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Interactive comment on "Detection and attribution of flood trends in mediterranean bassins" *by* Y. Tramblay et al.

Anonymous Referee #1

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As the authors rightly point out (lines 97-103) there is very little understanding on how changes in antecedent moisture conditions are modulating flooding under the assumption of non-stationarity. The authors not only demonstrate the modulating effect of changing antecedence on the 95th and 99th percentile of stream flow but also present results demonstrating the effect of catchment non-stationarity (e.g. urbanization). I enjoyed reading this manuscript. I find the manuscript to be novel and I recommend publication. A few minor comments are below.

General comments:

By chance of the three references I checked in the text two were missing from the bibliography e.g. Prein et al., 2016 and Bloschl et al., 2016. Please do check the referencing.

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Discussion paper

Line 210-217: Does quantile regression use all the data? If so, how is the 99th percentile of this comparable to the 99th percentile of the POT analysis where you only end up with a handful events per year?

Section 4.1: I get the impression that Figure 2 might not have used the Mann-Kendall test or quantile regression described in the methods but possibly a different technique? I am not sure. But this can be easily clarified but inserting a sentence at the start of this section.

Line 330: You say "was not regionally significant" does that mean that all the figures have significance only tested on a site by site basis even though you said in the methods you use a FDR? This needs to be clarified.

There a couple small grammatical things like line 48 "These results imply . . ." and Line 166: "As a very common . . ." but these are an easy fix that can be addressed at the editorial stag

Line by line:

Line 70: A global study might make the point of high spatial variability better e.g. http://dx.doi.org/10.1029/2011GL048426; https://doi.org/10.1002/2016GL071354 But this is at the authors discretion as it may be that they were referring to variability on a smaller spatial scale (not sure because the reference was missing).

Line 106, 138, 482 I probably prefer "e.g" rather than ". . ." at the end of the examples. But again at authors discretion.

Line 120: Number of rain days or mean rainfall or both?

Line 217: Remove "but preliminary tests" and just write "and". This will sound more robust.

Line 274: Can you add the "with precipitation below 1mm" to the figure legend also please.

Line 325: I didn't think the trends were low? Actually they seemed quite large given the number of events per year?

Line 400: Is the 30 and 365 day averages also lagged or is it just the coincident month/year that is averaged?

Line 401: remove "rather"

Line 403: opposite -> "other"

Line 421: "mean altitude" – this typo made me chuckle!

Line 427: This section needs rewording I think. You say "R values up to 0.6" for small basins but to counter this you say values "about 0.1 to 0.2" for large basins. One statistic is a maximum and the other is more related to the mean. It may be larger basin values also have R values up to 0.6 but I wouldn't know? Picking a more consistent statistic would give me more confidence in these results.

Figures: Can it be clarified in the manuscript text and on (every) figure legend that only statistically significant sites are shown (and at what level)?

Figure 2: At least on this figure (but preferably on all the figures) the axes should be labelled "lat/lon" on at least one panel.

Figure 2 caption: Add "rainfall" to the list of variables.

Figure 3: Scale for triangles?

Figure 5: Are all sites presented here or just statistically significant ones?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-307, 2019.