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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 #2

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General Comments

This study discusses the development of a climate indicator for ground water table levels and stream salinity based on the rate of change of the Interdecadal Pacific Oscillation (IPO). Furthermore the paper aims to demonstrate that this climate indicator may be used to remove the climatic influences on stream salinity trends in order to assess the impacts of land use changes. The development of a process via which climatic influences may be separated from land use impacts (on water table heights and stream salinity) would be a very useful tool for water resource management purposes. The work presented in this paper appears to be novel, representing a positive step

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Interactive Discussion

Discussion Paper

towards integrating climate variability insight into other areas of impact assessment (such as land use change). A number of suggestions have been provided below that may help improve or extend certain aspects of the study.

Specific Comments

In Section 2, paragraph 2, the authors refer to the ‘El Niño/Southern Oscillation Index (ENSO/SOI)’ as a ‘phenomenon’, however the SOI is merely an index of sea surface pressure variability and hence does not actually encompass the entire climate phenomenon that is ENSO. It is therefore suggested that the authors drop the ‘SOI’ component when describing the phenomenon. The authors refer to the SOI as the most common index to represent ENSO, however do not cite any other ENSO indices, such as the widely used SST indices (eg. Nino3, Nino4, Nino3.4). A number of studies have established a relationship between SST indices and climate variability for many parts of the world. In fact Verdon et al (2004) studied the impact of the Nino3 on rainfall and streamflow in eastern Australia. The manuscript also incorrectly states that Kiem et al. (2002) and Kiem and Franks (2004) used the SOI to analyse climate effects on hydrology, whereas these studies employed the Nino3 as an ENSO indictor.

In Section 2, paragraph 3 the authors discuss multi-decadal modes of climate variability occurring over the Pacific Ocean, in particular the Inter-decadal Pacific Oscillation (IPO). However the authors do not mention the Pacific Decadal Oscillation (PDO). The PDO is derived from an EOF analysis of monthly SST data over the extra-tropical North Pacific (Zhang et al. 1997), while the IPO is based on an EOF analysis of near-global data sets (Folland et al. 1998). In fact, research by Folland et al (2002) has demonstrated that the IPO can be regarded as the Pacific wide manifestation of the PDO. International readers may not be familiar with the IPO since it has mainly been described in an Australian context in previous research papers, therefore it is suggested that the authors include a small discussion of the PDO and its relationship to the IPO.

In section 2.1, paragraph 2 the authors make note of a 50 year rising trend in the IPO

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and this is highlighted in Figure 1. However, any trend line that is drawn over two cycles of the IPO initiating during a negative phase (i.e. cool epoch) and concluding with a positive phase (i.e. warm epoch) will result in an apparent rising trend. In fact if the same trend line were drawn for the 1925-1975 fifty year period the result would be a falling trend (since the IPO is trending from positive to negative during this time). It is suggested that this trend line be removed as it is not relevant.

In Section 3.2, paragraph 3 the authors explain that the climatic indictor used in the study is based on the difference between the January value of the smooth IPO between consecutive years. It is unclear as to why the authors have chosen to use only the January value of the IPO rather than a 3 month summer mean or an annual mean. An explanation of why the authors chose to base the indicator on a single months value is needed.

The authors derive (or calibrate) the WL model and the EC model using data covering 1971-2002 (EC) and 1968-2001 (WL). The authors then apply the model over the entire data set (1960-2002 for EC and 1950 -2001 for WL), thus the verification period for the model covers the years 1960-1970 (EC) and 1950-1967 (WL). The results for this verification period are not presented clearly. In particular, it is ambiguous whether the verification period is included in the results presented in Figures 5 and 8. Perhaps it would be better to show the modelled vs actual data for the verification and calibration periods separately, or at a minimum discuss the R2 for each. Considering the models are developed using data that is predominantly from the IPO positive epoch, it would be beneficial to see how the models perform during years that cover the IPO negative epoch (i.e. the verification period).

In Section 4.2.1 field evidence of the modelled climate signal is discussed. Paragraph one makes mention of results obtained by McNeil and Cox (2002), with Figure 10 displaying results from this study. It is not obvious however if the climate indicator presented in this section is the same as that used in the current study (i.e. change in IPO). The study of 2002 is published as a conference paper and is not easily obtainable;

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therefore a more detailed explanation of these results is required.

In terms of further work it would be interesting to see if a successful climate indictor could be produced using the raw value of the IPO rather than the smoothed value. Due to the techniques used to produce the slow varying or smooth IPO, present values of the IPO cannot be calculated. Therefore in order to assess likely impacts of current or near future climate conditions, as suggested in the conclusion section, it would be advantageous to use the raw value of the IPO.

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