

***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 #1

Received and published: 21 September 2006

Recommendation: I consider the manuscript to be suitable for publication in HESS subject to a more careful discussion of one of the final sections.

Overview: The manuscript represents a good contribution to a better understanding of the impact of climate variability on groundwater dynamics and surface water quality as well as human impact on hydrological systems. The paper first establishes qualitative trends in runoff dynamics that are related to the Interdecadal Pacific Oscillation (IPO) and changes in the IPO (Δ IPO). Based upon physical reasoning, changes in groundwater levels are related to changes in runoff and electrical conductivity in runoff. The aim of the paper is to establish the IPO as an indicator of groundwater levels and

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electrical conductivity in runoff at regional scales. The statistical analysis appears to be appropriate to establish the IPO as such an indicator. Additionally the authors then go on to assess whether the cause of observed trends in EC at a particular site are related to land use or to climate variability. The presentation of material up until this point is generally well structured. The introduction nicely explains a conceptual picture of EC's relationship to base flow and the nature of the IPO and its impact on runoff. This leads into the use of just two variables the IPO and delta IPO as predictive tools for statistical analysis. The last section investigating trends in EC is a little short on detail and appears to be a rushed finish to an otherwise well structured paper. This section should be changed to provide a more careful presentation of concepts, and results. There are a number of points in the "Technical Considerations" below pointing to this section for further guidance. Finally I would have liked to have seen some discussion of this papers relevance to the topic of the special issue. Thresholds and pattern dynamics are likely aspects of the dynamics, for example as discussed the relationship between EC observed in runoff and groundwater levels is the most relevant example, however this is not brought out in the text. This might be a consideration for the editors

Technical Considerations

References

There are numerous errors with the references. Without discussing every one, the following issues stand out:

- "et al." should be in italics when used;
- spaces after et al. and before the comma then year often occur i.e. Chiew et al. , 19xx;
- bracketing errors occur regularly as part of citations with extra or missing brackets; (d) missing "et al." from a number of references in the text;

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- in the references section journal names should be, where ever possible, in abbreviated form consistent with HESS guidelines;
- occasionally “Author et al. (Same Author et al. year)” occur
- missing “.” Sometimes from et al.

Figures.

- The small text in Fig. 1 could be removed it is not clear.
- The small legends in Fig. 3 cannot be seen at the current resolution. They need to be made larger to improve visibility.
- Title of figure 4 (possibly also figures 5 to 9 and 11) could be removed. The axes and the figure caption should contain sufficient information. Please check if the term “representative” has been defined in the text prior to a reference to this figure. Also the regression equation would be as readable in the figure caption. Also recommend removing R squared and regression equations from all figures putting instead in caption.
- It is unclear from the text of the figure caption what “Ho” means in Fig. 11. The figures should as much as possible be stand alone products.

Equations.

- Equation numbering out of alignment.
- Equation 2 mixed upright italic fonts in variable name

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- With reference to Equation 7 the B parameter must have units $s^{1/m} m^{-3/m}$ if Q has units m^3s^{-1} and is not simply a “constant”.

Text.

- Introduction paragraph 1 would benefit from references;
- Introduction 2nd paragraph third sentence needs a full stop after (QDPI, 1994);
- Have described ENSO and IPO already linked to stream salinity in the authors previous work. Would be helpful to elucidate some of these results here to help the reader identify the innovation of this paper.
- Intro 3rd para. Last sentence seems out of place. Link 1st part of para. to next para.
- After these paragraphs would be a good place to state an hypothesis.
- Sect. 2 last paragraph “Similarities have been noted between the ENSO/SOI and the IPO, although they were derived independently (Folland et al. , 2002a; Folland et al. , 2002b).” Please rewrite to make clearer what you mean.;
- The next sentence requires a full stop.
- Sect 2.1 1st paragraph. Emphasis placed on ENSO and IPO but then in next paragraph talk only of IPO. May be helpful to guide the reader more carefully here.
- On page 8 the sentence with “time series 19” makes no sense. I don’t know what this means. Also the argument “was less straight forward than the WL. For a start the delta IPO appears to vary stochastically about its median” does not seem justified given the apparent successful use of the delta IPO to predict water levels. I would reconsider this line of argument;

- Page 6 at the bottom, with respect to the discussion of the use of arithmetic and geometric means, is not clear. A short explanation and a reference here to a standard statistics book might help.
- Page 5 the text “As the rate of change slows, flows diminish, . . .” would benefit from a reference to either a figure or another article.
- Sect. 3.1 last sentence. Avoid the use of “about” make the description more accurate.
- As above for the last sentence.
- Sect. 3.2 3rd sentence requires a full stop.
- Sect 4.2 2nd paragraph I would refer to the “absolute value of the IPO”. The text “the reversed sign values fit sufficiently well with the positive values” is rather convoluted
- Sect 5. 1st para. Last sentence. I am unclear as to what you mean by the “representative EC value”. See the discussion below about this section.
- Conclusions 1st sentence. Do you mean “based on the IPO and its rate of change” as discussed throughout most of the text.

Section on Trends in EC.

The section evaluating trends in EC at a site requires a better explanation. It introduces new concepts in a rushed manner. It is difficult to understand and follow the line of reasoning. For example I could not understand why there was a need to transform the EC data to a flow weighted value, or the justification for the Thorburn *et al.* (1992) method adopted. How would another simpler flow weighting influence results? Also there is no discussion of the B and m parameters obtained or the optimization method

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adopted to fit the data or the goodness of fit, the statistical tests used to test the fit or even a pictorial presentation of the before and after transformed data that we may evaluate the Thorburn *et al.* (1992) method. This section would be improved by a more careful discussion of the approach taken here. Also see point 3b above. Given that the authors hinge the paper on the use of IPO as tool to evaluate trends in stream salinity much more time and space can be devoted to the development of this argument.

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