Hydrol. Earth Syst. Sci. Discuss., 3, S1584–S1588, 2006
www.hydrol-earth-syst-sci-discuss.net/3/S1584/2006/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Sensitivity of point scale runoff predictions to rainfall resolution" *by* A. J. Hearman and C. Hinz

Anonymous Referee #2

Received and published: 24 November 2006

Reviewers comments on 'Sensitivity of point scale runoff predictions to rainfall resolution' by AJ Hearman & C Hinz

General Comments

The paper uses a lumped parameter bucket model of hydrological processes, combined with synthetic rainfall data created by a bounded random cascade model, in order to investigate the effect of non-linear high resolution rainfall data (compared to time averaged rainfall) on model predictions of infiltration excess and saturation excess runoff. The synthetic rainfall data was generated to be representative of south western Australia, and four different soil profiles were investigated comprising clay, loam, sand

and a layered soil.

The paper demonstrates that a simple bucket model approach to modelling runoff can be used to investigate the potential magnitude of errors in predictions of both the magnitude and dynamics of infiltration excess and saturation excess runoff which arise from using low resolution rainfall inputs. In doing so the paper addresses relevant scientific questions within the scope of HESS using a combination of approaches that together yield a novel means to investigate the impact of rainfall resolution on prediction of runoff. The results are sufficient to support the discussion and conclusions, and substantial conclusions are reached. The scientific assumptions are outlined, but some of the limitations of the modelling approach, together with applications of the findings, could be clarified for the reader in the text.

Specific Comments

The introduction focuses on the rainfall aspects of the research, with little explanation of soil conditions. The authors could highlight (in the methodological section) to what extent there is any limitation to the soils dataset (e.g. hydraulic conductivity, drainage coefficient) that might affect the modelling outputs when predicting runoff using this method. This is important because it is a stated aim of the paper to identify both the rainfall and soil conditions under which model predictions are most sensitive to rainfall resolution. For example, the paper ignores the effects of macropores on infiltration or runoff generation. Could the authors comment upon whether, for the region of south western Australia that has been modelled, this is a fair assumption to make. How might a consideration of macropore flow (due for example to the root channels of the Jarrah tree) in the modelling affect the results that were obtained? Will future improvements in the modelling approach need to take these types of mechanism into account when considering point scale runoff predictions?

Modelling approach

The introduction to the bucket modelling approach does not adequately explain to the

HESSD

3, S1584–S1588, 2006

reader which alternative approaches may have been considered, and the final advantages and limitations of using the bucket approach. There does seem to be some passing acknowledgement of alternative approaches (e.g. Green-Ampt model) and also an indication that there are limitations in comments such as 'trajectories were approximately followed' but no explicit explanation of what these limitations might be. In particular, the model uses a threshold value of infiltration capacity, and therefore does not consider changes in infiltration capacity during a rainfall event. There is some acknowledgement of this, for example, when the authors compare their findings to those of Bronstert and Bardossy (2003). I would like to see the perceived relative advantages and limitations of alternative approaches, which would capture intra-event variation in infiltration capacity, explained more explicitly in the article.

Section 2.4. Could the authors add a short statement to explain to the reader why dimensionless analysis is a useful methodology for exploring soil and rainfall thresholds at which model predictions are most sensitive to rainfall resolution.

Interpretation of results

p3527. I found the explanation of Figure 4 very useful in demonstrating the processes that were further elaborated upon in Sections 3.2 onwards. I wonder whether this section might benefit from a brief outline of the reasons why the findings are important for other hydrological research. Alternatively each section considering amounts (3.2.1 & 3.3.1), dynamics (3.2.2 & 3.3.2) could perhaps be concluded with such an analysis. Section 3.2.2 does go some way to addressing this (when considering the importance of differences in the dynamics of predicted infiltration excess overland flow when using high resolution rainfall inputs) by considering implications for research on soil erosion. Implications for the predictions of contaminant transport models could also be addressed (for example surface-applied agrochemicals).

In places the interpretation of the results is quite wordy, and I found the links between the figures and text difficult to follow. For example, the paragraph on page 6 which

HESSD

3, S1584–S1588, 2006

begins ‘Figure 6 can be used to explain’ was particularly confusing. Figure 6a-c did not seem to conform with the descriptions provided in the text (e.g. Figure 6c does not appear to consider Ink* values greater than 2.5), and Fig 6d & e do not contain labels.

p3536, line 5. The meaning of the paragraph was very difficult to follow and could be more clearly expressed.

At the end of the discussion the authors note that simulations were run for soil moistures at wilting point, but not presented. This would be better explained earlier in the paper. In addition the previous paragraph states that the simulations indicated that the biggest changes in storage amount occurred in clay soil where the initial soil moisture is at wilting point which seems to imply that the results of simulations from wilting point have been presented?

Concluding comments

The concluding comments summarise the different conditions under which thresholds are triggered for the simulated rainfall-soil characteristics. This summary could be shortened somewhat with more emphasis on to what extent the authors consider that these results are transferable to other climates and soil structures (rather than textures which have been considered in modelling). Also I would like to see further consideration of the wider implications of these findings. For example, in the discussion the authors allude to the potential importance of the findings for studies of soil erosion, for ecology and predictions of hillslope instability. All of these points require further elaboration.

Minor errors

In the abstract - split inturn into two words.

Page 3520 line 12. Consider using ‘as an illustration of a method to determine the soil-storm relationships’ instead of ’soil, storm’

In eqn (3) there is a zsoil but also zsoil. The z in italics should be changed.

HESSD

3, S1584–S1588, 2006

Eqn(4) is not presented with the same nomenclature as the other equations. Possibly there is an 'if' statement missing?

On Page 3523, line 4 the concept of 1.875 minutes resolution is introduced before the explanation which follows in Section 2.2. This statement would be better coming later in the article.

The conditions required to generate infiltration excess (qi) are listed in equation (2). Could the authors check the second part of this statement. It appears from (2) that once soil infiltration rate exceeds soil infiltration capacity then the model starts to calculate a value for qi(t). However eq (1) states that if the rainfall intensity at time t exceeds soil infiltration capacity then the soil infiltration rate is set to equal the soil infiltration capacity, hence there is no time when the condition in eqn (2), of psoil(t) $>$ ksoil would be met?

Figure one includes both thresholds and flow processes - could a clearer distinction be made between the two on the diagram? Also modify the diagram to make it clear that qi and qsat are lateral fluxes.

The caption for Figure 9 seems to be incorrect, as there are six diagrams in total and only five headings.

Section 3.3 discusses the modelling of saturation excess overland flow, which is necessarily explained by describing the thresholds for infiltration excess overland flow. Figure 9 might be of more help in explaining the thresholds for saturation excess flow to the reader if the infiltation excess overland flow and thresholds were also included in accompanying graphs.

Section 3.4. There is no Figure 14 provided - do the authors mean Figure 13?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 3517, 2006.

HESSD

3, S1584–S1588, 2006