Review of « Spatially dependent Intensity-Duration-Frequency curves to support the design of civil infrastructure systems »

GENERAL COMMENTS :
The authors have significantly modified and improved the manuscript, taking into account the reviewers' comments. However I still think that the manuscript needs to be improved on three main points before publication (see below for the details and additional comments):

- The presentation of the inverted Brown-Resnick model is still unclear (Sections 4.2 to 4.4). I think it will hardly be understandable by the readers of HESS.
- Section 4.5 and 4.6 should be better motivated beforehand. Personally I understood these sections only when reading Sections 5.2 and 5.3. The few sentences at the beginning of Section 4 and Figure 14 are not clear enough for me to understand what are the needed mathematical ingredients.
- Several equations lack consistency (see below).

DETAILS :
(The lines refer to the marked version)

- The title hasn't been changed (unlike written in the response to Major Comment 1). Anyway, even with « relationships », I still find the title confusing with regards to content of the article. Why not « Spatially dependent flood probabilities to support ... » ?
- L 63-64 « to overcome... used » : repetition with the previous sentence
- L 138 « the lack of dependence » → the underlying independence assumption
- L 145 « preserve dependence » → account for
- L 149-150 : could you eleborate more on the difference between copula and max-stable processes ? Why did you choose to use MSP rather than copulas ?
- L 166 « spatially dependence IDF curves » → I haven't seen such curves in the manuscript
- - L 262-273: This is a list of what you'll do in the next sections but we don't understand **why** you'll do that (what are the goals?). Please rewrite.
- L 269 : « to transform conditional rainfall to conditional flows » → This is confusing. I think you transform quantiles, not the absolute values.
- L 271-273 « An analysis .. comparison » : Actually I don't see any comparison with the independent model (apart in Fig 10). Please remove it from Fig 4 as well.
- Figure 4: "probability **of** rainfall" , "conditional **probability of** flows", "assume 1:1 relationships **for the probabilities**", joint flood probability → probability of system failure (give the section number)
- Sections 4.2 to 4.4 should be partly rewritten and reorganized.
- L 295: please specify that Z is associated to a given duration
- L 297-298 "without loss... distribution": I don't think that the reader will understand why one can assume that Z is unit Fréchet distributed. The transformation should be given.
- L 305 "An example … process": Yes but the Gaussian process is another example of AI model. What is the advantage of the inverted BR model with respect to a Gaussian process?
- L 308-319 "A general … margins": this is not understandable for the great majority of HESS readers. Anyway there is a lack of consistency because tin the construction (2), margins are assumed to be exponential.
- Eq (4): Again this lacks consistency: written as such, you assume that Z has uniform margins. What is y in the limit? For importantly, what does eta represent **in practice**? This will stay obscure for most of the readers.
- L 346-360: this is a very complicated way of saying that the dependence depends not only on the distance but also on the duration. Please make it shorter and clearer. The reference to

the time of concentration is confusing because it was nowhere said that you will consider for the duration the time of concentration of the basin. By the way, do you only consider that duration later?

- L 377-385: this part (at least the joint distribution) should come before in Section 4.1. Does Eq. (7): apply to any z1, z2? I guess it applies only to threshold exceedances.
- Eq. (9): I'm confused here. (9) seems to implicitely use P(Z1>z1)=1/T1 with z1 the T1 year return level for Z1. However is that true? I though that Z1 and Z2 were threshold exceedances, whereas P(Z1>z1)=1/T1 applies if Z1 is an annual maximum, doesn't it?
- L 428-429 “the joint probability … marginals”: it could also be specified that in case of independence conditional=marginal.
- Eq. (10): is this useful? I don't think you use it anywhere... Anyway, if you specify this probability in the case of independence, you should also give it for the IBR process.
- L 440: A better title might be “Simulation-based estimation of ARFs”
- L 463-465 “the empirical distributions … thresholds”: I don't understand how an empirical distribution can be derived using a response surface since by definition it is not parametric! And what about above the threshold?
- L 474-475 “36 and 6 h durations”: only? Other durations are shown in Fig 6...
- L 475-476: “ARF are calulated”: I would like to have here a clear explanation on how it is calculated because it is not clear to me.
- L 500-508: I'm a bit lost here because you seem to be able to simulate rainfall in space (see Section 4.5) so why don't you directly simulate rainfall and compute the basin accumulation rather than simulating at the centroid of the catchment and then using the ARF to transform it into a spatial accumulation? My concern is that this may introduce a bias.
- L 553 rainfall extremes → rainfall return levels
- Fig 10: I don't understand what “% AEP” means. Isn't it just “%”?
- L 746: Shouldn't “P(Z2>z)” be “P(Z2>F_Z(u))”? Idem in Eq. (A.1)