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Interactive comment on "Quantification of Ecohydrological Sensitivities and Their Influencing Factors at the Seasonal Scale" *by* Yiping Hou et al.

Anonymous Referee #2

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Sorry for the late review on the manuscript by Hou et al.. It is a very interesting study to define the eco-hydrological sensitivity and explore its influencing factors at seasonal scale, using 14 watersheds across climate, soil, topography and landscape gradients in Mainland China. The unique perspective of eco-hydrological sensitivity is attractive and promising, and the results are potentially significant. The article is well written with clear logic and detailed analysis. Overall, I think this article is worthy of publishing. However, there are a couple of major concerns before I recommend for publishing the research. First of all, the methods and data usage are not very much clearly presented. For instance, the definition of dry and wet season is of critical importance as the study covers the climate regions from subtropical in southern China and cold temperate re-

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gion in northeast China. However, authors do not clearly present. Similarly, it seems not clear how the dry and wet season LAIs were calculated for the watersheds located in the very different climate regions? Second, the justification for using large number of topographic and landscape indexes is missing. In reality, almost every feature in the watershed will have impact on the watershed responses, even though some can be ruled out and some are relevant than others mathematically. Thirdly, 14 watersheds studied are subjected to different disturbance regimes hydrologically and ecologically, yet, the separation of stream change in dry and wet season into vegetation change and climate change seems not very much convincing. This point also should be addressed or at least the weakness of current study should be indicated in the discussion and/or conclusion sections.

Specific points are as follows: Line 43: Please make a clear distinction between the dry and the wet season in your study. Line 49: 'shown' is more common. Line 65: Please delete 'in spite of its usefulness'. Line 89-92: Here, the logic is elusive. The background is right in China. But this doesn't mean a good opportunity to explore the index on a short time scale (e.g., seasonal scale). Line 98-99: What were the selection criteria for fourteen large watersheds? And their representativeness is not distinct. Line 115: How can the author define the equally divided dry and wet seasons for the watersheds located in the very much different climate zones across China? This is critical, please specify clearly. Line 117-118: Please give the PET formula. Line 121-123: How to reclassify land cover types? What is the basis for this? Line 124-134: Have you compared the two RS products with observations in the fieldïij§Which is more accurate for your studyïij§ Line 149-150: Remove it. Line 154: Repeat. Remove 'which is calculated by the improved single watershed approach'. Line 157: How do you consider the auto-correlation between the influencing drivers? Line 189: Why estimate the significant at a level of 0.10 rather than usual 0.01 or 0.05? Line 199: How do you consider the interaction effects? Or if there is collinearity between the selected variables? How to overcome this problem? Figures 2-5: They cannot display the differences intuitively and clearly. Please redraw these figures. Line 331: should be ‘a dominating factor’. Line 378-379: What are the uncertainties of the simple multiple linear model for providing a reliable and robust assessment framework based on the selected fourteen watersheds in China? I do believe that authors should address this issue.

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