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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

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AC: We would like to thank the referee for open and constructive criticism to our paper. We have responded to it and modified the paper as detail as we possible could. Please see the response below:

General comments

RC: 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 crop land as Fig. 3 seems to suggest?

AC: The paper now incorporates all the comments of two reviewers. Therefore scientific merit of the paper should be clearer. How the land use changes are incorporated into the model and what parameters were necessary to change are detailed in Section6.4.

RC: 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.

AC: Clearing of native forest for pasture development is the major cause of stream salinity and land degradation in Western Australia. Other land use changes include forest thinning, operational logging, forest fire and reforestation. The effect of forest clearing on water balance is the greatest compared to other land use changes. Therefore, we assumed that if the model were capable of simulating processes related to forest clearing for pasture development, it would be able to do the others. And it has successfully done so. The basin-scale operational model, which uses the present model as 'building block', has been successfully used for predicting the effects of logging, forest fire, clearing and reforestation on streamflow and salinity (eg Bari et al., 2003). The model parameters need to be changed for representing different types of land use is discussed in Section6.4.

AC: We would like to keep the title similar to the monthly model so that it is easier to understand that the present work is further development from the monthly model.

AC: The structure of the model and conceptualization of fluxes among the Stores are completely new. Numerical representation of the interception and transpiration (ac-

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counts for 80-97% of water balance) are completely new. If similar concepts of other components of the model were used elsewhere, they have been properly referenced and acknowledged.

RC: 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.

AC: As the daily model is a further development from the monthly model, many of the details are not presented as it would be just repeat from the paper - Bari and Smettem, 2004, which was also published in HESS.

RC: 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.

AC: Description of the calibration process is elaborated (Section 5). Section 6.3.1 (daily streamflow) elaborated and Table 1 with statistical criteria of model performance has been added. The observed groundwater level was taken as guide for changes in storage content and parameter calibration. All the parameters were calibrated against observed streamflow data, with some statistical criteria as now added into the text (sections 5, 6.3.1, 6.3.2). The best parameter set gave best performance for both the groundwater level and streamflow.

AC: Figures and equations were modified as suggested.

AC: Presentation of model results was elaborated. Two figures (groundwater level trend and flow duration curves) have been added and results were discussed in detail. Uncertainty of model parameters is now detailed in Section 6.4. The rainfall analysis is again reviewed. There was no instrumental error. Accordingly the text is modified to give better picture of the model performance (section 6.4).

AC: The rainfall analysis is again reviewed. There was no identifiable instrumental error. Accordingly the text is modified to give a better picture of the model performance (section 6.4).

RC: 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 before hand but how can the reader know how reliable these estimates 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.

AC: The number of parameters are kept absolutely minimal to represent the process - streamflow generation on a daily time step following clearing of native forest. This was the model development strategy - 'downward approach' - annual, monthly then daily. Therefore the number of parameters in the model does increase as time step diminishes, but this is necessary.

AC: The robustness of the model is very high which is demonstrated in the paper Bari, M.A. and Smettem, K.R.J., 2005a. Further testing of robustness is beyond the scope of this paper. A table for key parameters is given in the paper, which describes the monthly model. At this stage we were not sure how many parameters of the model would be able to be estimated a priori. Therefore the model was applied to 4 additional experimental catchments and then the parameters were classified into three groups - 'known', 'fixed' and 'variable'. The 'known' and 'fixed' sets are estimated a priori and only the 'variable' set of 7 parameters are calibrated for all other subsequent applications. For details see - Bari, M.A. and Smettem, K.R.J., 2005a. Parameter sensitivity to climate and landscape variability of a simple, lumped salt and water balance model, Hydrology and Earth Systems Science Discussions, 2:1405-1447. However, a detailed discussion about the sensitivity and calibration is now given in Section 6.4.

AC: Observed data were used as much as possible eg. Porosity, soil moisture content, hydraulic conductivity, top soil thickness, groundwater level, regolith thickness, surface slope, stream depth etc. How the conceptual soil moisture content of each of the stores changes with time was given in the monthly paper (Bari and Smetttem, 2004). However, changes in content of all five stores are now discussed in detail in Section 6.3.4 and presented in Table 2.

Special Comments:

RC: -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).

AC: As the problem in Western Australia is so different from other parts of the world (particularly the magnitude of the groundwater response to clearing and the initial depth of the unsaturated zone), we primarily focus on WA hydrology. The last para of Section

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1 is modified to clarify the model objective and the interesting/innovative part of the paper.

RC: -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.

AC: Modified as suggested.

RC: -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.

AC: Structure is generally number of stores and how they are connected. Complexity is the numerical representation of the processes. Both the terms have been cross-checked and are now uniform across the whole document.

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

AC: Elaborated as suggested (Section 2).

RC: -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 rain gauges did you consider, what other equipment has been installed). What data series do you have over what time period (do you have data before and after the clearing)?

AC: Elaborated as suggested (Section 2).

RC: -P.825 you mean that following the clearing, the groundwater rose by more than 15

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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 behaved in the same way before the clearing took place.

AC: Yes! A new figure (Fig.8) is added. Reference is given that both catchments behaved similar manner before clearing (Ruprecht and Schofield, 1991)

RC: -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?

AC: There was no groundwater induced saturated area before clearing in the Lemon catchment and this increased to 8% in 1990s. Section 3 is modified accordingly.

RC: -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?

AC: The figure is modified as suggested. As Ernies catchment was a control for Lemon, it was established that the streamflow for both catchments was similar before clearing (Ruprecht and Schofield, 1991). It was more important to know how the flow generation process changed following clearing and when groundwater reached the stream bed.

RC: -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.

AC: Surface runoff is generated when the stream zone gets saturated. The highly conductive top soil (stream zone) gets saturated because of very low conductive soil underneath (this is a strong texture contrast soil). Text is elaborated and a reference added (Section 4.2).

RC: -P.826 what do you mean by 'updated' parameter set?

AC: Calibrated parameter set, text modified.

RC: -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?

AC: The equations (2 and 3) we have provided are correct and found no error in it. What you have suggested is another special form.

RC: -Equations6a and 6b are not understandable as long as Wd, Wdmx and Wwmx have not been defined.

AC: Variables defined as suggested.

RC: -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 particularly vague.

AC: Text modified as suggested.

RC: -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.

AC: Figure modified as suggested. Definition of and are found in Appendix A.

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

AC: Figure modified as suggested.

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

AC: A table for key parameters is given in the paper, which describes the monthly model. At this stage we were not sure how many parameters of the model would be able to be estimated a priori. Therefore the model was applied to 4 additional experimental catchments and then the parameters were classified into three groups

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- 'known', 'fixed' and 'variable'. The 'known' and 'fixed'sets are estimated a priori and only 'variable' set with 7 parameters are calibrated for all other subsequent applications. For details see - Bari, M.A. and Smettem, K.R.J., 2005a. Parameter sensitivity to climate and landscape variability of a simple, lumped salt and water balance model, Hydrology and Earth Systems Science Discussions, 2:1405-1447. However, a detailed discussion about the sensitivity and calibration is now given in Section 6.4.

RC: -P. 838 'The daily model accounted for the trend in groundwater level very well' I can not find the data that would prove this!

AC: A new figure (Fig.8) with observed and predicted groundwater level is added. Accordingly Section 6.1 is elaborated.

RC: -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.

AC: Reference to Wights catchment is deleted. A new figure (Fig.8) with observed and predicted groundwater level is added. Accordingly Section 6.1 is elaborated.

RC: -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 fiure8.

AC: The figure shows the predicted variations in the stream zone saturated areas. Except for the observed shallow groundwater level in the stream zone there is no measurement of the stream zone saturated areas. It was indirectly estimated annually from satellite photographs and field visits. Accordingly Section 6.2 is elaborated.

RC: -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.

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AC: It is the evidence of overland flow and also shallow subsurface flow (interflow) over the texture contrast. Text is modified accordingly.

RC: -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, bias etc.).

AC: Section 6.3.1 (daily streamflow) elaborated and Table 1 with statistical criteria of model performance has been added.

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

AC: Typo error, fixed.

RC: -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?

AC: A 1:1 line is added. Daily Nash value is given in Table 1. How the monthly Nash value increased (details in another paper - Bari, M.A. and Smettem, K.R.J., 2005a. Parameter sensitivity to climate and landscape variability of a simple, lumped salt and water balance model, Hydrology and Earth Systems Science Discussions, 2:1405-1447.) is discussed in Section 6.3.21.

RC: -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.

AC: The bias of observed and predicted monthly streamflow is now discussed in detail. Section 6.3.3 also discusses how the daily model performed better than the monthly model.

RC: -P.841 the methodology to correct for uncertainties in the rainfall data is not very convincing. If there is a problem with the measuring equipment why should it be acceptable to take the average?

AC: The rainfall analysis is again reviewed. There was no identifiable instrumental error. Accordingly the text is modified to give a better picture of the model performance (section 6.4).

RC: -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.

AC: The observed groundwater level was taken as guide for changes in storage content and parameter calibration. All the parameters were calibrated against observed streamflow data, with some statistical criteria as now added into the text (sections 5, 6.3.1, 6.3.2). The best parameter set gave the best performance for both the groundwater level and streamflow. There was no trade off.

Technical corrections:

RC: Abstract: .are the most important components p.828: transpiration from Wet Stores p.828 are expressed as p.830Eq. 8x needs to be defined Eq. 13 As has not yet been defined Eq.25 how do you calculate the base flow 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

AC: Technical corrections were made and equations were added as suggested.

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