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Discussions

Interactive comment on "On the Relationship between Teleconnections and Taiwan's Streamflow: Evidence of Climate Regime Shift and Implications for Seasonal Forecasting" *by* Chia-Jeng Chen and Tsung-Yu Lee

Anonymous Referee #1

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This paper presents an analysis of correlations between large-scale climate indices and streamflow in 41 Taiwanese catchments. Additionally, a climate regime shift (CRS) analysis is employed to detect changes in the relationships between the climate indices and streamflow across time. Comments are made about the impact of CRS on predictor screening routines and forecasting.

The purpose of the paper is to identify the relationships between climate patterns and Taiwanese high season (July-August-September) streamflow. My understanding of the key findings suggested by the authors are: concurrent JAS correlations are positive

and high for West-Pacific, Pacific-Japan and NAO indices; 9-month averaged preceding climate indices (ONDJFMAMJ) are generally more weakly correlated with JAS streamflow with the exception of the QBO which is negatively and significantly correlated; and climate regime shifts occurred in the 1970s and 1990s.

I suspect the study will not vastly benefit the general seasonal streamflow forecasting community, however it could be of interest in the study region. The writing is not yet publication quality, there is not enough detail for the study to be repeatable, and some choices related to data prevent this study from being clear cut with robust conclusions. My overall opinion is the paper is not coherent enough to be published in HESS at this time. However, I do encourage the authors to rethink certain aspects of the study and seek eventual publication. My general and specific comments are below.

General comments

1) The stated purpose of this paper is to understand climate impacts on seasonal streamflow forecasting (as per the title) in Taiwan. Of concern is empirical prediction (P11 L5-6). P5 L22-23 states that lagged correlations are used to investigate forecasting possibilities. What is not made sufficiently clear is why the concurrent analysis of climate indices and streamflow is included in this study. To make use of concurrent relationships, models would need to be used to forecast the climate indices in the first instance. I suggest clarifying the reasoning and reconsider the weight given to the concurrent results in the paper unless knowing concurrent relationships is actually useful for empirical seasonal streamflow forecasting in Taiwan. Furthermore, the results and discussion interweave concurrent results and suggestions about the implications for forecasting in a way which I interpret as incompatible.

2) A different point, but related to the above. It severely bothers me that the Pacific Japan (PJ) correlations are different to all the others in that they are "semi-concurrent" (P5 L30-32) and not consistent with the separate concurrent and lagged analyses. The JJA PJ index is treated as concurrent for JAS, which in my mind is technically incorrect,

and concerning given the PJ results feature so heavily in the paper. The CRS analysis and discussion, as far as I can tell, hinges on the PJ index (Table 2 and P8 L24-27). It seems to me an effort has been made to include the PJ index because it yields high correlations (Fig 4) and garners some significant CRS results when really it should be excluded or included in a way that is consistent with the other results. Can the authors obtain the full PJ time series and complete the analysis more rigorously and put the PJ correlation and CRS analyses in the context of forecasting?

3) The manuscript is far too scant in the detail of the data and the methodology for it to be repeatable. Questions I have are: a) What is the period of data used? P4 L16 states that an "extended record" is selected. Exactly what is the period of data for each gauge? Are they all the same or different? b) What are the assumptions of the correlation analysis? What are its limitations and how is it suitable for this analysis? c) Is the CRS detection method robust for picking out multiple change points in short data records?

4) For me it would be far more interesting to analyse lagged correlations with increasing lead time as suggested at P5 L28-30, rather than averaging climate indices over the preceding three-, six- and nine-month periods and then presenting the best results. For forecasting, some of the indices are not immediately available after the end of June. E.g. at 15 July 2016, the PDO index for June is not yet published at jisao.washington.edu

Specific comments

5) Related to major comment 3, I suspect the QBO can have a strange distribution. Given it has the most outstanding lagged correlation (overall correlation actually) and the concluding remarks section trumpets its forecasting potential (P11 L10), this should be analysed further. It would be helpful to confirm that the results are not a quirk.

6) In the abstract and elsewhere it is mentioned that the lagged correlations represent 1 month lead time. From my experience, the lagged correlations would be called 0

months lead time, since data up until the end of June is used.

7) Should the RHS of equation (3) be 2(1-r)?

8) P6 L10 – not sure that it is correct to say x = y = 0 if the variables are normalized. The variance analysis doesn't appear to depend on this anyway.

9) Is it possible to mark significance thresholds on Figure 4? I understand it may not be possible if the data records have different lengths.

10) I may have missed the reasoning, but I don't understand why the pink and blue lines in Figure 5 start at different points

Technical corrections (typing errors, etc.)

11) Some captions are far too brief, e.g. Figure 5.

12) P7 L10: I suspect this should be *any* rather than *none*.

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