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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "The influence of antecedent conditions on flood risk in sub-Saharan Africa" *by* Konstantinos Bischiniotis et al.

Anonymous Referee #1

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The authors investigate the influence of high-intensity rain events and antecedent moisture conditions on flood probability in a large target area, including almost the entire African continent. Based on a data set of reported floods (provided by Munich RE), the short-term (event-precipitation) and long-term conditions (SPEI) before each event are systematically compared. The results indicate, that most of the reported floods are related to high precipitation events during the last seven days. Further the authors argue, that rather moist conditions on seasonal scale lead to enhanced flood risk, most likely due to filled up storage systems. While the research target is timely and the manuscript is well structured and easy to follow, I have some serious concerns about the statistical methods and the interpretation of the result. Particularly the conclusions remain very

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vague! Thus I recommend to extend the statistical analysis and to better test, whether the conclusions are robust and really supported by the underlying data sets.

In the following I will summarize my major concerns. Since I expect the text to change significantly, I will not go into detail at this stage of the review process.

1) Introduction and data sets: The presentation of previous research in the field of flood forecasting in Africa is very short. No information is given on the timing of floods in different subbasins of this vast target area and on the general climatic conditions. This information would be highly valuable in order to interpret (and scrutinize) the results of the statistical analysis. (E.g. It could be interesting to identify some differences between Eastern and Western Africa, which seem to behave differently in terms of the SPEI-flood relationship.) Likewise the introduction of the data sets is insufficient. I would expect a detailed description of the advantages and drawbacks of the data – especially the daily precipitation values on a coarse grid are extremely uncertain, since they do not cover local scale convective events, which frequently trigger high-intensity rain. The Munich Re data are introduced in two sentences only – again I think it would make sense to better discuss its origin and shortcomings!

2) Statistical Methods: The majority of the methods used is purely descriptive and does not allow to draw verified conclusions. One example is Fig. 9., which shows the mean seasonal SPEI values before reported floods. The authors argue, that floods, which are not preceded by high-intensity rains (0-33%-interval) have larger SPEI-values during antecedent seasons. However, all of the lines are very close to each other. A test for statistical significance (t-test or similar) would be necessary to support this statement. Further a presentation with boxplots would be more suitable, since it does not only show the mean, but also the range (and overlap) of the different classes.

3) Dependency of SPEI and 7-day precipitation An increased SPEI value before flood events might have different reasons. One would be the limited capacity of the storage. A second one could be the persistence of the climate (if it has been moist for the last

couple of months, a high-intensity rain event might be more probable). That would explain, why the 66-99% interval in Fig. 9 shows the highest SPEI for all seasons. In order to draw robust conclusions, it would be necessary to disange those processes. In Fig. 10, the frequency of flood under different SPEI combinations is shown. The analysis of further combinations (e.g. SPEI0 → normal and SPEI3 → moist) could support the conclusions of the manuscript. Again, a test of statistical significance is highly recommended.

Would it further be possible to show a point-cloud of seasonal SPEI against 7-day-precipitaion for all flood events? A clear negative relationship (higher SPEI values lead to flooding although 7-day precipitation is not extreme), would also support the conclusion.

4) The authors highlight, that a forecast based on the findings is possible and that uncertainties could be reduced. I have the feeling that this is very optimistic. Would It be possible to establish a very simple tool for each of the FPU-units (e.g. based on a SPEI threshold value) and quantify the probability of hits and false alarms?

5) Conclusions and discussion: The conclusions and the discussion section include many statements, which are not proven by the data or by literature (E.g. second paragraph, page 15, but also others). I recommend to carefully check, and to focus on findings which are really supported by the data.

Minor remarks: 1) Section 2.1 and 2.2 could have more meaningful subtitles. 2) p.5l.5: The weather scale and SPEI periods do not overlap and the SPEI period lasts until the date of the weather scale period. This is not possible, since the SPEI is defined on a monthly time scale? Does the SPEI period end with the month before the flood event? 3) Fig. 2: I am confused about the hydrograph. Is this a schematic figure or is it somehow based on discharge time series? If I understand the figure correctly, discharge already increases seasons in advance. Usually the start of a flood event is defined as the first significant increase of discharge (which would be 5 months in

advance in Fig. 2). 4) p6.l14: Floods are grouped into wet and dry seasons? How exactly is this relevant for the statistical analysis?

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