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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Hydroclimatological influences at multi-spatial scales on recently increased droughts in China's largest freshwater lake" *by* Y. Liu and G. Wu

Anonymous Referee #1

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General: the manuscript by Liu and Wu uses a simple statistical approach to evaluating some of the available hydrological data for Poyang Lake, attempting to make inferences about drought severity/frequency and causal factors. Unfortunately, this has been done previously, using far more rigorous methods. The approach here is really the simplest possible analysis one could undertake. None of the conclusions are new, and in fact, the manuscript casts uncertainty in areas that have been studied by others. The weakness of the written English makes it hard to follow, but it is clear that the current work is inferior to existing studies and it draws, without basis, conclusions about 3GD impacts on Poyang Lake that are not defensible, and contradictory to more rigorous methods.

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The authors need to collaborate with researchers from their own organisation, who are doing far more sophisticated analyses of Poyang Lake drought causal factors using a host of physical hydrological modelling, statistical modelling, climate data analysis, and various other techniques. The current submission is a backward step in efforts to characterise this complex lake-catchment-river system.

Specific comments: Abstract: 1. Overall – unfortunately, there appears to be no new insights above what is already known about Poyang Lake in the Abstract. There doesn't appear to have been a novel methodology used, and all of the insights regarding Poyang Lake were published previously. The Abstract needs to do a far better job of highlighting what is new. It seems to lay claim to novel findings that are clearly published in other journal articles, including some from the authors own organisation. 2. L3: "It may" – "It" is the incorrect pronoun to use here, because the previous sentence has several subjects. 3. L4: "and economy" is grammatically incorrect. 4. L5: "under the changing climate" is awkward and should be "under changing climate conditions" 5. L5: "which is of highly valuable" is weak English also. 6. In general terms, English problems occur with significant frequency. The authors require assistance from a native English speaker to improve the document to a publishable standard. I won't invest heavily in offering writing improvements in the remainder, but a complete and comprehensive overhaul is needed to reach the usual standard for publication – I encourage the authors to seek assistance in doing this. There are many areas where the meaning is obscured by the poor English, or it makes no sense at all – e.g. L11-12 (amongst many other parts), where it states: "At the lake region, water deficiency severed as the hydroclimatic foundation for the worsening droughts." – I have no idea what this means. 7. L9-11 – This conclusion (worsening droughts) is not the outcome of the current study, but is the finding of previous works by Liu et al. (2013), Shankman et al. (2012), Li et al. (2014), and a long list of other studies on Poyang Lake (including a recent paper by Zhang et al. (2014; Journal of Hydrology). The Abstract needs to be clear as to what is a new finding and what was already known. It reads here as though the authors are claiming this as new knowledge, but it is not new. I note that the very

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recent study on Poyang Lake by Zhang et al. (2014) is from the same institution as the authors, and hence it seems odd that the study by Zhang et al. (2014), which undertakes a similar analysis, is not cited, albeit I think it was only recently available online (http://www.sciencedirect.com/science/article/pii/S0022169414004156). Nonetheless, it is odd to receive similar studies from the same institution, but with no cross-over in citation or mention of similar concurrent studies. The authors are strongly encouraged to check with their colleagues and make sure that the current submission takes into account this most recent paper. 8. L13 – It is not enough to comment on increased inflow and outflow without offering insight into the sources of inflows and outflows, when so many previous studies have quantified inflow and outflow sources from their origins – e.g. catchment inflow, incidental rainfall, Yangtze River interactions, pumping, etc. Also, at L14, where it refers to local precipitation, does this mean a lower amount of rainfall on the lake surface, or in the Lake catchment, or in the upstream Yangtze River catchment. 9. L16-18 – The weakening blocking effect of the Yangtze River has already been discussed in a series of papers. This is not new insight, but repeating what several others have stated, based on sophisticated modelling. 10. L18-20 – What is the basis for the statement that 3GD has limited impact? It seems to have been stated without a foundation or evidence. What does it mean by "should be" here? How can something enhance the drought magnitude but not the drought occurrence – this seems almost impossible? 11. L22 – It refers here to the changing climate, but there is nothing in the rest of the Abstract that provides any link to climate change effects or a manner in which these might be discerned. Introduction: 1. P5635, L6 – The author leave out socioeconomic and ecological drought (Tallaksen et al., 2004) from the list (http://www.ncdc.noaa.gov/monitoring-references/dyk/drought-definition) 2. L10-12 – The list of causes of streamflow drought is incomplete. Groundwater pumping is another factor, reservoir construction, river pumping, amongst other factors. 3. L15-17 – This doesn't make sense – stream flow droughts are dependent on climate related changes in their catchments – it is inferring here that they are not. 4. L20-24 – "few studies" is not right. There are dozens of examples of lake studies that explore the

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causes of water level changes. There are even plenty of analyses of the current paper's case study area, so globally, investigations of lake hydrology has seen a massive research investment. 5. L23-24 – A reference is needed for this statement, because I disagree that floods are simple and droughts are poorly understood. 6. L27 – Suggest deleting "Among numerous lakes of the world" 7. P5636, L21-23 – I'm sorry, but this is not a sensible argument: "Definitely, the low water level is different from the drought since the latter may occur in any season (Smakhtin, 2001); thereby the existing studies do not provide a full description of the recent drought events". The drought is by definition related to low water levels. They are inherently linked, and the argument posed here that others somehow haven't studied droughts properly is untrue and ill-based. 8. L26 – The "hidden mechanism" has in fact been well studied already. The lower water levels in the Yangtze River are a key part of the lower water levels. Several papers already identify this. The mechanisms are not "hidden". 9. L28-29 – Indeed, robust methods are used, and previous studies apply these, whereas the current study (having now read the remainder of the manuscript) applies the simplest of statistical analyses. I'm sorry, but this paper is a backward step in the study of Poyang Lake. 10. L29 – Please define how exactly the current study is "multi-scale". This term has been used more than once, but it needs to be clear as to how the analysis is special in some way, and multi-, rather than single-scale. It is simply inferring that different climate stations are considered, then the use of "multi-scale" reads more like the authors are trying to make it sound more grand than it really is. 11. P5637, L2-6 – Stating what the different sections are about is not useful for a journal paper and adds to its length unnecessarily. Reserve this approach for student theses and books. 12. L6-8 – I can't see how the findings are going to be useful in the manner suggested. How is an improved knowledge of Poyang Lake useful for the international body of knowledge? Is a technique being demonstrated that has relevant elsewhere? All of the Poyang Lake process associated with droughts have been studied to death, so I see little opportunity for natural functioning insights for the international community here. Methodology 1. In general, Zhang et al. have undertaken a similar, but more rigorous analysis of trend

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to assess the water balance. Unfortunately for the authors, this manuscript is now available for access by the international community on the Journal of Hydrology web site, and aside from the many other short-comings of this manuscript, it renders their research as inferior and a body of work that has already been undertaken by someone else. 2. L10 – What is the mathematical definition here of magnitude and severity, and how do they differ from duration and spatial extent? In the lines that follow, how is severity mathematically related to magnitude and duration? What is drought magnitude? 3. L15-17 – SPI is a rainfall related measure, but what is written previously is referring to lake hydrology, so how is SPI relevant here. It isn't clear. 4. L20 – "normalised with a gamma distribution" requires explanation and a reference. 5. L21 – Perhaps "standardized deviation" is meant to be "standard deviation"? 6. L19-25 – The SPI is a very simple measure, that should be explained as something like: SPI represents the number of standard deviations from the mean (monthly average) rainfall. The explanation, that flows over onto the next page, reads as excessive – it really is a simple parameter and warrants a simple mathematical treatment. 7. P5638 - An SPI value of -1 to 0 does not infer drought. It is simply a month rainfall value less than average. This doesn't mean that a region is in "drought". A more thoughtful approach is needed here, because drought is defined as a sustained and regional extensive occurrence of below average water availability. A single slightly below average month of rainfall does not equate to drought. There needs to be threshold which distinguishes drought from non-drought (Tallaksen et al. 2004; Page 6) 8. P5639 – Equation 3 variables need units. It seems to me that this doesn't make sense, because you can't add rainfall rate to a lake inflow. One has an areal extent that differs to the other, so catchment areas are needed here. In any case, I and ET, for a lake, are part of I and O. 9. Equation 4 is simply the SPI index without normalising to the standard deviation. It is odd to have two very similar variables such as these. 10. Equation 5 only makes sense if all of the terms in equation 3 are in the same units. My other issue with equation 5 is that part of the denominator represents the error in the calculation of the water balance, and not necessarily the deviation due to a climate impact. Study Materials and Data

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Processing 1. “Study materials” is an odd term to use for the study area. This section is poor in referencing. The sources of the information here should be disclosed, rather than the authors claiming these facts as theirs. 2. P5640, L27-28, and P5641 – This is not an adequate description of the method for calculating discharge data for the five sub-basins of the catchment. What is the accuracy of 90.4% based on? How were these estimated? How was “discharge” estimated (L5)? There is not enough information here to follow the process. This also reads as Methodology and not study area. The Data Processing approach belongs with the other methods in the Methods section. 3. P5641, L8-9 – There is no need to redefine the meaning of SLI. 4. L10-11 – The authors now change their definition of drought compared to earlier and are making up new definitions as they go. Now, it is 1 SD difference that warrants a “drought”. 5. L13-15 – What is the consequence of these assumptions regarding the methodology for defining a Lake drought? Is the method for Lake drought and rainfall drought consistent? 6. L18-19 – A catchment the size of Poyang Lake will have variable rainfall across it. What is the error by assuming the rainfall is uniform? 7. L21-22 – What is the difference between lake region and lake basin? 8. P5641 generally – It has already been discussed that the Yangtze River is a factor, but it isn’t included anywhere, so this seems like an oversight in the analysis. 9. L26-29 – Now it seems that lake rainfall and evaporation are ignored! The authors are changing the methodology as they go. The manuscript reads as a journal of what they did rather than a logical sequence of research description. How were they implicitly included as part of the water budget? 10. L5642 – What is a F test and a T test? It is unexplained. Results and Discussion (there should be no “s” here) 1. From this point, I am ignoring both the very weak English, the illogical structure, the text that is wrong placed (i.e. the first sentence of Section 4.1 are not Results and Discussion, but is Introduction), and am focusing on simply the nature of the findings. 2. Figure 2 vertical axes are not all labelled. 3. P5642-5646 – this lengthy discussion of a run-of-the-mill drought analysis carries no relevance to anything except the Poyang Lake area. What hypothesis is being addressed here? What is the international significance of this? If the reader is not interested in Poyang

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Lake, why is this important? This reads as the results of a Chinese Government report and not an international journal paper. This is simply a description of very basic elements of freely available data for a particular region, which has been evaluated more thoroughly by others. 4. P5648-5649 – It has already been stated in previous papers that the blocking effect of the Yangtze River is the key factor in the increased droughts of Poyang Lake. The results are presented as though this fact is unknown, and that the current analysis reveals it, but the authors had the answer before they started, and of course they will arrive at the same outcome because they are using the same data and largely the same sort of analysis as previous authors. 5. P5649 – Zhang et al. (GRL) have done an analysis of the effects of 3GD on Poyang Lake, but this is not mentioned or considered here, and rather, the authors are drawing their own conclusions using an inferior methodology, and as though the wealth of previous investigations don't exist. Research is meant to build on the findings of previous studies; this manuscript take a very significant backward step and makes statements about uncertainty where previous studies have done thorough investigations. 6. P5649, L25-27 – There is absolutely no basis for this statement whatsoever. It reads as though the authors have more of a political intention than a scientific one with statements like this. The current method is grossly simplified and in no way is able to determine that 3GD is not impacting Poyang Lake. Conclusions: 1. The use of multi-scale has no meaning here – the authors are simply using different available data sets, doing the simplest of statistical analyses, and then trying to draw conclusions which don't exist, or have already been stated, but are marketed here as being novel. 2. None of the Conclusions are new and internationally relevant, or demonstrate the usefulness of a novel methodology, or the application of an existing methodology in a novel way.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 5633, 2014.