Hydrol. Earth Syst. Sci. Discuss., 3, S1818–S1821, 2007
www.hydrol-earth-syst-sci-discuss.net/3/S1818/2007/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Flash flood modeling with the MARINE hydrological distributed model" *by* V. Estupina-Borrell et al.

C. Paniconi (Referee)

panicocl@ete.inrs.ca

Received and published: 17 January 2007

Review of "Flash flood modeling with the MARINE hydrological distributed model" by Estupina-Borrell, Dartus, and Ababou

The paper introduces and applies a model for flash flood prediction based on a 2D kinematic overland flow module for the "upstream" portion of a river basin and a 1D St-Venant channel routing module for "river" portion.

Minor remarks:

The paper puts a good emphasis on the need to rely on remote sensing data for model inputs (topography, rainfall, land/soil cover, etc) in order for flood forecasting models

to provide real-time results in gauged as well as ungauged (or poorly gauged) basins. However in the Introduction it is stated that flash floods "occur on small areas", so how feasible is reliance on remote sensing data (in particular satellite imagery) in this case, where the spatial resolutions needed by a model may not match the coarser resolutions currently provided by satellite sensors? The Introduction also admits that ground radar rainfall data is not always available, so how can a model such as the one being proposed be applied in these types of "poorly gauged" basins when rainfall data is so critical in flood forecasting?

Page 3 lines 11-12: the proposed overland runoff model "supplies interesting results, so we are going to use this same run off model in the rest of the paper" - this is not a very convincing justification.

The literature review is very cursory (one paragraph) and does not warrant a separate section heading (1.2) in a section that is more about the "modeling philosophy" behind the proposed model than a detailed literature review.

In Figure 5 the colors indicated in the caption do not correspond to what is shown in the figure. Figures 5 and 7 can be combined.

Major remarks:

The proposed model ignores saturation excess runoff (pg 3 lines 4-5) and subsurface/groundwater flow (pg 3 lines 13-14) except for 1D infiltration. This may be OK for some flood events, and perhaps even for some regions (southern France, as this paper focuses on), but the model cannot claim to be generally applicable. To give an example, by ignoring redistribution of water in the subsurface (lateral flow, groundwater losses and contributions, etc) the model cannot make accurate/reliable estimates of the antecedent subsurface storage status (soil moisture, water table levels) of a basin, which can be quite critical in both triggering and sustaining a major flood event. The paper provides a reasonable discussion (together with a sensitivity analysis) of this issue in section 4.3, and admits that the issue is "still being explored".

HESSD

3, S1818–S1821, 2007

Another example that suggests the model is actually quite specific rather than generally applicable is in the analysis used to "decompose" the river basin into "hillslope" and "stream" components. This analysis doesn't actually distinguish these two components, but rather it gives an indication of where a kinematic approximation may be valid based on hillslope length, angle, and other parameters. "It turns out" (pg 6 line 13) that the region of the basin (presumably the Orbieu basin where the model is later applied, though this isn't mentioned here) where the kinematic approximation is not valid "matches the main stream network", and thus the authors apply the 1D St-Venant channel model here and the 2D kinematic overland model elsewhere. Therefore the "decomposition" criterion presented does not appear to be a rigorous one that can be generally applied to other basins.

In light of the above two comments, I strongly suggest that the Abstract, Introduction, and Conclusions (which make some very general, broad statements), and even the title of the paper, be altered to better reflect the specificity of the proposed model and its considered region of application.

There seems to be a "mismatch" between the level of numerical detail used in solving the overland/hillslope component and the channel/river component. For the former the authors introduce many simplifications to compute the infitration and runoff contributions, justified by the need for a real-time model, whereas for the latter the full 1D St-Venant equation is solved numerically. Might not real-time considerations - not just computational efficiency but also the need for a robust model - also warrant some simplifications for the channel component? The St-Venant equations are nonlinear and obtaining a robust, stable, efficient solution is not a trivial task under some conditions. Do the authors justify this different level of detail by the 2D vs 1D nature of the two components? Or perhaps by the relative portion of the basin designated as "hillslope" vs "channel" flow? Some discussion of this point would be interesting.

"Calibration" and "data assimilation" are quite different things. Section 4.4 presents the calibration of the model, thus this section heading should be changed.

HESSD

3, S1818–S1821, 2007

The model setup for the Orbieu application is well-presented, but the actual application of the model (i.e., presentation and discussion of the results) is quite limited. It is limited to some comments on the different infiltration modules and the rainfall input data. Given the paper's focus on how the model represents and simulates the channel and hillslope components, it would have been nice to see more on these aspects.

In summary:

Towards the end of the Introduction the authors state that "The main objective of this paper is to bring some elements of reflection on flash floods". In fact the paper is a very detailed presentation and application of a specific model. The model has some innovative features, and the application to a real flash flood event is interesting. These are the strong points of the paper, and it should thus be revised to focus specifically on these. The paper should avoid more general statements (such as the above quote), and overall re-visit the claims and discussions wherein the model is presented as being of general applicability.

Final remark on style/grammar:

The paper needs editing for English. Most corrections are minor, but some are less so. E.g., "pounded" should be "ponded"; "cinematic" -> "kinematic"; "run off" -> "runoff"; "perceptual model" -> ??.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 3397, 2006.