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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A global approach to defining flood seasons" *by* D. Lee et al.

Anonymous Referee #3

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I general I think this manuscript lacks a coherent structure. Firstly, the introduction is mainly focussing on seasonal and long range forecasting, whereas the work actually reported is mostly concerned with classification of runoff time series. Critically, there is little or no discussion of other existing classification schemes, especially why they might not be adequate, and as a consequence it is not clear what scientific knowledge gaps is being addressed here. Secondly, I don't think there is much scientific merit in the comparison between the observed and simulated runoff series. Especially in section 4.1 where it is reported that the observed and simulated FS only share the same three months at (only?) 40% of the considered time series. Importantly, there is no discussion of what the authors would suggest is a lower limit of acceptable performance. It would have been more interesting if the mismatch between the observed and simulated series had somehow been used in a more quantitative assessment of the reliability of the model predictions. As it is, it seems like the performance has been

accepted as it is in order to enable the production of some global map, but the usefulness (or reliability) of these maps is not really discussed. In my opinion, this makes the outcome of the study seem too open-ended with no firm conclusion, which is also partly down to the lack of a clear hypothesis in the beginning (i.e. identification of a knowledge gap). Finally, I think the presentation of the methodology could be made more refined. In the current version it reads, I think, too much like a working paper where the individual sections are reported in the order that the authors encountered and fixed problems. Maybe group together 3.1, 3.3 and 5 to first present a coherent methodology and then apply it to the two datasets?

Specific comments: Section 2.2: Was the PCR-GLOBWB model calibrated against observed streamflow data? Page 4600, line 17-18: I think the POT model was proposed somewhat earlier than this - see e.g. Shane and Lynn (1964) or Todorovic and Zelenhasic (1970) Page 4600, line 25: What is meant by 'bi or multi-model flood conditions'? Page 4601, line3-4: Is this really a volume-based threshold? Seems to me it only considers a particular threshold based on daily runoff data. What part does volume play in this? Page 4601, line 15-: The high degree of correlation is to be expected as these different criteria are extracted from the same dataset using only minor variations in threshold levels. However, I don't understand the statement that this should somehow indicate successful success in capturing volume and magnitude. Please clarify (see also comment above). Page 4603, line8: The statement that seasonality if often used to delineate catchments is backed-up by three (out of four) references to the same (excellent) research group. However, I don't that is enough to suggest that it is often used. Also, how did these publications define seasonality? Page 4607, line7-9: Why are seven references needed to state a well-known fact? Page 4608, line 7: what unit does 'cms' refer to? Page 4608, line 25: why does (50%) refer to? Page 4609, line 7-15: This reads more like the motivation for the study than a conclusion of the work undertaken. I think this belongs in an introduction.

References Shane, R. M., & Lynn, W. R. (1964). Mathematical model for flood risk evaluation. Journal of the Hydraulics Division, 90(6), 1-20.

Todorovic, P., and E. Zelenhasic (1970), A Stochastic Model for Flood Analysis, Water Resour. Res., 6(6), 1641–1648

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