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Hydrology and Earth System Sciences Discussions

Interactive comment on "Defining the climatic signal in stream salinity trends using the Interdecadal Pacific Oscillation and its rate of change" *by* V. H. McNeil and M. E. Cox

Anonymous Referee #4

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Standard and scope

The manuscript describes an attempt to relate electrical conductivity measurements, a measure of salt concentration, to an index of the IPO.

As a reviewer, I was hamstrung because the manuscript did not include a list of references. I was particularly keen to see McNeill and Cox (2002).

The index of IPO plotted in Figure 1 appears to be heavily smoothed. The claim of a trend in these data for the last 50 years seems very tenuous. I'd like to see more stringent analysis of the Burdekin flow record and the relationship with IPO. How does the

Burdekin flow relate to ENSO signals? Does the IPO account for the nonstationarity of ENSO/streamflow relationships identified in Australia that is mentioned in the literature review?

I found the analysis and results unconvincing. In particular there was no conceptual model of how the IPO affects the hydrometeorology of Queensland. (In contrast, for example, studies of ENSO effects use the ideas of anomalous wind directions and hence rainfall anomalies when La Niña or El Niño conditions occur.) The combination of equations 1 & 2 to get 3 could be better done by fitting 3 directly to the data. Also, I'd like to see which of the coefficients were statistically significant.

I'd suggest that for the EC data, the time series approach be abandoned and replaced with simply a presentation of the EC results grouped into (say) 5 or 10 year periods with a comparison with regional rainfalls or streamflows. There is, as I understand it, a reasonably clear picture of relationships between ENSO status and regional rainfalls and streamflows in Queensland.

Presentation

I found the paper difficult to read because the figure numbers the text did not correspond with the Figures. For example "Fig. 3" (line 2, page 8) clearly refers to Figure 4.

As noted above, the list of references was omitted.

The term "dry fallout" (p3) was new to me & could usefully be explained.

In Figure 7, there appeared to be little basis for a quadratic regression rather than a simple linear regression.

Page 10, 2nd last line, refers to Figure 11, which was absent from the manuscript.

I felt that the Conclusions were too tentative for a published paper.

Recommendation

HESSD

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The authors may have a useful approach to demonstrating climate effects on electrical conductivity of fresh water, but firmer results and more rigorous analyses are needed to enable clearer conclusions to be presented. In its present form, my recommendation is that the manuscript is not suitable for publication.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 2963, 2006.