Hydrol. Earth Syst. Sci. Discuss., 7, C4231–C4236, 2010
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Hydrology and Earth System Sciences Discussions

Interactive comment on "Effect of radar rainfall time resolution on the predictive capability of a distributed hydrologic model" *by* A. Atencia et al.

Anonymous Referee #1

Received and published: 17 December 2010

General summary

This paper tries to show the impact of different temporal resolutions of weather radar rainfall data on the simulated catchment response for four different flash-flood events in Catalonia. In order to improve the quality of the weather radar, a 3 by 3 window above the rain gauge is averaged. Then two different window probability matching methods are applied between the radar reflectivity and rain gauge rainfall intensity, in order to obtain proper Z-R relations for different types of precipitation (convective / stratiform). As a last step the rainfall is advected using a cross-correlation technique. In the last part of the paper, for different temporal resolutions, different hydrological simulations are performed. At a 15 minute resolution, the response of the catchment

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is best predicted, which according to the authors can be related to apparent basin size and spatial and temporal resolution of the precipitation field.

Overall quality

Personally, I think the authors had quite some difficulties when writing this paper. It contains multiple ideas, but these are not well merged into one paper. It therefore does not contain a clear message, but basically consists of separate parts. Unfortunately, many of these parts are difficult to understand, due to the fact that the authors are non-native English speakers. Next to that, most of the presented ideas have already been mentioned elsewhere in literature (like e.g. the WPMM, weather radar rainfall advection correction, and the impact of temporal resolution on simulated discharge). In my opinion these parts are threated far to elaborate and do not add to the quality of the paper. The authors idea to link the optimal temporal resolution of 15 minutes, to some kind of catchment characteristic (as was done by Berne et al., 2004) is nice, but needs a lot more clarification and further analysis. In its current state, I would therefore reject the paper. Below, I have provided some ideas on how to improve the quality of the paper.

Major comments:

Too much emphasis is placed on different weather radar rainfall correction steps. This part of the paper has been done in a lot of other papers, and for the current paper, do not lead to new insights. I would therefore try to alter the focus of the paper, focusing less on rainfall corrections but more on the impact of the temporal resolution on the simulated discharges. With respect to the latter, try to obtain more insight the relation between the catchment characteristics and the temporal rainfall input. In order to do this, probably a considerable amount of analyses have to be performed. But looking at the data which is used in the paper, the authors do have the possibility to perform this analysis. In my opinion, such an analysis would improve the quality of the paper.

p. 7996, lines 1-19: This abstract is not well written and misses a clear message, what

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to expect from the paper.

p. 7996, line 20 – p. 7997, line 4: Unclear what the authors mean by this paragraph.

p. 7997, line 17-18: Rewrite: or selecting – the network.

p. 7997, line 20: Rewrite: the rainfall – Z/R relations.

p. 7998, line 8: The impact of advection correction is mainly dependent on the size of the catchment and the spatial-temporal gradients of the precipitation field (see also Fabry et al., 1994, J. Hydrol., 161, 415-428).

p. 7998, line 11: This method was not proposed first by Anagnostou and Krajewski (1999), but appeared much earlier. See the work by Rinehart and Garvey, 1978, Nature, 273 287-289.

p. 8000, line 15: Rewrite the statement "It can – 46 mm"

p. 8000, line 17-29: At the end of this paragraph it is mentioned that both networks are being merged. What is meant by this, the merging of the SAIH with the XEMA network, of the XEMA 30 minute with the 1 hour network. What is the final resolution of this product? One hour? If so, how come the WPMM is done at half hour intervals. If the resolution is 30 minutes, what is done with the 1 hour XEMA gauges? Next to that, it is mentioned for the SAIH gauges that they will be calles "IBS, hereinafter" (line 20). However, in the rest of the paper, the name SAIH is used.

p. 8001, line 1-13: Are these procedures implemented by the SMC or is this an extra step which has been performed by the authors. Please rewrite this paragraph.

p. 8001, line 21-22: This statement is very important, but for the current submission rather vague. Please rephrase.

p. 8002, line 1-7: This totally doesn't add anything to the paper. I would remove this paragraph completely.

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p. 8002, line 8-10: Results of a previous paper by Atencia et al. (2008) are mention a few times within this paper. Why not give a brief summary (but a bit more elaborate then just these 3 lines) either here or in the introduction section of the paper.

p. 8002, line 18-20: Please rephrase this part.

p. 8002, line 25 – p. 8003, line 4: In Section 2 you mention there are multiple networks of rain gauges which somehow have been merged. Why do you mention here that you have used 5 minute data? Try to rewrite this part.

p. 8003, line 5- 13: Rewrite this part. I believe the first and last bullet can be merged. Why is the SAIH rain gauge network used only? What about the XEMA gauges?

p. 8004, line 1-13: Remove this part from the paper.

p. 8004, line 24-27: This part is unclear, please rewrite.

p. 8004, line 24 – p. 8005, line 7: Remove this part from the paper.

p. 8005, line 16: For mountainous regions, Li et al. (1995, J. Appl. Meteor., 34, 1286-1300) show that due to residual clutter, erroneous cross-correlations were obtained. Because in the current paper, this method was also implement within a mountainous region, did the authors encounter similar problems?

p. 8005, line 19 – p. 8007, line 1: Because the method is not new and has been explained in multiple papers, either remove the cross-correlation identification method to the appendix, or refer to these other papers, briefly summarizing it's implementation.

p. 8007, line 10-11: Please explain this statement.

p. 8007, line 20 – p. 8008, line 17: Is it necessary to explain the model or would it be an obtain to remove this part to the appendix or refer to other papers. I understand that the different parameters of this are being analyzed in the Table 3. However, this table might even be removed by just mention that the different parameters were optimized using a statistical approach at different temporal resolutions.

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p. 8008, line 22 – p. 8009, line 2: This is such a basic step in distributed hydrological modeling. I would therefore remove these sentences.

p. 8009, line 3 – 17: The authors show they have tried to preserve the amount of precipitation, while converting the 2 by 2 km radar rainfall grid onto the resolution of the DEM. I was wondering how important this aspect truly is, especially when considering the fact that in order to obtain the 2 by 2 km resolution of the radar, also some kind of interpolation was performed when converting the polar radar data into the gridded framework.

p. 8009, line 21: Having not worked with the RIBS model myself, I was wondering whether the it is necessary to define any initial conditions. At the event scale, as is performed in this paper, initial conditions tend to be very important in order to perform proper discharge simulations. The authors do mention something in lines 14-16 (p. 8010).

p. 8009, line 23: Because the authors have the possibility to calibrate their model using 6 discharge measuring points. Why then, only focus on the outlet and do the verification again on all 6 points?

p. 8010, lines 1-3: Remove these lines, but just refer to Figure 2.

p. 8010, lines 10-14: These are all the parameters which are mentioned in Section 3.2.1, so why use the phrase "This analysis showed that the most influential parameters . . .". Please remove both table 3 and this section from the paper, and just mention that the model has been calibrated using the method as proposed by Freer et al. (1996).

p. 8012, lines 2-7: Please remove these lines.

p. 8012, line 18: Unfortunately, I do no understand what is meant by Figure 10.

p. 8014, lines 3-15: As I said before, this method has been used in many papers and in the current submission no new results are obtained. These results therefore do not have to be provided, or just mention them briefly.

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p. 8014, lines 17-23: In case the authors really would like to emphasize on the impact of advection corrections, please show a clear example of this in the form of a figure where you compare a non-advected rainfall field to an advected one. However, because this would lead to 1 minute radar-rainfall data, why not use this in the rainfall-runoff modeling part as well?

Section 4.3 and the Discussion: If the focus of the paper is to show the impact of the temporal input resolution on the simulated catchment response, then this part of the paper is rather short. No hydrographs are presented and highest temporal input resolution is 6 minutes while advection corrected radar data is available at a 1 minute resolution. Please consider altering the focus of the paper, as I mentioned above. When doing this both of these section should be completely rewritten.

Minor comments:

p. 7996, line 24: especially on convective , replace “on” with “for”. Please be consistent with using the word gauge or gage. Both words occur within the paper.

p. 7999, line 5: Replace “processes” with “variability”

p. 8002, line 23: Add after “velocity”, “of the rainfall/storm-cell system”

p. 8004, line 17: Replace “improve” with “improved”

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 7995, 2010.