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Hydrology and Earth System Sciences Discussions

Interactive comment on "Phreatic surface fluctuations within the tropical floodplain paddy field of the Yom River, Thailand" *by* S. Chuenchooklin et al.

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GENERAL COMMENTS

Rationale and Performance

1. The authors of this paper apply the water balance model to a very flat tropical floodplain (mean slope = 0.14 per mil). They describe the problem of lack of water during the dry season because of the high demand on paddy fields resulting in decreasing phreatic water tables. They stress the importance of this investigation for finding acceptable solutions for this problem.

However, I miss a clear rationale of this paper. It presents a simple water budget model without clear approval for the assumptions being made. The main part consists of the comparison between observed data of groundwater and river stage, often without

relation to the modeling outcome. On the other hand, there are no recommendations given for the regional water management. Therefore, The purpose of the paper must be clarified, and then this aim must consequently worked out.

Additionally, the paper remains mainly descriptive, there are no real clear conclusions, but this might have to do with the main focus of the paper, which was not totally clear to me.

Water Balance Model

2. In my opinion, the water balance model should be the core of this paper. Therefore it is necessary to describe the single components and fluxes properly. Figure 7 should illustrate the model somehow, but the text does not deeply refer to it. Many points remain unclear to me, especially the model for the unflooded case.

3. Intersection of Thiessen polygons with flooded respectively unflooded areas: How are partly flooded Thiessen polygons treated in the balance model? Were the areas intersected by overlaying the flooded area with the polygons resulting in a split of the polygons into two? This would be the right approach in my opinion, but unfortunately I could not find anything about this procedure in the paper.

4. Any balance model is based on the fact that the fluxes throughout the system's boundary are known. In our case, the area is so flat that it cannot be said a priori that the surface and subsurface water divides or catchments are identical. It might be true that the lateral groundwater fluxes are negligible, but I miss the proof of indications for this assumption.

5. I cannot find any validation for the model. Errors can be based on the modeling structure itself or on the estimation or determination of parameters. One important testing is weather the balance holds or not. As I can oversee the implementation and application of the water balance model, all state variables or fluxes are either measured or calculated. Therefore, the difference in the balance must be able to be determined.

HESSD

2, S367–S373, 2005

But maybe, some parameters were fit in order to close this balance, but in this case, it should be indicated in the paper.

6. Another way to judge the uncertainty of the modeling results is a sensitivity analysis of the parameters. I would suggest to carry out some of this analysis for the most relevant parameters.

Infiltration Experiments

7. The small infiltration rings can be representative only for a very limited area because of the heterogeneity in the soil. Chuenchooklin et al. (2004, see reference list in paper) give values in a range between 0.27 and 2341.2 mm/d, which underlines this statement. Besides this, lateral components of the infiltrated water result in an overestimation of the infiltration rate, if not the soil in all layers within a larger area around the infiltration rings is saturated during the time of the experiment. With only 49 infiltration experiments over an area of 153 km2, the representativeness of the results must be questioned.

Furthermore, in the applied water balance model it is assumed that the same amount of the measured infiltration rate/flux at the point contributes to the regional aquifer by only vertical fluxes. As long as not the whole subsurface layer is saturated, it cannot be excluded that lateral fluxes of the water lead to significant changes in the vertical fluxes with depth, so that the point fluxes need not to be identical with the regional fluxes. This problem is inherent in the balance model and leads to a simplification which is ok, but it should be mentioned or addressed in the paper.

Instantaneous Unit Hydrograph

8. The authors used the method of Snyder in order to determine the shape of the unit hydrograph. This method was developed in the Appalachian highlands (Chow et al, 1988, p. 224, see reference list in the paper), which can topographically not be compared with the very flat floodplain in the case study. Slopes, river network

HESSD

2, S367–S373, 2005

structure, and geometry as well as connectivity of the floodplain or catchment area with the river network must be assumed to be very different. I miss some verification that this model structure holds. If there are not enough data within the study area themselves, the authors could refer to comparable study sites, where Snyder's method for the determination of the unit hydrograph could have been applied successfully.

Language

9. The English must be revised, preferably by a native speaker. The text is partly difficult to understand or sometimes even not clear due to language errors.

Figures

10. The figures are basically ok with respect to their content. The fonts could however be harmonized.

Final Remarks

11. The approach of the investigation is interesting. The "environment" of the study area is hydrologically difficult to model because of the flat area, vast inundations and the size of the area. Accurate modeling would imply sophisticated numerical modeling, which is not trivial in such a big area with a high surface / subsurface river /impoundment / groundwater interaction. There is therefore a need for the development, application, and testing of simplified models such as the water balance model used in the study.

The comments above lead me to the suggestion to revise this paper again. Nevertheless, I really would like to encourage the authors to do so, since the approach is well worth to be applied, and the data seem to be sufficient in terms of quantity and quality. It would be a pity not to publish. In my opinion, the revision requires mainly: 1. clear separation of the modeling strategy and water managements issues (there might be even enough material for two papers after some additional elaboration), 2. a better description of the model and of some assumptions made behind, 3. some more testing

HESSD

2, S367–S373, 2005

or validation of the model, 4. a clear conclusion (which might come out by itself after having considered the suggested issues).

SPECIFIC COMMENTS

to issues in order of occurrence in the text:

"Material and methods: topographical map and conceptual model":

1. The heading is misleading. I cannot see any description of a conceptual model.

2. "[...] contour-lines were constructed based on available data [...]" What kind of data were available to draw the contour lines? There is no specification, only the source of data is described in the paper. Were they precise enough in this very flat area?

"Material and methods: Model description and existing parameters":

3. "The losses by infiltration (I)" Infiltration cannot be considered as a loss, since infiltrated water remains within the system's boundary.

4. "effective rainfall (Pe)" I miss a clear definition of effective rainfall. In most cases in hydrology, this means the amount of rainfall, which does not infiltrate and becomes surface (or at least fast response) runoff. In our context, this definition would not make sense.

5. "actual potential infiltration capacity" This is a contradiction: either actual infiltration rate or potential infiltration capacity.

6. "hydraulic conductivity (K)" I assume that vertical hydraulic conductivity is meant. Measured by infiltration experiments or otherwise?

7. Equation (1) Why does Q(Pumping wells) not appear in the balance equation as it appears in figure 6? Why does the leakage between river and groundwater through the river bed not occur?

8. Explanation to eq. (1): "Delta S is the storage volume" Considering fig. 6, it is not

HESSD

2, S367–S373, 2005

clear, where the storage occurs in the case of non-flooding, since the boundary of the control block goes below the groundwater table.

9. "The total amount of infiltrated water [...] will be recharged to the unconfined aquifer and increased phreatic surface level during inundated period" At the beginning of the inundation there are probably unsaturated conditions. Is there a significant delay of the wetting front until infiltrated water reaches the phreatic surface? If yes, is this considered in the model?

10. Thiessen polygons for estimating rainfall There are only 6 rain gauges, and it is a tropical climate with often very local convective storms. The error for the areal precipitation might therefore relatively high. It is not a reproach for the experimental setup, but this problem should be mentioned, and its consequences maybe evaluated in the paper.

11. "To estimate lateral inflow [...], t h e method of ungauged basin with lack of rainfall and stream flow data was applied using the neighboring catchment's data of topographical and watershed characteristics." There is a whole hydrological decade of PUB, this means there are many different methods. It must be specified, which one has been used.

12. Figure 7: This needs more explanation. For example, What role does Vc play? Why does equation (1) no longer occur? What is the difference between P and Pe? What, if there are no crops? There are even more points, which are not completely clear to me. Regarding the text this flow chart holds for non-flooding conditions. How does it look like in case of a flood?

13. "The change of phreatic surface levels in subsurface water part (Fig. 7) would be compared to the computed recharge over the effective porosity" I could not found this illustrated in figure 7. Probably it should refer to another figure.

"Results and discussions":

HESSD

2, S367–S373, 2005

14. "Those trend lines in Fig. 16" I would not call them "trend lines". This might be misleading.

15. Table 1: The precision of the numbers in this table is too high.

16. Fig. 8: Many isolated infiltration values can be seen. This supports the hypothesis that the number of measured points is not sufficient with respect to the high spatial variability. Additionally, why is the variation of Ac much lower here compared to the data of the infiltrations experiments in Chuenchooklin et al. (2004, see reference list in paper)?

17. Fig. 9: Is Y17 the same as Phaitapho as it was referred to in the text above?

18. Fig. 10: How could the total runoff volume been calculated under flood conditions where bank overtopping occurs and a lot of the water is ponding the extended floodplain? Do the authors know something about the flow velocities there?

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 731, 2005.