Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-576-AC2, 2021

Hydrology and Earth System Sciences Discussions
Open Access

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

Louise J. Slater et al.

louise.slater@ouce.ox.ac.uk

Received and published: 14 February 2021

We would like to thank Reviewer 2 for these positive and helpful comments which will improve the quality of our review. Below we provide the Reviewer's comments verbatim in black font, and our responses immediately below each comment in blue font.

A review paper serves a very specific purpose. A reader seeks to read a well-structured paper providing a well-developed synthesis of the literature in a clear and concise manner. This paper in my opinion succeeds in its role and offers an excellent addition to

Printer-friendly version

Discussion paper

the literature. Since there aren't any major issues with the manuscript, I can only offer some minor points that might be useful to the authors. These are mainly some thoughts emerged as I was reading the manuscript line-by-line.

We are grateful to the Reviewer for this very positive feedback and for the suggestions made below.

1.11 This definition is too simplistic and misses the join properties. Yes, indeed this is a property of a stationary process, but we can easily create a process that has the same distribution over time but a changing autocorrelation. So, I you wish to keep it as simple as possible and avoid the formal definition just add ". . .statistical properties of the distribution and correlation do not. . ."

We will modify the sentence accordingly.

12.14 I am not sure if I understand this. Definition of nonstationarity is mathematical and precise; in simple terms any process that does not fulfill the formal mathematical conditions of stationarity is a nonstationary process. Thus this should not be linked with the data. Identifying nonstationarity or stationarity from data is another issue and doesn't differ than any other data-driven inference. Please clarify.

We agree and will remove the sentence.

12.16 Whether a record is short or long, or sufficiently large for trend detection does not depend only on the absolute record length. So, I am a bit skeptical about such statements. Heavy tailed distributions for example introduce larger uncertainties.

This is a good point. The sentence will be removed.

13.7 Not necessarily, the variance does not affect the significance of a trend and it is incorporated in the test. Or a process might change only in its very high values. Anyway, if you have a reference about this statement please add it as I am not sure if it is absolutely correct.

The original sentence was "Highly variable time series ... require a longer period of time for a significant signal to emerge....". Instead, we will simply state that for a highly variable time series it takes a larger percentage change in the mean of the data to identify a statistically significant change compared with a less variable time series (e.g. Chiew and McMahon, 1993).

13.19 This depends on what you define as trend here and is a bit confusing. If trends refer to a local systematic increase or decrease this is a property of the dynamics of a process or of the external factors causing this change. So it “feels” a bit confusing saying that trend depends on period of record. To clarify, a trend in the record is a trend anyway, but we assess if this is significant (for whatever reason) based on the properties of the process (inferred from the record).
Agreed. This comment will be removed ("trends depend intrinsically on the period of record and indeed on the quality of the dataset").

14.21 Just recheck this. I think the Theil-Sen is not a test, it’s just an estimator based on the median slope of all pairwise point of the record. Sometimes provides better results that the regression slope sometimes not. Still you can use also an intercept estimate based on Theil-Sen and show the fitted trend line.
Agreed; this is an error in the text. We will re-write and clarify this point.

19.22 Large scale variability cannot be a driver of nonstationarity, unless you define nonstationarity in a “local” or short-term way. Long term variability causes local trends, but the process can still be stationary. This is very easy to show with MC simulations.

For example, multidecadal oscillation can could cause multi decadal trends but this does not imply nonstationarity based on the formal definition.

We agree and will re-write this sentence.

20.7 This is an assumption that circulates but it need more investigation as there are contrasting results regarding the light precipitation https://doi.org/10.1029/2019JD030855
The Reviewer is referring to the statement that "Extreme precipitation is expected to become more intense, and weaker rainfall less intense". We will clarify that this assumption needs further investigation, with reference to Markonis et al. (2019).

20.10 Also you can add information of the literature on convective non convective events and changes e.g.
https://doi.org/10.1175/JCLI-D-17-0075.1, https://doi.org/10.1038/ngeo1731
Thank you. We will discuss these, including references to Park and Min (2017) and Berg et al. (2013).

27.12 KS has a lot of theoretical issues and the AD should be favor. If I'm correct there's a tendency to slowly stop using the KS test.
Yes; we will mention this. The AD test is more powerful when comparing two distributions than the KS test (Engmann and Cousineau, 2011).

If the authors wish they can add more information on downscaling of climate model since it relates with nonstationarity, see e.g., https://doi.org/10.1029/2009RG000314
We agree and will include more information on downscaling, including a reference to Maraun et al. (2010).

Summarizing, I am happy I do not have much to report. This is well-written review paper on the topic. It was joy to read and it offers an excellent addition to the literature.
We are very grateful to the Reviewer for these kind comments and for their supportive review!

References

Berg, P., Moseley, C., and Haerter, J. O.: Strong increase in convective precipitation in response to higher temperatures, Nature Geoscience, 6, 181–185, 2013

Chiew, F. and McMahon, T.: Detection of trend or change in annual flow of Australian rivers, International Journal of Climatology, 13,643–653, 1993.

Engmann, S. and Cousineau, D.: Comparing distributions: the two-sample Anderson-Darling test as an alternative to the Kolmogorov-Smirnoff test, Journal of applied quantitative methods, 6, 1–17, 2011.

Maraun, D., Wetterhall, F., Ireson, A., Chandler, R., Kendon, E., Widmann, M., Brienen, S., Rust, H., Sauter, T., Themeßl, M., et al.: Precipitation downscaling under climate change: Recent developments to bridge the gap between dynamical models and the end user,Reviews of geophysics, 48, 2010.

Markonis, Y., Papalexiou, S., Martinkova, M., and Hanel, M.: Assessment of water cycle intensification over land using a multisource global gridded precipitation dataset, Journal of Geophysical Research: Atmospheres, 124, 11 175–11 187, 2019.

Park, I.-H. and Min, S.-K.: Role of convective precipitation in the relationship between subdaily extreme precipitation and temperature,Journal of Climate, 30, 9527–9537, 2017

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-576, 2020.