Hydrol. Earth Syst. Sci. Discuss., 9, C5734–C5740, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C5734/2012/

Hydrology and Earth System Sciences Discussions

Interactive comment on "A critical assessment of simple recharge models: application to the UK Chalk" *by* A. M. Ireson and A. P. Butler

Anonymous Referee #1

Received and published: 10 December 2012

The manuscript of Ireson and Butler (2012) presents an interesting methodology to emulate with more conceptual less computational intensive hydrological models recharge fluxes with the knowledge of a physically based complex model, in a study site dominated by geological layers of chalk. It is an innovative idea and concept which could be helpful in many different other modelling studies. It is not a completely new idea and the authors should include comparable studies, where similar approaches were established. E.g. Lee et al. (2007) have presented a methodology of a combination a physical based hydrological model and an REW model. They present the three different model concepts; especially the Richards model is markedly well explained. At the end to conclude the simple models were not able to simulate the process on the one hand because there is no correlation between storage and recharge and on the other hand the model gives the right answer for the wrong reason. The developed bucket model was not able to realise simulations of low recharge on a small temporal resolution. Small recharge fluxes of the matrix cannot be represented and bypass is compensating that. The solution of using a larger time scale, leads to set up which still slightly overestimated low recharge rates. In the conclusions they formulate the idea to test assumptions of their models similar Weiler and McDonnell (2005) and Graeff et al. (2009). To fulfil their objective they still have to adapt their models to represent at least the processes which were defined by the Richards model. In a revised manuscript they have to analyse carefully the results of the complex model and then establish simplifications which are able to represent the data sets similar to Fenicia et al. (2006, 2008).

The manuscript in current version is in some chapters too long and pregnancy is missing. The information content should be reduced to the real essential parts, see specific comments. The authors tend to mix different terminology which makes it hard to follow their argumentation, e.g. preferential recharge and fracture recharge. The abstract needs modification. In the introduction the focus is concentrating on chalk aquifer hydrology, which should be extended to other geological formations.

Chapter 2.1: It is not easy to understand what kind of data they have and for what it is used as observation points or as boundary conditions. In a revised version they should precisely clarify these facts. Vegetation, soil, climatologically boundary and more than geological layer information would be helpful to understand what kind of catchment they present. Under the aspect how the authors have chosen there parameterisation these information is essential.

Chapter 2.2: The manuscript has to be stand alone. Parameter values of the complex model should be presented. Essential information about spatial discretisation of the complex model is missing. In all their models they have not taken into account interception losses. Which are on the long term time scale an important process (e.g. Gerrits et al., 2007).

HESSD

9, C5734–C5740, 2012

Chapter 3.1: The use of the discussed Duffy approach is unclear. The approach is to develop a volume weighted balance model and not a linear regression model. As far as I have understood, they were trying to find linear correlations between unsaturated storage and flux and between root water uptake and actual ET including additional autoregressive terms, in the first case without success. That there is no relationship between storage and flux is not reasonable and should be discussed carefully. It looks like the Richards model is not able to represent the unsaturated zone. I would guess a nonlinear relationship between the S and R. For estimating the relationship they should take as well GLMS (for example Crawley, 2002, 2007; Francke et al. 2008) into account. Finding a solution of ET is not in the scope of the paper. Even without that part the complexity of the manuscript is hard to follow. The authors should focus on how to simplify a complex Richards model with the concept of Duffy and Rushton. And 3.1 helps additionally to analyse the more physical based model if it produce the right fluxes and to which amount. Here the dominance of fracture flux which is I would guess preferential flow in the unsaturated zone and the strange relationship between unsaturated zone storage and flux needs some critical discussion.

Chapter 3.2 presents the bucket model concept after Rushton. They used the parameter permanent wilting point which is a measurable value here presented in a length unit. To link it to physical measurable values they should present it in volume percent which then can be scaled to a well-defined soil column into length unit. In that case their model would have a more physical relationship. They have to add porosity to define the size of the storage. The model was not able to reproduce slow percolation. Here an exponential function like in linear storage models could be an alternative solution, with the drawback that another parameter has to be estimated and calibrated. The selected range of the parameter space for PWP does not look like it has any soil physical background and the best values around 2000 mm makes no physical sense.

Specific comments:

P12066, L13: incorrect citation, “and Loague” is missing.

P12066, L17-26: The authors have effective 4 piezometers which they can use, with Compton at the food of the hillslope close to the Pang River as mentioned in chapter 2.4 treated as a boundary condition. That should be mentioned in the study site description to give the reader a clearer image of their set up.

P12066, L23-26: It is hard to follow the study site description by only getting geological layer information. Delete the part of the not used piezometer. It is not helpful by understanding the complexity of the manuscript.

P12067, L8-10: The authors should explain soil physical differences in lower and middle chalk in a table. Explain the soil layer and its physics. Information of dominating vegetation cover at the study site and climatic statistics would be helpful.

P12069, L12: Write ODE 15S in capital letters.

P12069, L5-7: Diersch and Perrochet (1997) mentioned the numerical instability of the head based solution. Small time steps have to be used to avoid that but still initial conditions and drastic parameters could lead to numerical errors. The authors should clarify that under their assumptions such a problematic can be avoided.

P12070, L2-12: Here the authors explain that their assumption is suitable for non wet conditions and discussing that for a two years period which is not defined in the paper but should at least include 2007 in the paper preferential bypassing is negligible. The argumentation is too long and can be shorten to the main point. But they are working with the period 1970-2000 and for that period they have to clarify that the process is unimportant. In the context of Fig. 5 the unimportance of preferential recharge which is identical with fracture recharge is not clear. Fracture recharge dominates total recharge tremendously.

P12070, L13: Parameter values should be presented in the paper.

P12071, L16-17: Unclear, if the authors use a 1 mm/d flux BC the expanding and contracting process will not be taken into account and the headwater is just a constant tributary to the simulated hillslope.

P12072, L4-11: Too long, the block can be reduced to the main facts.

P12072, L23-24: Which kind of vegetation is here underlying?

P12073, L15: Without knowledge of the dominating vegetation it is not possible to understand if that unique value of 0.2 m is comparable to observed phenological properties or only a parameter which gave the best results.

P12074, L12: Add quality measure like RMSE or Nash

P1075, L25-27-P1076, L1-2: It is not clear if they have coupled the Boussinesq model as well with the linear model or only with the Rushton model.

P12076, L17: ODE 15S should be written in capital letters. Which Bousinesq model is here presented, the approach with Rushton or Duffy? Have to be clarified.

P12077, L13-15: As far as I understood the authors the balance equation is U-Pe-R = 0 but it should be dU/dt-Pe-R = 0. It is not quit reasonable that there is no relationship between recharge and unsaturated soil storage. That fracture recharge which would be better explained as preferential flow has no correlation could be true. But matrix recharge should have a correlation to the storage. The authors should explain that carefully. The study site is not dominated by ET so that cannot be the reason.

P12077, L17: How have the authors separated the unsaturated zone into different storages, as a function of time or as a constant storage?

P12078, L18-20: A description of the model concept has to be added to Fig. 7. The figure pretends that the unsaturated zone is separated into two storages a plant available storage and a drainable storage which are not interacting. The authors have to explain all variables and should avoid changing variable names. I and M in Fig. 7 is not defined in the table. Add TH in Fig. 7.

P12080, L25- P12081, L4: Move to discussion, but be careful in interpretation of saturation deficit of a bucket model and linking it to soil physics especially by using PWP with no link to soil physics. Please, use one classification MA or SMD and not both.

P12082, L25 - P12083, L13: Move that block into the introduction or method part.

P12085, L7: Adding here climate change is tautologous. Maybe better to add that dry and wet conditions will be increase under climate change.

Fig.1: contains too much information. And the important part is hard to understand. Reduce information content and size in a) and enlarge b) and c). Explain in the caption the used abbreviations.

Fig.3: Plot the data with two ordinates P and AET on one side and recharge on the other to increase readability.

Fig. 8c): Typo: BP should be BF and RC-PWP PWP-RC

References:

Crawley, MJ.: Statistical Computing: An Introduction to Data Analysis using S-Plus. John Wiley and Sons, West Sussex, 761, 2002.

Crawley, MJ.: The R-book. John Wiley and Sons, West Sussex, 951, 2007.

Diersch, H.J.G., Perrochet, P.: On the primary variable switching technique for simulating unsaturated-saturated flows, Advances in Water Resources 23(1): 25–55, 1997.

Fenicia, F., Savenije, H.H.G., Matgen, P., Pfister, L., Is the groundwater reservoir linear? Learning from data in hydrological modeling, Hydrol. Earth Syst. Sci., 10, 139–150, 2006.

Fenicia, F., Savenije, H.H.G., Matgen, P., Pfister, L., Understanding catchment behavior through stepwise model concept improvement, WATER RESOURCES RESEARCH, 44, W01402, doi:10.1029/2006WR005563, 2008.

Francke, T, López-Tarazón, JA, Schröder, B.: Estimation of suspended sediment

HESSD

9, C5734–C5740, 2012

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

concentration and yield using linear models, random forests and quantile regression forests, Hydrological Processes, 22(25): 4892-4904. DOI: 10.1002/hyp.7110, 2008.

Gerrits, A.M.J.; Savenije, H.H.G.; Hoffmann, L. Pfister, L.: New technique to measure forest floor interception - an application in a beech forest in Luxembourg, Hydrol. Earth Syst. Sci., 11(2), 695-701, 2007.

Graeff, T., Zehe, E., Reusser, D., Lück, E., Schröder, B., Bronstert, A., Process identification through model rejection in a mid mountainous rural catchment I: observations of rainfall-runoff response and geophysical conditions, Hydrological Processes, 23(5), 702-718, 2009.

Lee, H., Zehe, E., and Sivapalan, M.: Predictions of rainfall-runoff response and soil moisture dynamics in a microscale catchment using the CREW model, Hydrol. Earth Syst. Sci., 11, 819–849, 2007.

Weiler, M. and McDonnell, J.J., Virtual experiments: a new approach for improving process conceptualization in hillslope hydrology, J. Hydrol., 285, 3-18, 2004.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 12061, 2012.