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Discussions

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

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We would like to thank the anonymous reviewer for their review of the paper "Non-stationarity in annual maxima rainfall across Australia-implications for Intensity-Frequency-Duration (IFD) relationships". We have considered the Reviewers' comments and provided detailed descriptions of how each comment will be addressed in the revised manuscript below:

Specific comments: 1. Recent studies (Montanari and Koutsoyiannis, WRR, 2014; and

references therein, Koutosoyiannis, 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 bench- marks. Thus, the authors' skepticism about the BoM and ARR's existing approaches may not be justified.

Based on this review and the review of our paper provided by Dr Serinaldi we will improve the paper as follows: a. Include a test for serial correlation using the Durbin-Watson (DW) statistic. The Durbin-Watson statistic tests the null hypothesis that the residuals from an ordinary least-squares regression are not autocorrelated against the alternative that the residuals follow an AR1 process. All DW statistic values were found to be greater than the 1.562 (the upper bound for 1% significance) providing no evidence to reject the null hypothesis (see figure below showing the distribution of all DW statistic values for the 1-day annual maxima timeseries at each site). b. Address the issue of potential spatial correlation among rainfall sites. We found that less than 9% of all possible pairings of rainfall data sets display a significant (yet weak) correlation at the 5% level (r >0.2, significance based on n=100). Only 8 pairings (out of 4465) were correlated at 0.5 or higher. It was also found that stations located more than 500km apart were unlikely to be correlated and that the strength of the correlation reduced as distance increased between the pairs. This is not surprising given annual maximum rainfall events are due to synoptic scale processes. c. Revise the text in the paper with respect to the use of the term non-stationarity. In particular, we will use the term non-stationarity only when referring to the IFD development (which is deemed appropriate given the IFD is essentially a model), however the sections of the paper that are focused on identifying change points in the rainfall timeseries will be edited and the term "regime shift" will be used in preference. Further, we will also change the title of the paper to "Regime-shifts in annual maxima rainfall across Australia – implications for Intensity–Frequency–Duration (IFD) relationships". The associated text will include the references provided by the reviewer

2. Also, the definition of return period itself (and equivalently that of ARI) may change in the non-stationary setting (Salas and Obeysekara, 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.

We agree with the reviewer that the ARI may change in the non-stationary setting. In fact, that is the point we are making in our paper; that depending on when the data is sampled from to generate the IFD, it may be biased to either a wet or a dry phase (or surplus or absence of high intensity events) and therefore would have consequences on the resulting return period for individual rainfall depths. Indeed we suggest in our discussion "that a separate set of IFDs could be developed for use in high risk modelling for engineers who need to account for the 'worst case' (in a similar manner to climate change allowances). This second set of IFDs could be developed based on the periods of elevated annual maxima alone (for those stations with clearly defined epochs of annual maxima) such that if we were to enter such an epoch, designs based on these estimates would be robust for the duration of such a period." We disagree with the reviewer that we do not show how nonstationarity (which we will now term regime shifts) may affect the IFD relationships. This is demonstrated in Section 3.2 "Effect of non-stationarity on IFD estimation" where we recalculate the IFD curves for the difference phases of the IPO. Importantly we show that the return period is different for the various rainfall depths and durations depending on the underlying rainfall dataset (i.e. depending on whether it is sampled from the IPO positive or IPO negative

distributions).

3. 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 base- line..." - this is a fat-fetched conclusion. The present research, however, doesn't provide any guidelines on how this baseline can be defined.

This will be clarified in the revised paper. Our intention here is to emphasize that, for regions where large-scale climate drivers operate on a multi-year to multi-decadal timescales and are known to influence extreme rainfall events, we can use this information to determine if the climate statistics on which the IFD are based are likely to be biased or missing crucial information.

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

The IPO is not the same as the PDO. The IPO is a Pacific Basin wide phenomena rather than just the north Pacific that is represented by the PDO. There are similarities between the two timeseries however and they are significantly correlated. According to Salinger et al 2001 "The IPO may be a Pacific-wide manifestation of the PDO, excluding subdecadal time scales, and seems to be part of a continuous spectrum of low frequency modulation of ENSO, and so may be partly stochastic". It is true that some existing studies suggest that the IPO signal on rainfall tends to be weaker in Melbourne due to competing influences from the Southern Ocean, however we cannot say that Melbourne rainfall/climate is "not affected by the IPO". Some studies suggest IPO significantly effects rainfall characteristic in Melbourne (e.g. Verdon et al 2004, Gallant et al 2012) while others do not, therefore in our study we do not make any assumptions about IPO effects on rainfall maxima in Melbourne and include it in our investigation.

Our results suggest there is a relationship where “all events (other than 72 hours) with a 2-year ARI are associated with a higher rainfall intensity estimate in IPO positive for Melbourne, however the reverse is true for the less frequent events.” While we agree with the reviewer that there are several modes of natural climate variability that may have an effect the extreme rainfall from year to year, in our study we were specifically interested in climate drivers that are likely to force a regime shift in extreme rainfall (similar to that observed for flood risk). Therefore we were interested in drivers that operate on a decadal to multi-decadal timescale (as is the case for the IPO). Other drivers (such as ENSO, Indian Ocean Dipole, Southern Annular Mode) tend to influence rainfall in Australia on much shorter timescales. However, if this method was to be applied to regions other than east coast Australia (i.e. where IPO is known not to be the primary driver on decadal to multi-decadal timescales), other potential sources of decadal to multi-decadal variability would need to be identified.

5. 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?

The Reviewer’s point is correct, the GEV distribution does indeed fit the annual maxima data well. In fact the updated IFD (which are NOT currently used in operation) are based on a revised statistical methods that includes fitting the GEV distribution to the data in preference to the Log Pearson III. However, the methodology adopted in this paper (including fitting the Log Pearson III) to calculate return periods of annual maxima deliberately follows that outlined in Australian Rainfall and Runoff (1987), Engineers Australia’s guide to estimating and utilising IFD information. The purpose of adopting the AR&R 1987 method was to assess the implications of varying data lengths and climatic variability on the resulting IFD (which have been historically used and are currently still in use) and to highlight the issue of underlying variability in the annual maxima that should be appropriately considered and addressed in current and future revisions of the IFD estimates. Further, as an additional check the KS goodness

of fit test was applied to test if the Log Pearson III was a reasonable fit to the data. Here the null hypothesis is that the data fits the Log-Pearson lll distribution (the alternate is that the data does not follow the Log Pearson III distribution). All p-values were greater than 0.05 (average p-value was 0.75), for all series (30min to 72hr durations at Brisbane, Sydney and Melbourne), therefore we accept the null hypothesis at the 5% significance level.

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

We agree and this will be revised and the discussion extended. The following text will replace the sentence above: “This study has highlighted the existence of regime shifts in annual maxima rainfall data in Australia. The driving mechanisms of these regime shifts are likely to vary from location to location and decade to decade. However, these shifts are typical of many natural phenomena and can be described by processes characterized by long range dependence (or regime-switching processes) and captured by hidden Markov models (or similar), resulting in a mixture of distributions that alternate stochastically according to the transition probability from one regime to the next (e.g. Serinaldi and Kilsby, 2015a). While the strategy of defining IFDs for two (or more) different regimes (e.g Serinaldi andKilsby (2015a)) currently only partially solves the problem, as we often do not know the beginning or the end of a specific regime (be it rainfall or climate driver), recent work has focused on optimizing designs and planning strategies based on the range of what is plausible rather than a reliance on knowing the current and future climate state (e.g. Mortazavi-Naeini et al., 2015). At the same time, work is also underway on seamless prediction at a range of timescales and if/when this eventuates the results discussed here become even more important/useful. Nevertheless, the immediate usefulness of the insights presented here occurs when first establishing the IFD, as an approach similar to that employed here can be used to determine if the underlying data are biased to a mostly wet or mostly dry regime (or a mix of both) which then provides an indication as to whether the IFD is likely to be an overor underestimate of the true risk. Importantly, this issue needs to be considered and accounted for when attempting to estimate IFD design rainfalls and prior to quantifying how those IFD estimates might change in both the near and long-term future."

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

The literature review will be extended to include the references mentioned by the Reviewer as well as the following international papers:

Cooley D. (2013) Return periods and return levels under climate change. In: AghaKouchak A, Easterling D, Hsu K, Schubert S, Sorooshian S, editors. Extremes in a changing climate. Water science and technology library, vol. 65. Netherlands: Springer. p. 97–114.

Douglas EM, Vogel RM, Kroll CN. (2000) Trends in floods and low flows in the United States: impact of spatial correlation. Journal of Hydrology 240: 90–105.

Guerreiro S.B., Kilsby C.G., Serinaldi F. (2014) Analysis of time variation of rainfall in transnational basins in Iberia: abrupt changes or trends? Int J Climatol 34(1):114–133

Koutsoyiannis D. (2003) Climate change, the Hurst phenomenon, and hydrological statistics. Hydrol Sci J 48(1):3–24

Koutsoyiannis D., Montanari A. (2014) Negligent killing of scientific concepts: the stationarity case Hydrol Sci J http://dx.doi.org/10.1080/02626667.2014.959959

Salas J.D., Obeysekera J. (2014) Revisiting the concepts of return period and risk for nonstationary hydrologic extreme events. J Hydrol Eng, 19(3):554–68.

Serinaldi F., Kilsby C.G. (2015a),Stationarity is undead: Uncertainty dominates the distribution of extremes. Advances in Water Resources, 77, 17-36

Serinaldi F., Kilsby C.G. (2015b) The importance of prewhitening in change point analysis under persistence. Stochastic Environmental Research and Risk Assessment

Yue S, Pilon P, Phinney B. (2003) Canadian streamflow trend detection: impacts of serial and cross-correlation. Hydrological Sciences Journal 48(1): 51–64.

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