Natural Hazards
and Earth System
Sciences

Discussions

Interactive comment on "Trends and variability in extreme precipitation indices over Maghreb countries" *by* Y. Tramblay et al.

M. G. Donat

m.donat@unsw.edu.au

The paper is mostly well-written, the analysis seems sound and the results are clearly presented.

However, I feel that some points still need clarification (see list below) and I also recommend to take recent literature into account (especially the repeated statement that this would be the first study of extreme precipitation for this region seems not valid).

- page 3626, line 5: provides the first regional assessment of trends on its southermost shores – stations in this region have also been previously investigated by Donat et al. 2013 (see http://onlinelibrary.wiley.com/doi/10.1002/joc.3707/abstract)

- page 3627, line 21: no such regional trend analysis exists for North Africa – see previous comment

- p 3628, l 14-30: it might also be worthwile to extend this review with the previous findings for the Arab region

- p 3630, l 8: form → from ?

- p 3630, l 10: went under → underwent?

- p 3630, l 11: not clear what unfeasible precipitation values means – unphysical?

- p 3630, l 16: most of the precipitation during late fall and winter – there is a verb missing in this sentence

- p 3630, l 23: rainiest and most populated area – should specify the context here; I guess this relates to the African continent and is no global statement?

- p 3633, Section 3.2: It should be clarified here to which kind of data (accumulations?) the homogeneity test was applied. Monthly/annual totals? Or to the indices defined in 3.1?

- p 3636, l 14-24: It is not clear to me how the variogram analysis relates to the research question of this study. Maybe this can be clarified.

- p 3637, l 6: "can represents" - please check language/correct

- p 3637, l 17: I wonder about the consistency of approaches here. On the one hand, Mann-Kendall/Sen slope estimates are used for trend analysis (making no assumption about the distribution of the variables), but on the other hand here least-square fit is used for detrending. This might not be the most appropriate approach for some of the extreme indices?

- p 3637, l 27: FQR → FDR ?

- p 3638, l 3: number of dry days (R1mm) – seems to clash with the definition of R1mm

in Section 3.1 (ratio of wet days)

- p 3638, l 17: R95pTOT not defined in Section 3.1 ?

- p 3638, l 5-10: Discussion of trends: The results by Donat et al. (2013) suggested that there was a shift from relatively wet conditions in the 1960s to drier conditions in the 1970s/1980s, with slight increases after 1980. This is also found in (partly independent) gridded observational datasets (see e.g. HadEX2 available on www.climdex.org, see also attached figure for this region). In this context, I am wondering if the long-term decreasing trend reported here for PRCPTOT is mainly caused by such a shift?

- p 3638, l 25: this is the case of the NAOi and MOi – clarification is needed here if also the precipitation indices (e.g. PRCPTOT) were detrended for this analysis

- p 3639, l 5-13: I would appreciate some physical interpretation of these relationships, which are presented as purely statistical relations here.

- p 3640, l 1: first assessment of trends... - please see earlier comment and rewrite taking previous literature into account

- Figure 2: an explanation of the green-yellow-red colours would be useful. Maybe a colourbar might help to indicate data ranges.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 3625, 2013.
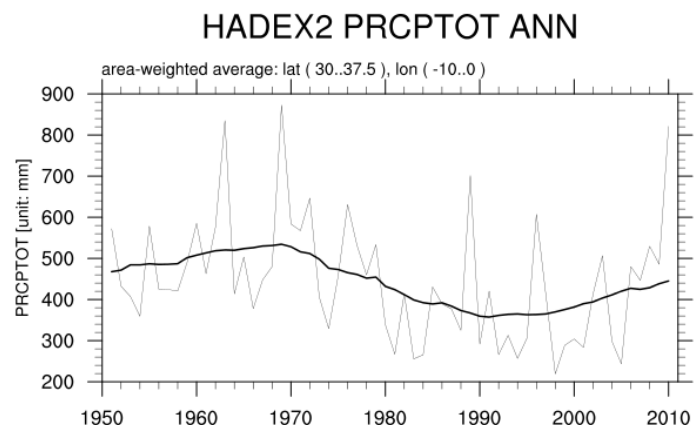
Fig. 1. PRCPTOT timeseries Maghreb from HadEX2