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Discussions
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Interactive comment on "Simulation of extreme rainfall and streamflow events in small Mediterranean watersheds with a one-way coupled atmospheric-hydrologic modelling system" *by* Corrado Camera et al.

Anonymous Referee #2

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This paper presents a modeling work on 22 small watersheds using WRF-hydro. the model is forced with modeled WRF data and observe precipitation for 2 periods in January 1989 and November 1994. The authors concluded that using WRF precipitation may not be suitable for hydrological studies in small mountain basins although they are still useful for long term studies.

General comment:

I am not quite sure what is the research question that the authors are trying to answer.

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Discussion paper

If it is WRF-hydro ability to simulate streamflow, I think that is widely covered in the literature review, but it may be important a benchmarking in this specific area. If it is the advantage of using observed precipitation, I think that it is not necessary to write a paper about it since it is well known that if there are observations available, it is better to use them over modeled data or to correct the modeled data. Therefore, it is not clear to me what is the actual contribution that the authors are trying to deliver.

The problem with modeled precipitation is because the WRF model does not work? Or the modeler did not implement it correctly? When the model and observe precipitation does not compare well, why did you use it anyway? I think that using incorrect input will certainly result in poor performance. But jumping from there to conclude that we should not use modeled precipitation is a big stretch. I think that the paper should focus on the performance of WRF-hydro. The model performance is not affected if the precipitation is observed or modeled since you are using it uncoupled.

Specific Comment

âĂć First line abstract: “Few studies evaluate the hydrologic performance of coupled atmospheric-hydrologic models when forced with observed rainfall and even fewer when forced with modeled precipitation.” This is not quite true, there is extensive literature on this topic. If you are specifically referring to WRF-hydro you should state that.

âĂć The first paragraph of the introduction is related to the land-atmosphere feedbacks, which is not the case of this study, so I suggest eliminating it or to refocus it to the topic of the paper

âĂć What about the bucket model parameter sensitivity? There is no indication of the interaction of those in the uncertainty analysis.

âĂć Is there any evaluation of precipitation disaggregation eq 1 and 2?

âĂć In Figure 4, there a series of inconsistencies between the performance of the model between the calibration and evaluation period. Is there any explanation for that? In particular basins, MA, An, Pi, Ka, where you have really bad results during the calibration but still validate the models and got very good results.

âĂć Also, it would be better to use percentage bias instead of bias alone to have a more general indicator of biases.

âĂć Figures 5 and 6, it is very hard to see the differences in precipitation. And then, why to use incorrect model precipitation at all.

âĂć If we look at figures 7 and 8, we can see that WRF-hydro does a really bad job (Figure 8) in basins where WRF precipitation is has good performance (Figure 7), can you explain why?

âĂć In the conclusion you mention this “Streamflow obtained with WRF-modelled rainfall forcing showed high discrepancies with observations, despite the good agreement between modeled and observed precipitation (average NSE of 0.83 and 0.49 for Jan 1989 and Nov 1994, respectively).” Did you try to calibrate the model with the modeled precipitation data and evaluate the observation?

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