

Note: The original comments by Referee 1 (R) are in regular text. Replies by the authors (A) are colored in green and changes in the text are in italics. Minor remarks on orthography etc. have been changed accordingly to the comments but are not listed here.

A: We thank the Referee for the constructive critic. With its help we could improve the manuscript substantially. Especially the reorganization of the discussion and the reduction on three regions helped to focus the paper clearer and increase the readability of the paper.

R: General comments: This paper presents an informative and authoritative analysis of the flooding dynamics in the Niger River basin for the past century. It systematically approaches a variety of hypotheses in order to test the explanatory power of possible causes. Moreover, it provides a good introduction to related publications. It is generally a well written paper which deserves publication in NHESS. I suggest the paper should undergo minor revisions before publication. In general I think the paper is lacking in the following:

1. I think the selection of the “Sudanian” region is questionable. A large part of it contains areas classically considered as “Sahelian”, it contains different geology (rightbank & left-bank), and many of the results are very similar to the “Sahelian” region (e.g. Fig 4,6,7,8). Hence, it does not add much information. Therefore I suggest to remove this region altogether.

A: The problematic definition of the Sudanian region as already discussed in the manuscript was also noted by referee. We agree with both reviewers. Therefore the analysis of the Sudanian region was removed from the manuscript. All the relevant sentences have been deleted or changed. In addition, as stated by the referee, the results of the Sudanian region were very similar to the Sahelian region. This supports a broader interpretation of the Sahelian results also for the Middle part of the Niger to some extent. This has been considered in the restructured discussion.

2. The discussion section is mostly a synthesis of the results and lacks a broader analysis of the implications of the results, and how this relates to hydrological and water management theory. Also, it would be interesting to discuss the generality of the method for other areas. The conclusion makes a small note on policy, which is good and could possibly be expanded in the discussion section. I suggest to rewrite it with a much broader perspective. In some parts the discussion is repetitive (relative to previous sections), which is boring to read.

A: We thank the referee for the suggestion for a broader interpretation of the results in the discussion. We extended it and discussed the constraints of our approach and a data-based attribution in a more general way. The implications have been pointed out clearer and the note on policy has been expanded in the conclusion section. For both chapters we removed redundant parts.

Specific comments:

P5172, L2: "addressing" suggests action (e.g. policies), which is hardly the topic for this paper. Rather the paper "analyses" or "investigates" the flood risk evolution / factors contributing to increased flood risk

A: We agree and changed the word.

P13009, L2: *"This study addresses the increasing flood risk in the Niger basin..."*

P5172, L6: "even indicates" to make sure the reader does not think the exponential trend is even..."

A: The sentence was changed accordingly.

P5172, L7: "Can it be quantified? What is the average increase, or what is the exponent per year?"

A: We agree with the author that the statements in the abstract should be backed with numbers and added it where possible.

P5172, L7: "correlate? (+ correlation coefficient)"

A: Please see answer to previous comment (P5172, L7).

P5172, L9: "can you quantify the change in AMAX for each region (e.g. average increase per year since 1980s)?"

A: Please see answer to comment P5172, L7.

P5172, L9: "can you quantify?"

A: Please see answer to comment P5172, L7.

P5172, L10: "based on what index? if possible give some numbers"

A: Based on the statements of different papers (e.g. Di Baldassarre et al. (2010), Tschakert et al. (2010)) therefore we cannot quantify it.

P5172, L15: "Yes, 1970s & 80s were very dry years, and the 60s and 50s wetter. In fact a lot of your paper goes on to explain this in detail, which is a major contribution in my view. Perhaps it could be stated even more clearly; when reading 1980s above it first seems your analysis is limited to that period. So it would be good to indicate that you actually analyze much longer records (1910?)."

A: We thank the referee for the suggestion and included this information in the restructured abstract.

P5172, L15: "can it be quantified how much of the increase is likely due to precipitation increases?"

A: No, as mentioned in the conclusions therefore a modelling approach would be necessary. The data-based approach is in the case of the Niger limited,

P5172, L18: "can it be quantified?"

A: Please see answer to comment P5172, L7.

P5172, L19: "+ that the "Sahelian paradox" is no longer happening."

A: This finding is added to the abstract.

P5173, L4-5: "This seems to be similar to this study. Could it be discussed further (in discussion section)?"

A: Yes, however the focus on these papers is only on the rainfall patterns and in their conclusions they mentioned that the changes may have influence on flood occurrence. This has been added to the discussion, especially Panthou et al. 2014.

P5173, L20-21: "well this is not really done here either, or?I did not see any figures for dollars/euro or even lives lost. Perhaps I just missed it..."

A: Damage was meant in the way of people affected as there are no other numbers. The inadequate term "damage" has been exchanged.

P5173, L20-21: "Still, the role of climatic variability for the increased flood risk in the Niger basin has not been systematically addressed; neither has the associated damage impact on people been systematically collected and quantitatively evaluated."

P5173, L23: "This is interesting. You clarify what value represents, but what about the others? It would be useful for the reader to understand the concrete aspects of your study that fit into each of these categories."

A: The study is focusing mainly on the hazard. The vulnerability aspect is only covered in regard of population density and studies on vulnerability in the region. This information has been added to the introduction.

P5174, L8: "A large part of this area is classically considered as Sahelian as well (e.g. by experts in Niamey). Also it spans both the right and left sides of the main river which have different geology. I suggest to reconsider the area selection."

A: Please see the answer to first main comment.

P5174, L9-26: "All this is method. It fits better there (as an intro to each step in the method)."

A: We agree and the part was added to the method like suggested as intro.

P5175, L11: "This does not show anything about rainy season... Guinea is visible in Fig 1."

A: The reference to the figure was changed to Figure 1.

P5175, L11: Also here, Fig 6 is about discharge probability. Not rainy season! If you mean the figures by Descroix et al. then you must write that explicitly. Or simply have the reference without these confusing Figure notation."

A: The reference was wrong and has been deleted.

P5175, L19: “again, strange figure references. If Descroix coined the phrase, then it's enough with a reference to the paper, not specific figures in the paper.”

A: The reference was wrong and has been deleted.

P5177, L6: “Does any dataset provide a longer history (e.g. 1950s-1960s?). It would be a very interesting analysis to see if any of these datasets also capture the climate variability thesis (“return to wet conditions”) put forth in this paper. If possible I suggest to do such an analysis and put in the Supplement.

A: The earliest information comes from the EM-Disaster list and is from the year 1974. Unfortunately it is not possible to analyze if there have been more floods and also people affected during the wet 50s and 60s, though we agree that this would be very interesting.

P5177, L7: “Needs definition.

A: The definitions for the datasets have been added.

P5177, L7: The EM-disaster data base distinguishes between “general river flood” and “flash flood”, the NatCatService and the dataset of the Dartmouth flood observatory between “flood” and “flash flood”. None of them is specific about the exact definition of flash flooding. Since the majority of the information is derived from media, the discrimination between the flood types is most probably not homogenous, however the rough differentiation is assumed to be correct.

P5177, L12: “This comes at an odd point (as if the figure showed something about Sudanian and Sahelian zones only). Rather I would put it at the beginning/earlier in this paragraph and state the main message: people affected by floods is increasing from 1980s to 2012.”

A: We apologize, the reference to the figure was wrong and has been deleted.

P5177, L18: “It would be useful to indicate where these are located on one of the maps, and how many you used in the flood analysis.”

A: The positions of the stations for which data has been analyzed have been additionally marked in the map (Fig. 1).

P5177, L23: “How did you treat missing values? Simply ignoring those days or some interpolation? Please clarify”

A: Missing values have been ignored. We added this information to the text.

P5177,L23: “Missing values have been ignored in the analysis.”

P5177, L25: “I'm skeptical these should be treated separately, given my comments above about the area. Here you also seem to group them together, and at least some of the results are very similar in these regions (e.g. Fig 4,6,7,8).”

A: We agree with the author (see longer answer in the main comments) and took out the Sudanian analysis.

P5178, L11-13: "1. I interpret this to be what the Fig S1 shows. If so the reference should be after this sentence, not before. 2. Also please provide some brief idea of how you interpolated this (possibly in the supplement). 3. Please also see my remarks on discharge vs precipitation evaluation in the supplement."

A: 1: The reference was replaced. 2. We added the explanation of the interpolation to the methods. 3. The incorrect axis-labels have been replaced.

P5178, L11-13: "The reanalysis data was interpolated via the Inverse Distance Weighting method to the location of the stations, and it shows good performance with regard to annual and heavy precipitation (Figure S1, Supplementary material)."

P5179, L20-21: "This is unclear. Should you not subtract the trend to get the residual (not the other way around), and then do the analysis on the remaining variability?"

A: Referee #2 questioned the method profoundly and we cannot proof its suitability. Therefore the whole abstract is removed.

P5179, L27: "I'm not an expert in this and probably many readers are not. Hence it would help with a brief explanation on the purpose of this function and the SN and AIC methods applied. What is the contribution of the cost function (etc.) to detect multiple changepoints?"

A: Cost function is a statistical terminology, also known as loss function. It represents values onto a real number representing some "cost" associated with the event. This function is then minimized in order to find the optimum for the "cost". We agree that the description was too short and the explanation is more detailed now.

P5179, L27: "*C is the cost function of the time series segment $Y(t_{(i-1)+1}):t_i$ and $\beta f(m)$ is the penalty function. The cost function relates to the "cost" of segmentation.*"

P5183, L1-2: "well, why not just fit an exponential function to this data to check?!"

A: We tried that however as the fluctuation is too high, the fitting was not significant. Since the expression can be misleading, the sentence was removed.

P5183, L11: "Well, you say the data is based on newspaper reports; so one can expect a bias toward cities in the reporting. This should be mentioned & discussed"

A: We agree with the author and added this aspect to the discussion.

P5183, L11: "*Another open question concerns a bias in media coverage, which might have increased during the last decades and could result in an increasing number of flood reports. Tarhule (2005) addressed this issue for the region around Niamey by comparing flood reports in the media with rainfall data and concluded that the quality of the environmental reporting of the newspaper is good in the*

Niamey region from 1970-2000. This cannot rebut the hypothesis of change in media coverage however as all data sets rely mainly on newspapers if gives more confidence on their consistency during the last decades. Another aspect of the media coverage bias is the better coverage of urban areas compared to rural areas. However, as we do not analyze the spatial distribution of the flooding on the subregional scale, this bias does not affect the analyses directly.”

P5183, L18: “So then you don't need these in the legend either. Please simplify.”

A: The legend with all stations was a relict and has been updated.

P5184, L8: “already defined. Not necessary, but ok if you want to keep it, same for WFDEI”

A: At the moment acronyms are defined again in each chapter for a better readability. We will discuss this again with the typesetter.

P5184, L20: “good to test. Could you also plot this in a scatterplot (supplementary)? That would be even more informative (as the relationship may not be linear or monotonic).”

A: We thank the referee for the suggestion and added a scatterplot for the three region in the supplementary material (Figure S4, Supplementary material) and discuss the correlation in more detail later.

P5184, L21: “Could the explanatory power be higher in densely populated areas? Is the residual related to areas/communities better adapted to regular high-flow events?”

A: There is no information about the quality of the flooding data, however this is now pointed out more in the discussion.

P5184, L22: “Why? Would be good with a small discussion point on this.”

A: Please, see answer to previous comment, the point is discussed now.

P5185, L2: “? the delay and spread of the water in the IND?”

P5185, L2: Yes, water is accumulating in the IND but the outflow is limited. The IND fills up depending on the precipitation in the Guinean region and due to the limited outlet the peak of the outflow is delayed in relation of the accumulated water. The explanation following in the text has been now connected more clearly to the basin effect.

P5185, L2 -7: “This means water from the Guinean subregion accumulates in the IND and only a limited amount can pass through the outlet near Diré. Accordingly, the delay in AMAX at the affected downstream stations is dependent on the total amount of water in the Delta. Due to this correlation, the positive trend in AMAX causes a significant delay in the timing of the AMAX of the Guinean Flood in and downstream of the IND.”

P5185, L7: “please indicate the direction as well (earlier/later)”

A: Please, see answer to previous comment and the changes in the manuscript.

P5186, L15: "ok. well you could also show the peak/mode of the distributions over time to illustrate this. Also for Koulikoro with very long time series. --> supplement"

A: Since the long time series change their "mode" several times from wetter to drier periods the formula gets very complex and loses the explanatory power. Therefore we only show the shorter period with change from a wetter to a drier phase and back.

P5186, L1: "Complex sentence. consider revising/expanding the explanation"

A: Since the analysis was adapted following a suggestion of referee 2 the explanation was rephrased completely considering the readability as mentioned by the referee.

P5186, L8: "How would a significant change be shown? By an appearance/disappearance of a strong wavelet coefficient at a certain period over time? Enough to say something in the Supplement figure on this."

A: As the additional time series for the Sahelian tributary shows changes in the frequency, the effect is explained in detail in the text now.

P5186, L26: "Nice numbers to have in the abstract."

A: The numbers have been added to the abstract.

P5187, L2: "Please indicate the correlation coefficients between AMAX, mean P and 95th percentile as well."

A: A table with the coefficients has been added and the results integrated into the discussion (new table 2).

P5186, L24: The annual and the heavy precipitation are strongly correlated with the AMAX for all regions (Spearman's ρ : 06-0.72) (Table 2).

P5187, L9-10: "Many paragraphs start with this description, which is not really necessary. You could make the manuscript more concise by removing this and instead putting the figure reference at the end of the next sentence. Applies to all paragraphs of this style. Not necessary, but will make it more concise and faster to read."

A: We thank the referee for the suggestion and avoided redundant explanations of figures.

P5187, L13: "Why? Perhaps because the Sahelian basins (e.g. Sirba) contribute more to the total river flow; and these are not dampened by the IND --> increasing flashiness."

A: This might be the explanation though Koulikoro is neither influenced by the IND. This is discussed in the reorganized discussion in more detail.

Discussion: *“We assume that this discrepancy is caused by the effect of the uncertainty in the absolute numbers. Since in the Guinean catchment the absolute numbers of people affected by floods are lower by a level of magnitude, the low quality of the data has more influence.”*

P5187, L17: “I would like to see more discussion of the possible use of AMO, e.g. for prognostic predictions or climate scenarios. How can AMO be calculated (from NWP models or AOGCMs? Satellites?)”

A: As referee 2 pointed out, the links between the West African Monsoon and other “atmospheric structures” are very complex. If we would extend the discussion here, the focus of the paper would have to change. Therefore we decided to remove the analysis of AMO of this paper and used it only to show the consistency of AMAX with the decadal pattern.

P5188, L11: “could be in supplement”

A: We would prefer the figure in the regular paper so the reader can better understand the different dynamics. However if necessary the figure can be put into the supplement.

P5189, L12: “A large part of this section is simply summarizing the results again. That is unnecessary. Rather the discussion should focus more on the implications of the results. It seems some points are also coming back from the introduction, which is boring to read again. Consider to only have it in the discussion, to make it more of a story. For example the population trends could probably be saved for the discussion only.”

A: We restructured the discussion completely as advised in the main comment.

P5190, L15: “As I understand the NSGEV analysis, it deliberately chooses the simpler model unless the data fits a more complex model better. So even if they fit about equally well the simpler model would be chosen. If so, then it is too strong to say that the regime does not change based only on that built-in bias for stationarity. Somehow you must also show that the complex models are much less likely than the simpler model to give confidence in this kind of conclusion. Perhaps you have results for this that could” [sic!]

A: This is correct. Due to the new results of the analysis of the Niger tributary for the Sahelian region, the discussion on the NSGEV has been accordingly adapted.

P5191, L20: “A contributing factor is probably also that the local tributaries are not dampened as much (no IND). With more runoff due to the landuse change the change in flashiness is even stronger than if a large wetland would have dampened it.”

A: We agree and added this point to the discussion (please compare answer to comment P5187, L13).

P5192, L26-27: “since these have not been properly linked to your analysis (see point above), this conclusion does not come naturally for the reader.

A: This has been addressed in the restructured discussion.

P5204, Figure 1: "I cannot imagine that the three datasets have the same geographic unit for reporting, nor that this would be the Niger river basin. So there must be some summation/interpolation to obtain these numbers. This should be clarified."

A: The datasets comprise data mostly on provincial level. These were summed up for all provinces in the Niger basin. We added this explanation.

P5177, L10: "The numbers for the whole Niger basin or the individual regions have been summed up from the provincial or village level information."

P5208, Figure 7: "What's the purpose of this? To me it would be enough to show the timeseries and the Theil-Sen trend."

A: We agree and removed the Loess curve in order to improve the readability.

Supplement:

Figure S1: "The figure y axis says "discharge". Is this river discharge or precipitation?! The evaluation should be made relative to meteorological precipitation observations. Discharge can be a complement, but not a substitute"

A: We apologize, the label was wrong. It should indeed say precipitation, not discharge.

Figure S2: "Legend needs to be expanded to explain the lines, the circle with crosses and the black bars. Could also refer to Fig 4 to keep it simple."

A: A reference to Figure 4 was added as suggested by the referee.

Figure S3: "Does this mean stronger association or weaker association (just clarify)"

A: The higher correlation with the wavelet means a stronger association. This has been clarified in the text.