

Interactive comment on “Impacts of a changing climate on a century of extreme flood regime of northwest Australia” by A. Rouillard et al.

Anonymous Referee #2

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The authors should be commended on developing a record of lake / marsh extent using remote sensing data, especially from an arid region (very underrepresented in the literature) and increasingly under climate and human pressures. This kind of data is therefore extremely valuable for science and management. Unfortunately, I do not support a large part of the analyses and some of the interpretations. There is also a poor (and inconsistent) use of terminology throughout the paper. For example, flood regime is in the title and within the paper, yet no clear analysis of catchment flooding is provided (e.g. magnitude and frequency structure), probably since the data is not available. This is not simply semantic, we have to reserve ‘extremes’ for when we have some understanding of the distribution of catchment hydrological events. Moreover, no catchment hydrology information is provided (or available?), only the lake / marsh

extent, which is obviously related to, but definitely not the same thing as catchment flooding. Of course it is reasonable to suppose that a quick rise in lake extent must be due to a large catchment runoff event, but this response is likely to be highly non-linear (especially concerning the role of antecedent conditions) therefore it is not possible to link lake extent alone to a formal analysis of flooding without more information. A smaller point relates to the attribution of a changing climate on the hydrology, the variability is so large I'm not sure it would be possible to extract a statistically meaningful trend from this data, and nor have the authors attempted it, so it is unclear why the authors do not instead try to assess the role of extreme climate variability rather than climate change.

My main concern is with the data analysis and the lack of a water balance to at least provide some realism for the extrapolated time series. The construction of the linear regression model is unclear, but it seems the final model has four variables, all of which would seem to be highly correlated with each other (monthly rainfall and number of rainfall days for example), but most importantly given the extreme variability, I have no doubt that the correlation structure between all these variables should shift over time. Given this noise, the parameters derived would have very little robustness, and thus any extrapolation (over 4 times the observation length in this case!) would have substantial errors (though the authors have made no attempt to quantify this), and I suspect therefore little value for prediction. This is of course one reason why multiple linear regression models are rarely used in trying to conceptualise highly non-linear catchment hydrological processes. That being said, lake extent could be tackled using a simple water balance approach very effectively, and one that is much more robust to the variable hydrology, and the authors clearly have much of the requisite data to achieve this. A more minor concern relates to the vague catchment description, the authors mention there is an upper and lower catchment (with the Marsh in the upper part), and that the Marsh may overflow, but then end it there! Surely the dynamics of the overflow are fundamental to the marsh hydrology, so why isn't this analysed in further detail or taken into account in any of the further analyses? How can we have

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confidence in the large time series extrapolation if we don't know anything about how the outflow dynamics of the system operate? This would not be particularly onerous to achieve, but its omission detracts from confidence in the results and interpretation.

For these reasons I do not recommend the manuscript be published in its current form, and given the scale of the required changes, a new paper would basically need to be written.

Some more specific comments are provided below:

Introduction

“Changes in hydroclimatic patterns and extremes that might alter the natural disturbance regime. . .” what would be a ‘natural disturbance regime’ in one of the most variable climates . . . on earth, and how would we know if it is altered?

“the interannual variability of rainfall is high” is used repetitively in the introduction, once is enough.

Study site

“as the largest freshwater feature” how fresh is this if it is dominated by ‘salt tolerant species’? Perhaps there is a short lived pulse of freshwater during initial inundation, but how quickly does this deteriorate?

“The marsh acts and an internally draining basin” but just below you say it can overflow into the lower Fortescue catchment, so how is it an internal basin? The authors say the lake can overflow at 410 m asl, but we have no way to compare this to the data in the rest of the paper since no depth scale is ever used, so the corresponding marsh extent would be very useful. This feeds into the larger issue regarding the overflow dynamics mentioned above in the general comments.

“the residence time of water in the upper sections of the catchment is short” how do you know, and what do you mean by short? Do you have age data, or tracer studies

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to determine the transit time distribution? If so then it would seem very important to include.

“does not retain water significantly diluted nor flushed by groundwater” if the groundwater is salty then I guess the pool is not really diluted by this inflow

Mapping flood history

“a groundtruthing expedition . . .” and did it match the remote sensing?

Model development and selection

The use of the dFa metric means a water balance would not at all be difficult, see general comments

Results

“Because it was not possible to calculate dFa. . .” why not, shouldn't it just be 0?

Lines 8-15: this is completely beyond the explanatory capacity of the ‘model’ and is crazy that the authors try to explain their model extrapolation in this way, please delete. There is no data or water balance to verify any of this

Spatial and temporal patterns of inundations

To me this is the most interesting part of the paper (and could be done with the remote sensing data alone), however the timing, duration, and magnitude dynamics of the marsh extent change are barely touched on (mostly aggregated statistics of the dataset). Teasing out the dynamics of these changes with different event magnitudes and possible thresholds would be a very interesting addition to this work.

Line 23: this makes me ask about any possible impacts of mining (given there is so much in the area) on the marsh hydrology. Has there been persistent mine dewatering, and if so, has some found it's way into the system which, even in a small way, might be a causal factor here?

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Lines 26-28, shouldn't the marsh overflow also be critical to consider here?

3.3 significance of predictability and persistence of drought

Lines 4-5: this is an arid climate, how would you expect a different result?

The use of drought in this section is also problematic, since it seems the authors simply mean low marsh water extent. Moreover, I'm not sure how conceptually useful it is to describe arid areas as being in drought or not, since they fundamentally lack surface water for most of the time, otherwise they would not be arid. If this is the norm, then a more useful exercise is to analyse the frequency and dynamics of wet punctuations in an otherwise dry (or drought ridden) landscape.

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