

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC1  
<https://doi.org/10.5194/nhess-2022-111-RC1>, 2022  
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## **referee comment on nhess-2022-111**

Anonymous Referee #1

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Referee comment on "How uncertain are precipitation and peak flow estimates for the July 2021 flooding event?" by Mohamed Saadi et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2022-111-RC1>, 2022

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### **Review of manuscript "How uncertain are precipitation and peakflow estimates for the July**

#### **2021 flooding event?" by Mohamed Saadi et al., submitted to NHES**

The authors present a modelling study targeted at evaluating different rainfall products and two hydrological models to simulate the flood event of 2021 in West Germany, in order to show the uncertainties in quantitative precipitation estimates (QPE) and the modelling. In general, the study provides insights in the usefulness and weaknesses of the different radar-based rainfall products and their use in simulating extreme flood events. The manuscript is overall well written and structured.

However, I have some reservations to the conclusions drawn, mainly because of the study design, in particular the hydrological modelling part. My concerns are as follows:

Different model parameterizations (ParFlowCLM) and calibrations (GR4H) were derived and later used without any differentiation of their performance simulating the historic period. This is actually hindering a proper evaluation of the QPEs, because poor performing hydrological models might be (or are) used to simulate the flood in 2021 with the different QPEs. I strongly recommend to list the performance of the different model parameterizations/calibrations and sort out poor performing ones. In any case the model performances should be provided by the Nash-Sutcliffe and Kling-Gupta performance measures, because these were already calculated. The claim of the authors that the model parameterization/calibration has a larger impact than the QPEs is not that surprising, considering the sensitivity-analysis-like selection of the parameters and calibration routines. The conclusions towards selecting a particular QPE would be more meaningful, if only well performing models for flood events (high discharge) during the calibration period would be used.

The parameterization of ParFlowCLM with uniformly distributed roughness values is very unrealistic for these catchments with diverse land uses, i.e. land surface properties. I am surprised that such a simplistic approach is used for such a sophisticated, physically based and spatially distributed model. Thus I strongly recommend to re-run the simulation with distributed roughness values estimated based on land use and standard roughness values, as mentioned in the outlook. This would give the ParFlowCLM simulation much more credibility.

For the GR4H model I find using the calibration not focussing on extremes for the analysis of the QPEs not convincing, because a conceptual model calibrated on mean flow is unlikely to get the peak discharges of floods right, and should thus not be used for evaluating the QPEs. You might prove me wrong listing the performance values.

Furthermore, some of the comparisons/evaluations of the QPEs and simulations are based on comparison with uncertain or unknown quantities. The missing flood hydrographs are a major obstacle here. Meanwhile reconstructed flood hydrographs are available at least for the catchments in Rhineland-Palatine by the Landesamt fuer Umwelt (LfU). Similar data should be available from the authorities in Northrhine-Westphalia. These hydrographs can be seen as the best estimate of the actual flood event. I strongly recommend to obtain these data sets. This would increase the impact of the evaluation in terms of ability to simulate the flood 2021 significantly.

Another point: the comparison of the catchment average precipitation used the Thiessen polygons as reference, but these values are also very uncertain. Thus, the general statement that some of the QPEs outperform RADOLAN in catchment average is actually not supported. You only show that these products are closer to the uncertain catchment average based on rain gauges. Which of the QPEs is actually closer to reality cannot be derived from this comparison. This should be mentioned.

I am also missing the discussion of hydrologic processes that might become relevant or only occur during extreme floods. This is a generally ongoing discussion in hydrology, but for this particular event the increase interflow and thus runoff generation by field drainage pipes or the creation of additional drainage channels by erosion has been reported. Unfortunately, this is not published yet, thus you cannot cite it, but there should be reports in newspapers or by the authorities available.

The role of the antecedent soil moisture has been briefly discussed in the manuscript, but studies for its impact on flood generation has been given as an outlook only. I wonder about two aspects: First, the used initial soil moisture for the simulation of the flood 2021: what initial soil moisture was assumed? Was it assumed dry, a guess of some wetness, or maybe based on satellite observation? Or did you use the hydrological simulations until the event to prime the model for the flood simulation? In the latter case the antecedent soil moisture should be realistic to some extent. If assumed, some justification or at least explanation has to be given. Second, an interesting aspect would be if the flood would

have been different if the soil was in different state (drier, wetter) than in reality. You mentioned this in the outlook, and this is surely worth investigating, as the role of antecedent soil moisture is likely to differ in different flood/rainfall situations. If you have any capacities, I recommend to include this aspect, and drop the discussion of the simulation results of poor performing models.

In addition to these general comments, I have some more specific comments in the annotated manuscript.

Please also note the supplement to this comment:

<https://nhess.copernicus.org/preprints/nhess-2022-111/nhess-2022-111-RC1-supplement.pdf>