Review egusphere-2024-2722

General comments:

In this study three hydrological models are applied to on the West-Bank of Palestine. Because of little data availability, satellite images of the inundation extent of a flash flood in 2013 are used to validate the models. Future rainfall scenarios for the West-Bank are derived from GCMs via down-scaling and then used as input for these models to investigate possible future flood scenarios.
The manuscript is well written, clearly structured and to the point. At the same time the manuscript provides enough detail to follow the methodology and deals thoroughly with the data, e.g. the use and comparison of different DEMs.
Providing solid flood risk management in regions with little data availability is an important topic, especially in regard to climate change and the expected increase of flood magnitudes. This study points out that creative solutions in these regions are required and offers the use of satellite imagery as one possible solution.
While I really like the practical and applied nature, I also have some questions and criticism to address, mainly the use of apparently uncalibrated models which are even more simplified with the use of just one (?) mannings roughness value for the whole study area.
However, after discussing following aspects, I recommend this manuscript for publication.

Specific comments:

- Study area: I am missing information about the land use within the study area. You are using the NDVI for the validation of the flood extent. Is the catchment mainly characterized by agricultural land use? It would be nice, if you could add a little description about the land use percentages in the study area.

- Validation via NDVI: While flood damages on crops is relevant, one main focus of flood risk management lies on the protection of urban areas and residential/industrial buildings. How does the NDVI help to identify inundated urban areas? How do you explain the NDVI change in urban areas in Figure 5? In the abstract you point out “populated urban environments” (with usually little vegetation) but your method can only detect changes in vegetation. This remains somewhat unclear throughout the manuscript and also relates to previous question. How well is the NDVI suited to detect flooding in urban environments? In my opinion you have to elaborate more on this point.

- Discussion: You could not validate the inundation depths with your approach. However, inundation depth is one main factor regarding damages. This should be discussed.

- Line 51: “NASA Global Precipitation Measurement” and antecedent soil moisture conditions via radar: I am highly skeptical about the quality of these products. Remote sensing of soil moisture is very uncertain and can just give information about the upper layers of the soil. The spatial resolution remotely sensed precipitation is too coarse to be effectively used in flash flood modelling were affected basins are usually small and spatio-temporal distribution of rainfall is decisive for the formation of flood peaks. Please discuss this, e.g. in the following sentence (“Barriers to earth-observation ….”).

- Line 184: Did you really use just one mannings roughness value for the whole study area? Why? You could derive these also from land use / or satellite data. How do you justify this enormous simplification of surface runoff processes? This is one main point of my criticism.

- Which parameters are needed to run the three hydrological model? How did you derive these parameters? I understand that there is no way to calibrate the models but did you just use standard settings for the models? Please be more transparent about this and explain how you derived the parameters. While your models might be able to replicate the extent of the 2013 flood, the might fail to reproduce the correct inundation depths and flow velocities, which are both crucial parameters regarding the flood impact on buildings. This is another main point of my criticism.

- Figure 5: I find the differences between fluvial and non-fluvial intersection quite small. (Fig 5b and c)). What could be the explanation for this? I would expect, that the inundated areas at fluvial intersections are much larger.

- Table 3: It is good that you use to different climate projections as they are really uncertain. Especially the GFDL-ESM4 on the SSP5-8.5 scenario seems quite strange with a decrease in rainfall in the mid-future. Is this an artifact?

- Table 4: How did you derive the return periods? How long was the time series you based this calculation on? Please clarify.

- Line 390: What do you refer to here, if you speak about opportunities for groundwater recharge? During flash floods infiltration is negligible. Do you mean infiltration from ponded areas after the flood? Also there seems to be an issue with causality in this sentence: “Flash flooding […] could reduce flood hazard”?

Technical comments:

Line 28 ff.: I think there’s something wrong in this sentence. How is a warming climate driven by higher magnitude flood events?

Figure 5: Why the label “Open water”. You can not see it in the three figures. I wonder if a simple table would be even easier to interpret than this figure.

Figure 8 and 9: Can you increase the resolution of these plots?

Figure 10: The differences in flooding extent are very hard to see. Maybe a table with the numbers would be enough?