March 31, 2011

Dear Prof. Remko Uijlenhoet:

We are writing in response to the reviewer comments on the manuscript hess-2010-300, entitled "Effect of radar rainfall time resolution on the predictive capability of a distributed hydrologic model *", for which major revision and resubmission has been requested.*

In accordance with editors and reviewer suggestions we have introduced major changes, clarifications and additions to the original text to improve the clarity of the exposition. Thanks to the comments of the reviewers, we have realized that some aspects of the methodology were not properly discussed in the original manuscript, and we have made an effort to improve them. Detailed responses to each reviewer's comments will be provided in this file.

The original comments are in bold face, author responses are in italics, and, when applicable, the corresponding revised text material is presented indented, in standard font.

COMMENTS FROM EDITORS AND REVIEWERS

GUEST EDITOR: COMMENT

Finally, both review reports for this manuscript are available to the editor and the authors. On behalf of the reviewers, I apologize to the authors for the delay in the review process.

Both reviewers have provided critical, but constructive and detailed comments regarding this manuscript. Their general remarks concern the structure of the manuscript and the usage of the English language.

Regarding the structure it seems the opinion of the reviewers is that the authors have attempted to present too many topics in one manuscript, necessarily leading to a lack of depth concerning each of the individual topics. It looks as if the authors have tried to "squeeze" an entire PhD thesis into one single manuscript. Please, consider the main message you would like to convey to the readers and adapt the structure of the manuscript accordingly.

We have made an effort to shorten and clarify the paper. Some background information non essential to understand the novel work has been removed. Furthermore, the structure of the paper has changed to clarify the novelties in each part and to focus on the main objective.

In addition, I strongly recommend having the manuscript checked in detail by a native English speaking colleague. This is an issue brought up by both reviewers and it is therefore evident that the authors have to take this into account.

The whole manuscript has been checked in detail by a native English speaking corrector.

Furthermore, both reviewers raise a number of technical issues the authors have to deal with before their manuscript would reach a form that would be appropriate for publication in HESS. I expect a detailed point-by-point response from the authors regarding all issues raised by the reviewers.

REVIEWER #1 COMMENTS:

OVERALL COMMENT:

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.

Thanks to the reviewer comment we have realized that the presentation of novelties applied into radar rainfall correction steps was no clear and this is probably due to our problems in writing in English. The methodology section has not been sufficiently clear about the novelty of our proposed amendment to the radar images, as discussed later in the discussion. Both radar corrections are needed in order to run the hydrological model properly. Case studies of large convective character could not be corrected with the traditional methodology and two different probability distribution fittings are applied to overcome problems in distribution tails, as the second reviewer has indicated it.

However, addressing your comment, and besides of the English correction by native speaking colleagues, we have changed the structure of the paper and we have made an effort to shorten and clarify the paper. Some background information non-essential to understand the novel work has been removed too.

With respect to the latter, try to obtain more insight the relation between the catchment characteristics and the temporal rainfall input. In order to does 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.

Authors thank the reviewer for this comment. The focus of the paper was changed and the paper was improved by extending the Results and Discussions. The structure of the paper was changed moving subsections throughout the manuscript.
A subsection focused on the selection of rainfall time resolutions link the manuscript to the findings obtained by Berne et al. (2004). The sensitivity analysis of the rainfall time resolutions and its hydrological implications were deepened, extending the results and discussion. More details of changes added to the paper can be found in the following paragraphs.

1. **p. 7996, lines 1-19: This abstract is not well written and misses a clear message, what to expect from the paper**

The abstract has been rewritten. The new abstract is:

> The performance of a hydrologic model depends on the rainfall resolution, both spatially and temporally. As the spatial distribution of rainfall exerts a great influence on both runoff volumes and peak flows, the use of a distributed hydrologic model can improve the results in the case of convective rainfall in a basin where the storm area is smaller than the basin area. The aim of this study was to perform a sensitivity analysis of the rainfall time resolution on the results of a distributed hydrologic model in a flash-flood-prone basin. Because this kind of flood is produced by heavy rainfall events with a large convective component, for which radar estimation exhibits poor accuracy, a second objective was the proposal of a methodology that improves the radar rainfall estimation at a higher spatial and temporal resolution. Composite radar data from a network of three C-band radars with 6-minute temporal and 2\,$\times$\,2\,km^2 spatial resolution were used to feed the RIBS distributed hydrological model. A modification of the Window Probability Matching Method (gauge-adjustment method) was applied to four cases of heavy rainfall to improve the observed rainfall sub-estimation by computing new Z/R relationships for both convective and stratiform reflectivities. An advection correction based on the cross-correlation between two consecutive images was introduced to obtain several time resolutions from 1\,min to 30\,min. The RIBS hydrologic model was calibrated with a probabilistic approach based on a multiobjective methodology for each time resolution. A sensitivity analysis of rainfall time resolution was conducted to find the resolution that best represents the hydrological basin behaviour.

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

The Authors tried to mean that a distributed hydrologic model is more appropriate to simulate a convective precipitation event than a lumped model. The paragraph was amended to avoid confusions. The new paragraph is:

> Accurate flash flood hydrological modelling requires both a suitable hydrologic model and appropriate spatial and temporal resolutions for rainfall estimation. The spatial variability of rainfall exerts great influence on basin processes \citep{winchell1998sia}, especially in the case of convective precipitation events, as

> the storm area is usually smaller than the basin area \citep{bell2000sensitivity}. The spatial distribution of rainfall influences runoff volumes, peak flows and the lag time of hydrographs \citep{krajewski1991mcs, arnaud2002irs}. Therefore, a distributed model can improve the simulation of flash floods events from a lumped model, as the former takes into account the spatial variability of rainfall. Furthermore, a more recent study has shown that distributed model simulations are statistically distinguishable from the lumped model simulations for basin areas around 1000\,km^2 \citep{carpenter2006ilv}.

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

The sentence was: "or selecting the higher value of reflectivity from each radar making up the network." We have changed the sentence. New sentence is:

> or selecting the highest reflectivity value from each of the radars of which the network is composed.

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

The sentence was: "the rainfall intensity could be obtained from low level by Z/R relations." We have changed the sentence. New sentence is:

> the rainfall intensity can be obtained by applying a Z/R relationship to the lowest CAPPI reflectivity value.

5. 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)

We thank the reviewer for the recommendation of this interesting paper. The paragraph refers to the estimated precipitation field, since the radar is discrete sampling (every 6 minutes), not hydrological modeling. We have a look at the paper and the main conclusion is: "When high spatial resolution rainfall data are required, it becomes necessary to use the most realistic accumulation method possible. It was shown that sampling errors can be large but they are easily avoidable given the computing power available today. These errors not only affect the accuracy of rainfall measurements by radar but also the stability of the radar calibration with raingauges (Kitchen and Blackall, 1992). Given a suitable accumulation method, sampling errors over a 5 min interval are minimized with the shortest sampling time combined with a spatial resolution that is slightly coarser than the resolution of the desired accumulation map. While individual rainfall rate maps are noisier at higher time resolution, they still capture with sufficient accuracy the temporal evolution essential for optimal rainfall estimation over short intervals of accumulation."

Furthermore, it shows not only the importance of advection correction to avoid sampling errors but also the importance of good rainfall estimation. "A good rainfall accumulation is reached in two steps. At first, instantaneous rainfall maps of the highest quality must be generated. Then, these maps must be properly accumulated. Although considerable care is usually devoted towards the first step, relatively little effort is directed towards the accumulation process itself *(see Rasmussen et al., 1989): maps of instantaneous rainfall are often simply added, neglecting the fact that the storm moved and evolved during the sampling period."*

Regarding the comments found in the paper about hydrology, it shows that: "Generalizing the improvement that can be derived by coupling such data with a hydrological model is complicated by the situational dependences of a particular catchment. The high resolution data must be coupled with similar resolution knowledge of the characteristics of a given basin before the net benefit or error can be assessed. In an urban environment, the high resolution data offer the knowledge of where a strong rain rate is to the level of a few street blocks at a time resolution such that active drain controls can be implemented. Specific knowledge of the catchment characteristics at the radar data resolution will dictate the effect of the reduction in rainrate measurement errors achieved by the methods described in this paper on the accuracy of hydrological modelling. In very general terms, a reduction in the error in rainfall measurements should result in improved forecasts of runoffs."

Taking into account all this information and maintaining the focus on rainfall, the sentence has been rewritten as:

> Another problem is that the rainfall intensity, especially the convective one, is a continuously varying field due to flux advections or mountain enhancement. It has been shown that sampling errors can be large, but they are easily avoidable given the computing power available today \citep{fabry1994high}. According to this article, the best methodology to avoid this sampling error is an advection correction scheme based on a cross-correlation technique \citep{rinehart1978three} and temporal linear intensity variation. Moreover, this study shows that the intensity variation between images is based on the temporal interpolation proposed by \citet{anagnostou1999rtr} but taking into account that the shape morphology transformation is conducted by means of temporal weights based on a more complex shape transformation \citep{turk2005stu}.

6. **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**

We agree with the reviewer that the first time that this method appeared was in the work by Rinehart and Garvey, 1978, Nature, 273 287-289. We hadn't accessed to this article in order to check the details of the cross-correlation technique used. Consequently, we have included both references.

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

The sentence was: "It can be observed that all of them have an average rainfall amount over the Besòs basin exceeding 46 mm." It has been corrected and rewritten as:

> For each case, rainfall amounts over the Bes\`{o}s Basin higher than 46\,mm were recorded.

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8. **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.

Thanks to the reviewer comment we have realized that this sentence was a bit confusing in the original version of the manuscript. Our main purpose with this sentence was just to show two things: the different resolution of the rain gauges networks over Catalonia and that a density of about 1 rain gauge per 100 km2, that is the case of Catalonia, is insufficient to reproduce the spatial pattern of most of the storms (Corral et al.,2001). Consequently, the sentence has been changed to:

> The merging of both networks produces a loss in temporal resolution (1 hour) and a density of about 1 rain gauge per 100\,km^2, which is insufficient to reproduce the spatial pattern of most storms \citep{corral2001distributed}. Consequently, radar information is essential to simulate flash floods. In this work, the ACA network was used to compute the new Z/R relationship, whereas the SMC network was used to verify the results.

9. **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**

The composed CAPPI imaged provided by SMC has been previously corrected by a first filter applied by SMC. However, a second manual filter has been applied by authors to avoid another ground clutter errors, interference, etc. The paragraph as been rewritten as:

> The radar rainfall estimation was implemented using data from the Catalan Meteorological Service (SMC) radar network, which covers an area of 53\,000\,km^2 over Catalonia and its surroundings. This network is made up of three C-band Doppler radars; a new radar was inaugurated in September 2008 but was not used in this study. The most important characteristics of the composed CAPPI imagery are the spatial resolution (2\,$\times$\,2\,km^2), time resolution (6\,min) and vertical resolution (1\,km) from 1\,km to 10\,km of altitude (10 levels). The CAPPI are calculated by means of the IRIS program, which is based on the linear interpolation of the range to the selected heights in spherical coordinates, with a correction for the earth's curvature to preserve data quality. The radar imagery was corrected at SMC by first passing a filter to remove ground clutter \citep{bech2003ssp}. A second filter was applied to remove the interference between radars (no data in radar location) and another still target, such as a wind power plant.

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

The proposed method carries out a sensitivity analysis of the rainfall time resolution on the results of a hydrologic model in a flash flood prone basin, to find the time resolution that best represents the hydrological basin behaviour. The phrase and the paragraph were rewritten:

The proposed methodology was used to perform a sensitivity analysis of the rainfall time resolution on the results of a hydrologic model in a flash-flood-prone basin. As a distributed hydrologic model is selected to better represent the spatial variations of rainfall in time, spatially distributed rainfall maps for different time resolutions were obtained from the 6-minutal radar rainfall observed. The calibrated hydrologic model was run taking these estimated rainfall maps as input data to determine the time resolution that best represents the hydrological basin behaviour.

The methodology is divided into two parts. The first part describes the estimation of the radar rainfall maps for different time resolutions. The second part describes the probabilistic calibration of the RIBS hydrologic model and the sensitivity analysis of the rainfall time resolution for the results of the calibrated RIBS model.

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

The paragraph has been removed.

12. 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.

The paragraph has been extended by including some results of the paper.

In a previous work \citep{atencia2008nnp}, a large number of Z/R relations were tested for four selected heavy rainfall events. This study showed that radar-based rainfall data underestimated what rain gauges registered by approximately 18\% (~56 mm), and consequently, the results were not suitable for hydrological purposes.

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

The sentence has been rephrased as:

Zawadzki (1975) has shown that both the window area (A) and the spread of the rain-gauge measurement in time (T) are related as follows:

14. 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.

The section 2 has changed to clarify this part. Moreover, this sentence has changed too:

In this study, the rain gauge had a time resolution of 5\,min …

15. **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?**

The ACA rain gauge network is used to compute the new Z/R relationship whereas SMC rain gauges are used just to verify the results. This is done due to resolution of each rain gauge networks. Section 2 (Comment 8) has been rewritten to clarify these aspects.

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

This part is needed to show the accuracy in computation of the new Z/R relationship. However, we have summarized this part.

> Using both parametric and non-parametric techniques, the derivation of the Z/R relation is very simple and straightforward. A randomisation process is applied by selecting different sizes of sub-samples to ensure the minimisation of spatial and geometric errors. This process also provides probabilistic information about the convergence of the population to a final relationship. In this way, the standard deviation (SD) is used to evaluate the consistency of the new relation and the range over which the final relation is absolutely sound.

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

This comment is answered together with the next one.

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

This paragraph is needed to show the methodology applied to distinguish the stratiform and convective precipitation. Moreover, the methodology applied in this paper is different that the technique applied in the original article by Rosenfeld et al. (1995). Nevertheless and taking into account the reviewer comment, it has been sorted into the methodology section and it has been shorten.

19. **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?**

The ground clutter and other residual clutter have been removed by applying a second filter (Comment 9) to avoid the apparition of errors in the cross-correlation computation.

20. **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 its implementation.**

Thanks to the reviewer, we have shortened this part by referring to some papers. We have focused on the temporal extrapolation between images and the modification applied into the methodology proposed by previous authors.

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

The use of a distributed hydrologic model is very attractive when spatially distributed rainfall is introduced as input data, as the spatial variability of rainfall can be taken into account. This is related to the first paragraph of the Introduction. The phrase was rewritten to avoid misunderstandings:

> The use of this model is especially attractive when spatially distributed rainfall is available, e.g., rainfall observed from a meteorological radar station or forecasts of spatially distributed rainfall.

22. **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.**

The Authors think that a brief description of the model should be placed in the manuscript to avoid that a reader could be lost while reading it, but the Authors agree with the Reviewer that this description must not be placed inside the Methodology. The subsection 3.2.1 devoted to the RIBS model was moved into a new section after the Introduction and before the Case study.

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

The Authors agree with the Reviewer that this step is basic. The paragraph was reduced. This subsection was moved into the 3.1 section devoted to the radar rainfall estimation.

24. **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.**

The Authors agree with the Reviewer that in order to obtain the 2 x 2 km resolution of CAPPI imagery some kind of interpolation has been performed. However, we only have this CAPPI imagery and we just try to avoid any extra interpolation while converting the radar rainfall grid onto the resolution of the DEM.

25. **p. 8009, line 21: Having not worked with the RIBS model myself, I was wondering whether 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).**

The RIBS model needs an estimation of the initial moisture condition as input data. This initial condition was estimated from precipitation and temperature data in the days before the beginning of the flood event. This was added to the manuscript in the second paragraph of the subsection devoted to the probabilistic calibration.

26. **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?**

The calibration of the RIBS model focus on three parameters: f, Kv and Cv. These parameters are global, not distributed. Therefore, they must have the same value throughout the basin. The Gramenet gauge station was selected to calibrate these parameters, as it is located in the basin outlet.

The validation with the other 5 gauge stations refers to the sensitivity analysis of the radar rainfall time resolution (subsection 3.3.2.). The 6 available gauge stations were used in the sensitivity analysis of the rainfall time resolution to take into account a possible relationship between the basin area and the rainfall time resolution.

The phrase about the validation was removed because this step is described in the subsection 3.3.2. The paragraph and the subsection were rewritten.

More details can be found in the following references:
- Mediero, L., Garrote, L. and Martín-Carrasco, F.J. (2011). Probabilistic calibration of a distributed hydrologic model for flood forecasting. Hydrological Sciences Journal (In press).
- Mediero, L. (2007). A probabilistic forecast of floods by Bayesian networks applied to a distributed rainfall-runoff model. Doctoral Thesis. Technical University of Madrid, Department of Civil Engineering: Hydraulics and Energetics, 1-268.

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

Lines 1-3 were removed.

28. **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).**

The RIBS model uses more parameters than these four parameters (e.g., Kon, Kop, ϑ, ϑr, ϑs, ε). Section 3.2.1 focuses on the selected parameters for the sake of simplicity. A complete description of the model parameters can be found in Garrote and Bras (1995 a and b) and Mediero et al. (2011).

The hydrologic model was not calibrated using the method proposed by Freer et al. (1996). A modification of this method was used to carry out the sensitivity analysis on the RIBS model parameters, which is a first step in the calibration methodology. The calibration methodology used in this paper is much more complex. A detailed description of the calibration methodology can be found in Mediero (2007) and Mediero et al. (2011). The manuscript presents a brief description of the calibration methodology.

This subsection was rewritten to avoid misunderstandings.

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

These lines are removed.

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

Figure 10 represents the mean value of the RMSE between simulated and observed hydrographs in the validation event. In this event the probability density functions fitted to each calibration parameter are used to generate the random sets of parameter values. The higher is the number of the simulations, the more stable is the value of RMSE. It can be seen that the stability is reached with 200 simulations.

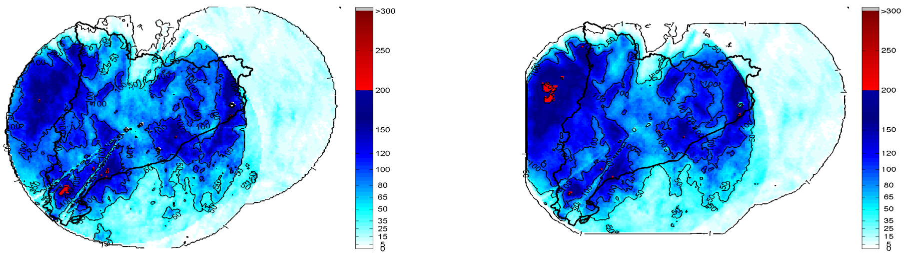
The figure was removed from the manuscript and the paragraph was rewritten to avoid misunderstandings.

31. 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.

According to the reviewer comment and taking into account the comments of the other reviewer, this section has been focused on the comparison between both methodologies of fitting the pdf. This change has emphasized the novelties in the methodology.

32. 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?

A new subsection was added to the manuscript to describe the selection of time resolutions. Regarding the impact of advection corrections two new figures are added. The second one is to show the impact of advection correction in one selected study case whereas two accumulated rainfall fields (before and after applying the advection correction) are plotted in the first one.



(a) Before adv. correction (b) After adv. correction

Fig. 11: Comparison between non-advected (a) and advected (b) accumulated rainfall field

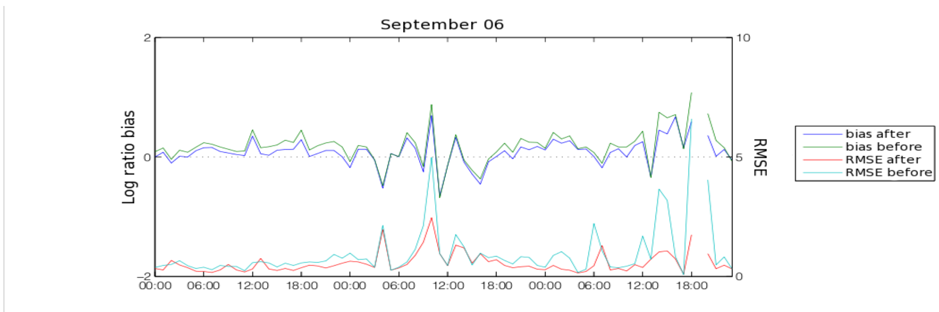


Fig. 12: Validation results for the September 2006 event before and after applying the advection correction.

33. **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.**

Authors thanks the Reviewer for this comment as it improves the quality of the paper. The sections devoted to results and conclusions were rewritten, extending and improving their content to better describe the conclusions of the study.

A new subsection was added to the manuscript to describe the selection of time resolutions. The required minimum time resolutions for the basin areas of the Besos River are between 12 and 24 minutes (as shown in Table 2). The time resolutions were selected between 6 and 30 minutes. Time resolutions higher than 6 minutes are not relevant in this study because time resolutions higher than 3 minutes are only relevant for basin areas smaller than 0.1 (Berne et al., 2004).

MINOR COMMENTS:

34. 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.

All gage words have been changed to gauge to be consistent.

35. p. 7999, line 5: Replace "processes" with "variability" *Done*

36. p. 8002, line 23: Add after "velocity", "of the rainfall/storm-cell system" *Done*

37. p. 8004, line 17: Replace "improve" with "improved" *Done*

REVIEWER #2 COMMENTS:

OVERALL COMMENT:

This is an interesting paper that combines radar-rainfall estimation using WPMM (Window Probability Matching Method which matches raingauge and reflectivity probability distributions) and advection correction methods (which accounts for storm movement in pixel rainfall estimation over a time-interval) with distributed hydrological modelling to investigate model simulation sensitivity to rainfall time-resolution for flash floods. It also attempts to treat rainfall at different resolutions as independent ensemble members, along with taking account of uncertainty in model states and discharge measurements, in a probabilistic model calibration. I am less convinced by the utility of the latter, brings added complexity to the paper and tends to hide judgements on the plausibility of the hydrological model. However, it also has interest.

Application of WPMM has novelty, using a nonparametric Kernel density function approach (and comparison with parametric approaches) to overcome problems in the distribution tails. A clearer comparison and discussion of the two approaches is required when the results are first presented. Categorisation into rainfall types adds further complexity to the method, and brings further issues that are discussed. Application of the advection correction approach also has novelty and draws on relevant literature sources.

Choice of the RIBS model seems a reasonable one, with infiltration excess runoff probably dominating the case study Spanish catchment; but the meaning of return flow needs clarification or a change of terminology.

Overall, the paper is deserving of publication. It could be much improved through detailed attention to the English, so as to improve readability. Thus I recommend provisional acceptance subject to this being done, together with addressing other detailed comments presented below. The amended paper will need to be re-reviewed.

We thank the reviewer for her/his interesting and useful comments to improve our paper as well as for the recognition of the positive aspects of the paper.

DETAILED COMMENTS:

1. **The English needs detailed attention by a native-English speaker: examples of problems on the first page are given below.**

The whole manuscript has been checked in detail by a native English speaking corrector.

2. **7996 line 3 rainfall surface data introduced – rephrase to estimates of surface rainfall used**

The abstract has changed.

3. **Line 5 model results – model predictions**

The abstract has changed

4. **Line 6 composed radar – composite radar**

Done

5. **Line 6 6-minute**

Done

6. **Line 10 in both convective and stratiform Z/R relations – rephrase**

Done

7. **Line 22 for rainfall estimation.**

"on" was changed into "for".

8. **Line 23 on basin processes….especially for convective…**

"on a basin processes" was changed into "on basin processes". The second phrase was rewritten.

9. **Line 24 Is the Bell and Moore (2000) in the References the right one? Think it should be HESS, 4(4), 653-667, (2000).**

Yes, it should be the other. It has been changed.

10. **7998 line 4 Using 1000 km2 as the "usual basin size for flash-flood prone basins" is rather strange, and seems rather large to me.**

We thank the reviewer for pointing out this. The phrase and the paragraph were rewritten as:

> Furthermore, a more recent study has shown that distributed model simulations are statistically distinguishable from the lumped model simulations for basin areas around 1000\,km^2 \citep{carpenter2006ilv}.

11. **line 20 The literature shows many Z/R relations…to more recent ones for different climate types.**

Changed.

12. **Line 28 other methods for obtaining**

Done.

13. **8000 line 12 The sensitivity of time-resolution on distributed hydrological models is addresse in Bell and Moore (2000)**

The reference was removed from the manuscript.

14. **8002 3.1 line 6 Sempere-Torres et al, 2000;**

The reference was removed from the manuscript.

15. **Line 10 "huge sub-estimation" - rephrase – due to gross underestimation.**

Rephrased.

16. **8004 line 27 non-univocal - better to use "ambiguous" as in more common usage.**

Changed.

17. **8005 line 4 commented on previously. Subsequently, the ambiguous relation between…as two independent unambiguous datasets.**

Done.

18. **8006 line 19 where the transformed field**

Corrected.

19. **8007 "return flow" requires closer definition: is it saturation excess surface runoff?**

Return flow refers to the saturation excess surface runoff. It was amended in the manuscript.

> Two modes of runoff generation are simulated: infiltration excess runoff and saturation excess surface runoff. RIBS applies a kinematic model of infiltration to evaluate local runoff generation in grid elements and also accounts for lateral moisture flow between elements in a simplified manner.

20. **8010 line 10 GSA – define as Global Sensitivity Analysis**

We thank the reviewer for this comment. It was added to the manuscript.

21. **Line 21 Nash-Sutcliffe Efficiency (NSE) were selected…..The NSE was used**

This index has been removed from the manuscript.

22. **8011 line 4 T s better written as Ts – also occurs elsewhere**

It was changed in the whole manuscript.

23. **Line 11 of goodness-of-fit tests**

Corrected.

24. **8012 Is it better to change the terminology from BIAS to "log ratio bias" and Error to "bias" (or "mean error")?**

It has been changed to log ratio bias and mean error.

25. **Lines 3-10 Better style to introduce equations sequentially within sentence construct, and not just a list outside the sentence.**

Some equations has been removed whereas other ones has been introduce within the sentence.

26. **8013 line 9 "that are not held between the prediction intervals" – should this read "that are within the prediction intervals"?**

Authors thank this comment that improves the readability of the paper. It was corrected in the manuscript.

27. **Lines 6-15 Again, better to introduce the equations as part of the sentence construct.**

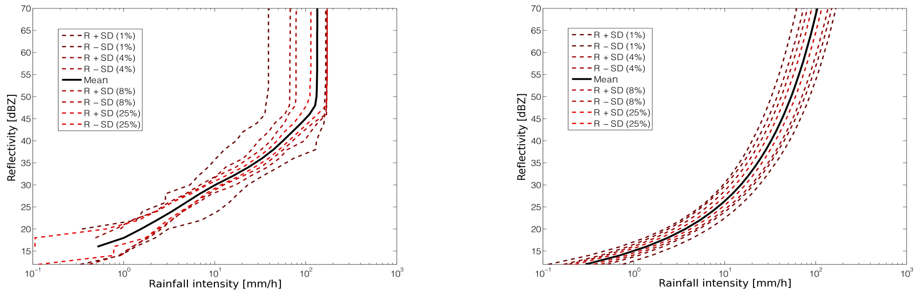
Some equations has been removed whereas other ones has been introduce within the sentence.

28. **8014 line 6 In Table 4 the eight**

It has been corrected.

29. **Line 1-15 There needs to be more discussion here on the relative merits of parametric and non-parametric approaches: this is discussed only later on page 8017.**

We thank this reviewer comment because the introduction of some figures and comments in the result section has emphasized the novelties applied in this contribution.



(a) Non-parametric (b) Parametric

Fig. 10: The new Z/R relation (solid middle line), as obtained from WPMM for the full dataset. The broken lines represent plus and minus one standard deviations from the Z/R when calculated by population from 1% to 25% sub-samples. The upper example (a) is the new Z/R relation obtained by non-parametric fitting whereas the lower one (b) correspond to the parametric fit.

30. line 13 "is own case data" – actually only 2 out of 4 cases. Comment on this case specific calibration in relation to flood forecasting application.

A figure comparing own case data against other case data has been added.

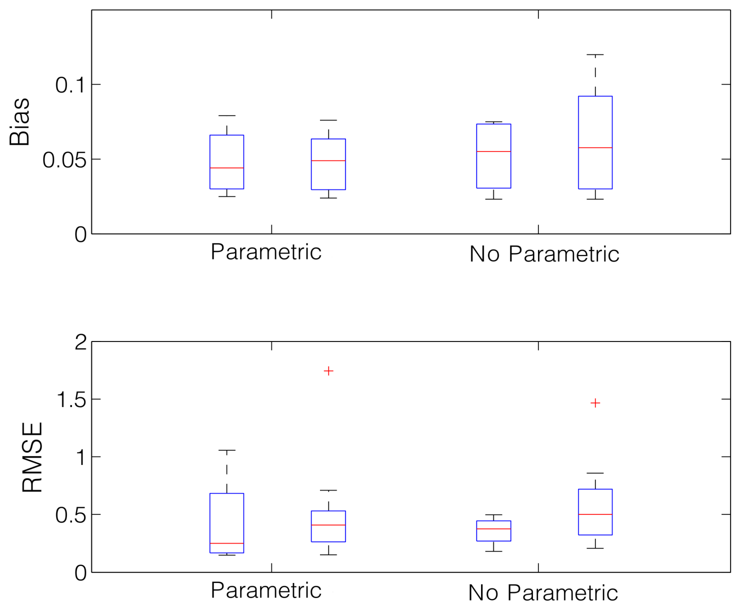


Fig. 9: Box-plot of both fitting techniques (Parametric and non-parametric) for the log ratio bias and the RMSE. The left box-plot for a given fitting methodology represents the results for the Z/R obtained for the same study case whereas the right box plot is the results obtained for the Z/R computed by the three other case studies.

31. **Line 20 Clarify why gamma function chosen rather than non-parametric kernel estimator.**

Besides the figures added, two new paragraphs have been added in the methodology section:

> Regarding the comparison of both methodologies, it can be observed that a parametric fit improves the results compared to a non-parametric fit (Fig. \ref{fig:COMP_BP}). The left box plot for a given fitting methodology represents the results for the Z/R obtained for the same study case, whereas the right box plot shows the Z/R results computed for the three other case studies. The log ratio bias and the RMSE show better results for both box plot regarding the mean and the IQ range. It could be observed that, for the most part, the most suitable data for the calibration are our own case data. Nevertheless, as the results show, the calibration could be performed by using data from other cases that achieve accurate precipitation estimations.
>
> A comparison between both methodologies shows that the parametric fit improves the range of applicability of the new Z/R relationship. It can be observed in figure \ref{fig:ZRSD} that the SD is higher for the tails of the non-parametric Z/R relation (Fig. \ref{fig:ZRSD_a}) than for the parametric Z/R relation(Fig. \ref{fig:ZRSD_b}). This is caused by the scarcity of values at the tails of the probability distribution function for the reflectivity and high intensity rainfall values. The parametric fit does not have this problem because it has only two parameters to compute, and this computation gives more weight to the central values of the distribution.

32. **8015 line 2 at six river gauging stations.**

It was corrected in the manuscript.

33. **Line 13 value for a 15 min time-interval.**

It was corrected in the manuscript as value for time resolutions of 12,15 and 18 min.

34. **8016 line 15 Because of this, an effort was made to couple radar data with a hydrological model for flash-flood cases recorded in Catalonia. This contribution provides a good example of….traditional Z/R power-law…**

It was corrected in the manuscript.

35. **Line 21 caused by the heavy precipitation**

It was changed.

36. **8017 line 1 minimum root mean square error is obtained**

It was corrected in the manuscript.

37. **Line 3 However, the results – not only in the lower tail of the distribution but also in the higher reflectivity tail – show…**

Done.

38. **Line 10 "potential form" and potential factor" – consider change of terminology**

The term has been changed.

39. **Line 14 of reflectivity into rainfall intensity…due to the convex shape of the WPMM function in the semilog….**

The sentence has been corrected as:

> … reflectivity into rainfall intensity, which increases the quantitative precipitation estimation due to the convex shape of the WPMM function in the semilog rainfall intensity -- reflectivity axis.

40. **Line 26 could be related to under-estimation of reflectivity**

The sentence has been corrected as:

> The second correction made by WPMM non-parametric methodology could be related to the underestimation of the reflectivity due to the power parameter calibration or own rainfall attenuation.

41. **8018 better line 3 QPE results**

It has been changed.

42. **Lines 15-17 Clarify how this overall result was obtained.**

The sentence has been rewritten in order to provide this information. The overall result has been obtained by comparing the new WPMM Z/R relation results with literature Z/R relation results. The new paragraph is:

> Comparing the results obtained in the literature for the Z/R relations \citep{ atencia2008nnp} with the results of the combined application of both methodologies, the RMSE has been reduced by up to 40\% and log ratio bias between 75\% and 95\%. These accurate results allow us to couple radar rainfall information across an area-weighted interpolation.

43. 8019 lines 11-12 Could link to findings and discussion of this by Bell and Moore (2000)

The finding in our work has been written as: "This work proves that the highest available rainfall time resolution does not necessarily provide the best results in terms of the predictability of peak flow while the radar system is coupled with a distributed hydrologic model."

The results obtained by Bell and Moore (2000) showed: "that the lower resolution rainfall data generally gave as good flow simulation as the higher resolution rainfall data. This result needs to be interpreted with caution in the context of the rainfall resolution requirements of a distributed rainfall-runoff model. It is cleary more appropriate for a distributed model to represent explicity the smoothing effect of the catchment on the runoff response to rainfall than for the modeller to degrade the resolution of the rainfall data to achieve the same purpose. This puts the onus on seeking improvements to the rainfall-runoff model to make best use of the higher resolution rainfall data."

The rainfall time resolution obtained by Berne (2004) formulae has no relation to spatial rainfall resolution but other formulas give the same results for the 2 km spatial resolution. Consequently, it could exist a link between the findings about spatial resolution and temporal resolution. However, the present study has been focused only on the temporal resolution and the authors would prefer not to suppose this link.

44. Line 18 "a previous analysis" – is the intended meaning "a further analysis"?

The sentence has been changed as: :

> The results for other basins could vary across the Mediterranean due to the dependence of the basin response on other characteristics not analysed in this work, such as geomorphology, geology and vegetation.

45. 8020 lines 22-24 Should this be the paper HESS, 4(4), 653-667, (2000)?

It has been changed.

46. 8029 Table 4. Validation results for the eight Z/R relationships in..

The word Z/R has been included in the caption.

47. September 06 in table

The year has been corrected.

48. 8030 "to the same rainfall estimation" – rephrase to clarify meaning

The caption has been rewritten as: "Comparison of results before and after of applying the advection correction."

49. 8031 Median R2 Efficiencies are low suggesting low model performance – does this deserve comment? There is no visibility of observed and simulated hydrographs to judge plausibility of hydrological model behaviour and performance. This is needed, and to be commented on.

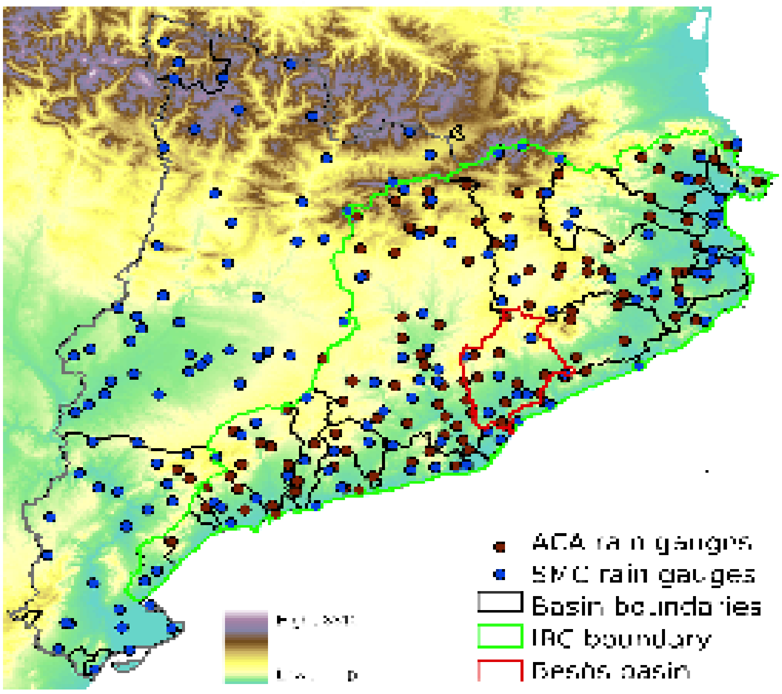
The R2 values are low because the whole event was considered and the simulated receding limbs do not fit fine the observed receding limbs. The R2 values increase if the higher part of the hydrograph were considered and the lower part were ignored. This can be seen in Figure 11 where the median of the simulated peaks are close to the observed peaks.

50. 8032 Fig 1. Boundary of Catalonia with DEM relief.

The caption has been changed.

51. On map key, contour should read boundary. Not clear if Basin boundary is Catalonia boundary. Need to get this right.

The word contour has been changed to boundary. The basin boundaries have been kept in black whereas the Catalonia boundary has been changed to grey in order to distinguish both boundaries.



52. 8033 Fig. 2. Location of river gauging stations:…

The caption has been changed.

53. 8035 Fig. 4. Examples of a

The word example has been well written.

54. 8036 for radar data window

Done

55. 8039 radar rainfall disaggregation

Done

56. 8040 Superposition of radar pixels…domain of area…

The upper-case has been changed.