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Hydrology and Earth System Sciences Discussions

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Interactive comment on "Nonstationary weather and water extremes: a review of methods for their detection, attribution, and management" *by* Louise J. Slater et al.

Anonymous Referee #1

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Whereas most reviews focus on a single aspect of hydrologic non-stationarity (e.g. the type of trend tests applicable) this manuscript I believe is the first manuscript to review the entire non-stationary management process from identifying metrics, data considerations, through to analysis and management.

I have provided a lot of comments, but they are mainly editorial in nature. I understand it is not possible to capture all published literature in a review. Hence where I have suggested references or additional methods to be described the authors should not feel these recommendations are prescriptive.

Maybe one general comment: The manuscript described changes to wind in detail at the start, but it didn't seem that wind was mentioned in Section 5. This may be something the authors wish to consider in a revised manuscript.

Otherwise, thank you for the allowing me to review this manuscript which I am confident will be a very welcome addition to the literature. Please see below for my specific comments.

Page 1, Abstract: As the title mentions “management” I think the abstract should mention management also.

Page 1, Line 18: I think you should define natural nonstationarity. My guess is you mean “large scale climate variability” like ENSO and Milankovitch cycles? I am not sure.

Page 2, Figure 1: I wonder if this figure would work better as more generic: “magnitude” on the y-axis and “time” on the x-axis rather than specific numbers.

Page 2, Line 4: “death of stationarity in water management” – I would add water management as I think that is what Milly et al was specifically referring to and it ties to your title.

Page 3, Line 3: I would remove “somewhat” and just state “are predictable”.

Page 3, Line 4-11: My feeling is this paragraph could be deleted. It is repeating what was said before.

Page 3, Line 13: Where you state your manuscript as an “introductory overview” maybe you could say something stronger like the manuscript is the first to review the entire non-stationary management process from identifying metrics to analysis through to management. I am not sure my wording is the best, but I hope the authors understand the sentiment.

Page 3, Figure 2: The words “issues” under attribution sounded a bit vague in the context of the other headings and sub-headings.

Section 2. I somewhat disagree with the choice of subheadings. I don't think you answer the questions but rather summarise the metrics used to investigate the symptoms (as you state at the start). Hence, I feel the headings would be better as "event magnitude", "frequency", and "timing". But I respect the authors may disagree.

Page 4, Line 22: I think a sentence defining the PMP is needed before the sentence which states "For a complete state-of-the-art review..." Page 4, Line 25: This doesn't quite fit in this section. It would fit however if you stated what metric they used and then it would be an example of the application of the metrics above.

Page 6, Line 10: I didn't understand what "both sides" meant.

Page 7, Line 22: "percentage of time" (remove "the").

Page 7, Section 2.2. It could be worth noting the identification of independent events for pot analysis can be performed by fitting Poisson models https://doi.org/10.1016/0022-1694(82)90136-6

Page 8, Line 28: I don't agree with the AEP definition as an average waiting time as I think a better definition is the probability of occurrence above a threshold in a given year.

Page 9, Figure 4: I am not sure all the panels of the Figure were referred to in Section 2?

Page 10, Line 5: A possible useful reference: https://doi.org/10.1038/s41598-019-52277-4

Page 10, Section 2.3: Some recent references on changed rainfall timing are: https://doi.org/10.1029/2018GL079567; https://doi.org/10.5194/nhess-18-2047-2018. Recent references on changed flood timing are: https://doi.org/10.1029/2019WR026300; https://doi.org/10.1029/2020WR027233.

Page 11, Line 11: I feel the word 'essential' in the title is superlative.

Page 11, Line 15: Could insert "particularly" in front of prevalent (as data issues tend to be worse for extremes).

Page 11, Line 21: "channel" cross-sections? This might help make it clearer that this refers to streamflow specifically (the factors listed before this apply to all weather stations).

Page 11, Line 27: I wasn't quite sure what "system drift" means.

Page 12, Figure 5: I don't know what "recon", "eval", "cal" stand for. I feel this figure could be better explained (at least in the caption).

Page 11, Line 29: Figure 5 presents an "approach" for dealing with homogeneity not "tests".

Page 12, Line 11: I agree with the statement, but I don't actually know what climatologists are doing better than hydrologists – could this be stated explicitly?

Page 13, Line 13: "Climate is non-stationary by definition": or does it depend on the record length as you have stated? That is, if you pick the correct length of record it will be stationary? This statement probably just needs explaining to fit in the context here.

Page 14, Line 20: I am not sure Theil Sen is a "test". Maybe a test statistic? But even then, this is not strictly correct. Maybe remove the following text: "An additional test called the"

Page 14, Line 21: "slope" -> "magnitude"? Page 14, Line 27: I think the statement "in cases where the assumptions of OLS are not met QR may be used" is correct but I want to note that when the assumptions of OLS aren't met (e.g. linearity) many authors argue non-parametric methods (such as MK) should be used. It would be helpful to state what the assumptions are and point out that the Theil-Sen slope estimator is an alternative also?

Page 15, Line 8: Or climate covariate? http://dx.doi.org/10.1016/j.jhydrol.2015.04.041

(the advantage being you can then covariate with GCM projections).

Page 15, Line 29: After 2018 can add a reference to Figure panel 6f and maybe start a new paragraph.

Page 17, Section 4.2: Max-stable models (after Coles, 2001) are another method of pooling data directly (e.g. https://doi.org/10.1016/j.jhydrol.2011.06.014)

Page 18, Section 4.4: I think this section mentions circular statistics used but not the methods used to calculate the trend (e.g. circular regression: https://doi.org/10.1029/2019WR026300; circular-linear associations (Villarini, 2016); Theil-Sen (Bloschl et al 2017); Linear regression before https://doi.org/10.1126/science.1152538 or after standardization https://doi.org/10.1029/2020WR027233.

I think alongside Gu et al., 2017 the following apply https://doi.org/10.1029/2018GL079567; https://doi.org/10.5194/nhess-18-2047-2018.

Page 19, Line 10: Not sure the other sections had this transitionary statement so not sure it is needed here.

Page 19, Line 25: “The effect of these nonstationary drivers, and their interaction, is complex. . .” Could insert “their interaction” to tie this to Section 5.5.

Page 20, Line 5: I recognise most studies approximate this to 7% but 6-7% is probably more in line with background land temperatures.

Page 20, Line 12: Can it just be made clearer that the “longer duration” events statement is specific to the UK example. The way it is phrased it sounds like a global change which may not be true.

Page 22, Line 19: I appreciate the following reference isn’t for extremes but it does provide a mechanism for more greening under climate change: https://doi.org/10.1038/nclimate2831

Page 24, Line 9: The following reference could be relevant to the soil moisture-rainfall feedbacks: https://doi.org/10.1029/2018JD029762

Page 24, Line 29: “Climate modelling framework in Section 4.2”. I might have missed something as I am not aware of a climate modelling framework being introduced in Section 4.2. Maybe some rephrasing would help the reader follow here.

Page 26, Section 6.3: I think I expected a reference to the following which I think was the first study to attribute extreme rainfalls to human induced climate change: https://doi.org/10.1038/nature09763.

Page 29, Line 4: Again, I am not sure the transitionary sentence is required.

Page 29, Section 7: In the context of whether or not a system is vulnerable and requires management this following might be of interest: https://doi.org/10.1007/s10584-019-02497-4

Page 30, Lines 7-19: I did fail to see the relevance of much of this section given that non-stationarity deals with “climate” and hence longer time scales than those in numerical weather prediction. Maybe this section of text could be shortened as I feel it is more a pointer to Section 7.3.

Page 30, Line 30: I would have thought the main limitation is the fact that in hydrology we are interested in projections at the catchment scale, but climate models work on resolutions much greater than this necessitating complex downscaling. I appreciate you pointed to this in the previous section, but I can’t help but feel this is a natural place for mentioning this point and describing it in a few sentences. I think a discussion on downscaling would not go astray here. See also: http://dx.doi.org/10.1016/j.jhydrol.2014.11.003

Page 32, Line 29: I am not sure that the presence of non-stationarity itself is controversial but whether it should be considered?

Page 33, Line 3: Would it make sense to write out the definition of functional non-stationarity again here?

Page 33, Line 31: Maybe “The” final step?

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