "Conclusions": Several of the previous comments are applicable in this section. . ..

P2 L 1: "MIKE SHE could be a good. . ." rather than "MIKE SHE should a good. . ."

P2 L 31: ". However" rather than ".However"

P4 L 9: "2000;" rather than "2000,"

P4 L 11: "1994;" rather than "1994 and"

P4 L 15 (and in many other parts within the manuscript): Please use consistent units-notation. For instance rather than using cm/cm please use cm cm-1, etc. In some parts of the manuscript one notation type is used while in the remaining part of the manuscript the second notation type is used. . . Please cross check well (page 11, tables 2, 3. . .., captions of figs. 3a, 3b, ... for instance).

P5 L 13: "depth are" rather than "depth were"

P10 L 14: "streamflow" rather than "stromflow"

P17 L 13: dot is missing at the end of the line

P18 L 28: "Vázquez R.F." rather than "Vázquez R."

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