

Interactive comment on “Non–stationarity in annual maxima rainfall across Australia – implications for Intensity–Frequency–Duration (IFD) relationships” by D. C. Verdon-Kidd and A. S. Kiem

Anonymous Referee #1

Received and published: 20 April 2015

General Comments: The paper investigates non-stationarity in short duration extreme rainfall events over Australia. Although the topic is of general interest to the hydrologic community, the paper suffers from serious limitations (listed in detail below) which collectively fail to make this paper rather weak. In the present form, the paper simply presents simplistic statistical tests and investigates statistical associations between the extreme rainfall and the IPO (much similar to the widespread composite analysis) and fails to provide insights on the nature of the processes involved. I hope that the authors will expand their study and take into account the suggestions mentioned below

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to improve their study.

Specific Comments:

Recent studies (Montanari and Koutsoyiannis, WRR, 2014; and references therein, Koutsoyiannis, JH, 2006) show that modeling approaches which consider non-stationarity of real world time series without examining the properties of the stochastic processes, may be inappropriate. The way the authors test and claim for non-stationarity in the extremes is inadequate and quite limited. In fact, it has been shown in some studies (Serinaldi and Kilsby, AWR, 2015) that non-stationary models may increase the uncertainties and that traditional concepts should still be retained as benchmarks. Thus, the authors' skepticism about the BoM and ARR's existing approaches may not be justified.

Also, the definition of return period itself (and equivalently that of ARI) may change in the non-stationary setting (Salas and Obeysekera, ASCE JHE, 2013). Moreover, although the title mentions "– implications for Intensity–Frequency–Duration (IFD) relationships", this paper only presents a discussion (Section 3.2) which contains rather generic discussion on how non-stationarity may affect such relationships, without carrying out any analysis on how the observed-period IFD relationships actually change because of non-stationarity (such as that done by Cheng and AghaKouchak, Sc. Reports, 2014), bringing into question the novelty, utility and scientific contribution of this study.

The authors mention, in their conclusions, "The research presented here demonstrates that information currently available on natural variability..can act as a guide to the baseline..." - this is a fat-fetched conclusion. The present research, however, doesn't provide any guidelines on how this baseline can be defined.

Is IPO the same as PDO (Pacific Decadal Oscillations)? If it was known apriori that locations such as Melbourne are not affected by the IPO, why was it chosen for the analysis? Perhaps a more appropriate approach would consider several natural vari-

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ability modes, as well as forced changes and investigate their individual effects on rainfall extremes.

GEV distribution is usually deemed appropriate for annual maxima. How does the GEV distribution fit the data at hand? How are spatial dependence between extremes taken into account? Why not consider peak-over-threshold approach?

Claims such as "we emphasize that there undoubtedly is non-stationarity in historical short duration rainfall extremes" might be inappropriate for reasons stated above.

Literature review pertains mostly to studies on Australian datasets, whereas much work on similar ideas are also carried out elsewhere.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 3449, 2015.