Hydrol. Earth Syst. Sci. Discuss., 6, C1079–C1081, 2009
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Hydrology and Earth System Sciences Discussions

Interactive comment on "The snowmelt runoff forecasting model of coupling WRF and DHSVM" *by* Q. Zhao et al.

Anonymous Referee #1

Received and published: 5 June 2009

This paper reported an interesting modelling study to predict floods generated by snowmelt events using coupled weather forecast model and hydrological model. The problem is very practical and the method is promising too. The authors also showed tremendous efforts in preparing various inputs and model validation data. In general, the topic of this study well fit into the publication scope of HESS. However, based on this reviewer's evaluation, some major revisions to the current manuscript are required before it could meet the publication standard of HESS or other peer-reviewed international journals of the same quality. Following are some major revision suggestions made by this reviewer.

(1) Although the author described both models (WRF and DHSVM) and the coupling

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procedures, inputs and outputs in some details and the results (Fig.7 and Table 1) was really promising, the reviewer hardly get any useful take home messages after reading through the manuscript. Many key issues remain unclear to the readers, e.g. (a) why those models were chosen out of many similar models? or what are the key features of those models that made them fit better in this study? (b) are there any key parameters need to be calibrated or tuned for the studied watershed? (c) what are the key processes that controls the final flood forecasting results, which algorithms / parameterisations made it a success in this study. It might be worth of some efforts to make more in depth analysis of the results to answer above questions.

(2) The results showed in figure 7 and table 1 were really good. However, the readers may not be convinced that the model will work for practical flood forecast since only one snowmelt event were presented. Modellers know that the simulation results could be tuned to fit certain observed outputs if sufficient parameter tunings were performed for certain site. To make this study more reliable, the reviewer would like to suggest the authors to make another simulation of another snowmelt period for the same watershed while keeping the parameters the same as previous run. It should not be difficult since most of the inputs (DEM, vegetation, river routes etc.) would remain the same.

(3) Some more references are required when describes the models and data. e.g. P. 3341 L.9-12, L.15-20; P. 3343, L. 19-20 and more: references to those data sources and modeling schemes will help those readers from slightly different background.

(4) The English of this manuscript needs a good editing. There were numerous typos, grammatical errors and unclear expressions through out the manuscript. Just name a few here: P.3336: (a) L.6: 'combing'? 'meso-microscale' (meso/micro scale?) (b) L.12:'absolute relative error' (unclear) (c) L.5-11: 'It included: . . . .", check grammar. P. 3341: L. 2 parameter(s)? L.2 and L.5: stimulated(?) area and stimulated (?) time? P.3341 L.25 – P.3342 L.1-2: check grammar. P.3343: L. 13: even-green (ever-green) and more. . . . .

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Some minor revision suggestions: (1) P.3338 L. 15: add references to support you claim. (2) P. 3342 L.6 (Eq. (2)): are 'es' and 'es (subscript)' same? (3) P3344. L.2.: Equation is not clearly presented: >0.4 for what term? (4) P.3345 L.17-20. 'the forecasted . . ., observed data is significantly higher than. . .', is this statement just opposite to the figure result? (5) P.3356 Fig. 6: what is the maximum number of SWE in the scales, 64747mm seems a unreasonable number to me.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 3335, 2009.