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Hydrology and Earth System Sciences Discussions

Interactive comment on "A daily water balance model for representing streamflow generation process following land use change" *by* M. A. Bari and K. R. J. Smettem

Anonymous Referee #1

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General comments

The paper contains some interesting ideas and the authors succeeded in the very difficult task of simulating reasonably well a basin's runoff behaviour before and after forest clearing took place. However, it is difficult to judge the scientific merit of the paper at this stage because the presentation is confusing. The title of the paper suggests that the model has been set up in a way to allow for land use changes to be taken into account. But it was not clear to me how the authors actually proceeded. Among the large amount of model parameters, which ones have to be modified after a clearing takes place? Only the areal fraction of forest and cropland as Fig. 3 seems to suggest? Furthermore, I think that forest clearing is a very particular case of land use change. How does your model take into account other types of land use changes? Hence, I am

not sure that the title is well chosen and I think it is misleading as most of the paper is about the presentation of a new model structure. The link between the new model structure and different types of land use should be explained in more detail. The paper is very much focused on the presentation of model components that, in my opinion, have been extensively used in similar studies in the past. Basically, this paper seems to be a follow up of a similar paper by the same authors. They succeed in demonstrating that some significant improvements of a previously described model have been achieved. Unfortunately, having not read this initial paper, I found it sometimes very hard to understand the model development. I have the feeling that many details have been skipped that would have been necessary to fully understand this paper. The description of the calibration procedure is a point in case. What performance measures did you use? How did you consider the groundwater data and streamflow data during model calibration? Which parameters could you estimate a priori? A major handicap is in the quality of illustrations and figure captions. Some of the illustrations (e.g. Fig. 6 & Fig. 7 unclear without reading the text) and equations (e.g. 6a & 6b) are very difficult to understand, because the abbreviations are not explained (or are explained later in the text). Also the way the results are presented is disappointing. There is no extensive discussion on the uncertainties of the model predictions. Near the end, the authors mention that the rainfall data is very doubtful but this does not seem to have had any particular consequences.

The number of parameters of your model is extremely high which gives a rather high flexibility to your model. While it is not surprising that the model performs reasonably well under these conditions, it must be expected that the robustness of the model is very low. Unfortunately the lack of parameter sensitivity analysis makes it difficult to assess the robustness of the model. Other parameters were apparently fixed beforehand but how can the reader know how reliable these etimates are? Additionally, ground measurements were only sparsely used to verify the accuracy of the internal state variables (e.g. soil moisture). If you want to predict the impact of land use changes you want to be sure that the internal states are well simulated.

HESSD

2, S396–S401, 2005

Specific comments:

-P. 822 - p. 824 the introduction is very much focused on the study area and the previous research that has been performed in this region. I would prefer if it would put the present study into a more general context. You could further develop the downward approach in model building. After having read the introduction, it remains hard to say what the contribution to the scientific community will be (i.e. what is the interesting/innovative part for people not living in Australia).

-at the bottom of page 824, the authors state that “the model is capable of reproducing streamflow generation processes following land use change with a small number of parameters that retain physical meaning”. This concluding sentence should not be put into the introduction of the paper because at this stage no data has been shown to prove the author’s statement. Instead, it could be written that this is one of the objectives of the underlying work.

-P. 824 how can you make a distinction between model complexity and model structure? On p.826 you mention that you introduced additional complexity into the model by changing the model structure...

-P. 825 the two catchment areas should be given in this paragraph as well as some information on the topography

-P.825 “both catchments were instrumented to measure water balance”. It would be helpful to get more information on the equipment you used (e.g.what kind of equipment did you use to calculate evapotranspiration, how many raingauges did you consider, what other equipment has been installedĚ). What data series do you have over what time period (do you have data before and after the clearing)?

-P.825 you mean that following the clearing, the groundwater rose by more than 15 meters!? It would be interesting to have a figure that shows this dramatic rising by the water table. It would also be useful to get some data that prove that both catchments

HESSD

2, S396–S401, 2005

behaved in the same way before the clearing took place.

-P. 826 the extent of the saturated areas varies in time. What do you mean when you write that “the stream zone saturated area increased from nil to 8%” to what time period do you refer?

-P.826 I don’t see any difference between Fig. 2a and Fig. 2b! I you want to show some impact on low flows it might be helpful to plot the graphs on log scale. Would it not be more useful to plot the streamflow before and after the clearing?

-P.826 if the top soil is highly permeable, did you ever observe surface runoff in those two catchments? You suggested that saturation excess overland flow is the dominant runoff generating process, but is this very likely with such highly permeable soils? Further explanations are needed to assess your choices of model complexity

-P.826 what do you mean by “updated” parameter set?

-P.827 I think there is a confusion concerning the calculation of actual interception: In 3a should it not be Ia=Csmx-(Cs-PET) if PET$<$Cs and in 3b Ia=Csmx if PET$>$Cs ?

-Equations 6a and 6b are not understandable as long as Wd, Wdmx and Wwmx have not been defined

-P. 829 “The potential volume of the Dry Store is determined by an inter-relationship between climate, vegetation cover, soil depth, physical properties and field capacity”. This explanation is particlarly vague

-Better annotations for Fig. 6 would be helpful: the difference between wd and wdm is not clear on this illustration. The way you have put it now, suggests that wd=wdm! what is Ap? I didn’t find the definition. Aw is not shown at all.

-Fig. 7 the same remarks apply for Fig.7 where it seems as if ww=wwwm

-P.836 in order to clarify the calibration process, a table with model parameters (estimated a priori and after calibration respectively) would be appreciated

HESSD

2, S396–S401, 2005

Interactive Comment

-P. 838 “The daily model accounted for the trend in groundwater level very well” I cannot find the data that would prove this!

-P.838 is it really necessary to include your observations on the Wights catchment? Paragraph 6.1 does not present any results of your modelling. Where can I verify the good agreement between simulated and observed water table depths.

-P.838 Does Fig. 8 show the time variation of the simulated saturated areas, or are these measurements? It would be more interesting to compare the model results to some “ground truth”. Again, you state that “the model represented this process very well”. But how can I be sure of this? Certainly not by looking at figure 8

-P. 839 why should the presence of groundwater in the shallow bore be the evidence of saturation overland flow. Saturated subsurface flow (e.g. transmission of pressure waves) could also be the dominating runoff mechanisms.

-P. 839 You should use performance measures to assess the quality of your simulation: when you state that “the predicted streamflow is in excellent agreement with the observed values” you should give some “hard” data that support your statement (Nash criterion, mean error, biasĚetc.)”

-P. 839 I do not see on Fig. 10b that “the interflow component is under-estimated”. What do you mean?

-Fig. 11 The R2 is not a very good performance measure because of the weight given to the low flows. Indeed, the figure shows that the peak flows are not well predicted. Could you give the Nash value as well?

-P.840 why do you discuss the agreement of fit with observed monthly and annual streamflow after having shown the results of the daily model? If the daily streamflow are in “excellent agreement” (p.839) why should it be different for monthly and annual streamflow? May be it would be more interesting to discuss the bias.

-P.841 the methodology to correct for uncertainties in the rainfall data is not very con-

HESSD

2, S396–S401, 2005

Interactive Comment

vincingĚ If there is a problem with the measuring equipment why should it be acceptable to take the average?

-P.842 “the model was calibrated using observed groundwater level”. This is new for me! Which parameters were calibrated with the groundwater data? Did you use multi-objective functions to calibrate the model? Did the best parameter set give the best performance regarding groundwater and streamflow data or did you have to make some tradeoff between the two.

Technical corrections:

Abstract: ..are the most important components p.828: transpiration from Wet Stores p.828 are expressed as p.830 Eq. 8 x needs to be defined Eq. 13 As has not yet been defined Eq.25 how do you calculate the baseflow component Qbl? I know that this function has remained unchanged from the monthly model but I still would recommend to add its equation here. The same applies for DWgl p.836 the first five years of data were used

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Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 821, 2005.