

Anonymous Referee #1

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.

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.

Response:

We thank the reviewer for his/her time in making all the comments. We got confused about many comments from the reviewer. From the comments, we wonder that we may have failed to adequately state the unsolved problems relevant to Poyang Lake, the differences between low water levels and lake droughts for the lake and their causation structure, the difficulties to obtain independent observation data for drought analysis with general water balance, and the advances in present understanding of the Poyang Lake droughts. Our inadequate description may have led to incomplete understanding of the topic addressed, if the reviewer had no pre-determined intention to depreciate it. This is the reason that we can speculate for the paper to receive the negative comments. Therefore, we made substantial revisions to our MS. We hope our revised MS could satisfy the criteria for publication. We would like to say, we very much thank the reviewers who made constructive comments to improve our manuscript (MS).

As non-native English speaker, we understand that we are certainly on the side being criticized for improper English expression and definitely remain weak in perfect expression to present our work. It would be highly appreciated if the reviewer could be more specific in the comments.

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.

Response:

We truly got confused about the comment states that our findings “are clearly published in other journal articles”. We would appreciate it very much if the reviewer could clearly list in which paper on which page the findings are published. We wonder if the reviewer got mixed some existing conclusions with ours; therefore, in the revision we spent more spaces to explain some of concepts and conclusions for a better understanding.

2. L3: “It may” – “It” is the incorrect pronoun to use here, because the previous sentence has several subjects.

Response:

The sentence was removed from the manuscript.

4. L5: “under the changing climate” is awkward and should be “under changing climate conditions”.

Response:

Change as suggested.

5. L5: “which is ~~of~~ highly valuable” is weak English also.

Response:

Remove “of ” from the sentence.

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.

Response:

“At the lake region, water deficiency severed as the hydroclimatic foundation for the worsening droughts” was removed from the MS. We highly appreciated all the specific comments that can improve the MS.

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 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.

Response:

We agree that “worsening droughts” are frequently reported in Media or mentioned in some papers. However, the studies the reviewer listed here including our work (Liu et al. 2013) focused on long-term change of lake stage/size or seasonal change of low water level mainly in autumn, on a background of the recent droughts. Indeed, long-term change or seasonal low water level is not equal to drought. Only a few paper explicitly addressed 1-2 drought events (Feng et al. 2012; Wu and Liu 2014). We clarified the point in the revision.

We got confused again that the reviewer used much page space to state that we did not cite the work by Zhang et al (2014). Wordily, we have no intention to hide any papers as if it is closely related to the topic to be addressed. Moreover, in what specific way

to collaborate with other scientists seems beyond the contents of the paper. Indeed, we do have close cooperation with Zhang et al and other colleagues studying Poyang Lake. The papers by Guo et al (2012), Liu et al (2013), Zhang et al (2012), Lai et al (2014) and Zhang et al (2014) all are the works on Poyang Lake from our institute. It is not odd that we do not know every paper before its publication, even in the same institute. Also, we may have different opinions even on the same topic. For example, you may find that the TGD-induced effect on Poyang Lake level differs in magnitude, among Guo et al (2012), Zhang et al (2012), and Lai et al (2014), if the reviewer has carefully read the papers.

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.

Response:

The present study provides change of inflow and outflow and their sources, with a focus on their contributions on droughts.

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.

Response:

We are really shocked with the comment. We believe that most works are built upon others. We did not plagiarize other works, and cannot spend space in an abstract to discuss the effect which is only a partial of the whole story. Does the reviewer mean we should remove the “weakening blocking effects” from the text?

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?

Response:

First, we revised our statements on 3GD based on updated analysis. Second, in the

revision, we made more descriptions to avoid the mixture of drought occurrence with drought magnitude (intensity).

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.

Response:

The “changing climate” was replaced with “hydroclimatic” to avoid the misunderstanding.

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>).

Response:

They are added into the revised MS (line 55-56).

2. L10-12– The list of causes of streamflow drought is incomplete. Groundwater pumping is another factor, reservoir construction, river pumping, amongst other factors.

Response:

They are added into the revised MS (line 71-72).

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.

Response:

The statement was revised (line 65-66)

4. L20-24 – “few studies” is not right. There are dozens of examples of lake studies that explore the 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.

Response:

The sentence was removed to avoid misunderstanding.

5. L23-24 – A reference is needed for this statement, because I disagree that floods are simple and droughts are poorly understood.

Response:

Two review articles were added here (line 76).

6. L27 – Suggest deleting “Among numerous lakes of the world”

Response:

Deleted from the text.

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.

Response:

The description was substantially expanded in the revision (line 95-141).

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”.

Response:

It was removed from the text.

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.

Response:

We made substantial revisions to the MS and highlighted the strengths of the present study.

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.

Response:

Multi-scale water budget analysis was designed. The relevant statements were revised for more accurate description.

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.

Response:

We wonder if the reviewer’s comment asked the authors to remove the paragraph. In according to “Write an aim statement, or a statement describing what the paper sets out to do. It is usually the easiest part of the Introduction to write. It will appear in the final paragraph of the Introduction, but it is useful to write it early in the drafting process.” (Page 50 in “Writing Scientific Research Article: Strategy and Steps”, 2009), we did not remove the paragraph but changed the description (line 153-164).

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.

Response:

The existing studies seem not support “Poyang Lake process associated with droughts have been studied to death”. We supply more descriptions in the revision for the misconception (line 95-141).

Methodology

1. In general, Zhang et al. have undertaken a similar, but more rigorous analysis of trend 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.

Response:

It is a repeated statement.

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?

Response:

Subsection 2.1 was added to expand the drought description (line 181-232).

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.

Response:

SPI was proposed and applicable to many hydrologic variables, but not only for precipitation, even in the first paper (McKee et al. 1993). In the revision, more references are added into the text (line 194-196).

4. L20 – “normalised with a gamma distribution” requires explanation and a reference.

Response:

A reference is provided (line 203).

5. L21 – Perhaps “standardized deviation” is meant to be “standard deviation”?

Response:

This is a mistake. It was revised as pointed out (line 204).

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.

Response:

The excessive description was removed from the text (line 218-219).

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).

Response:

Threshold for distinguishing the drought from non-drought is in agreement with drought classification with SPI. More description was added in the revision (line 208-215).

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.

Response:

Units for equations are added into the text. Equation 3 was revised for a clearer description.

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.

Response:

In the revision, we added descriptions on the differences between two equations (line 246-250).

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.

Response:

Equation 5 uses monthly values. The values from independent observation contain errors of systematic or random. Given the monthly values and the simple form of the equation, the random errors would be minimized. Even for the systematic errors, it would not propagate into other independent observed values (line 394-397).

Study Materials and Data 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.

Response:

The section title was revised (line 301).

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.

Response:

The validation procedure was removed from the text, which was already described in our previous study (line 390-394).

3. P5641, L8-9 – There is no need to redefine the meaning of SLI.

Response:

It was removed from the text.

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”.

Response:

The description was combined into Section 2.1 (line 208-215).

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?

Response:

The description of the methodology was expanded (section 2.1, line 181-196).

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?

Response:

In the revision, we updated the precipitation with the data from more (73) rainfall stations. The update does not significant change our results about precipitation.

7. L21-22 – What is the difference between lake region and lake basin?

Response:

We expanded Figure 1 to illustrate it and defined it in the text (line 285-299).

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.

Response:

We agree that the Yangtze River is ONE of the factors, contributing to the lake droughts.

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?

Response:

In the revision, we substantially expanded the description on water balance (section 2.3).

10. L5642 – What is a F test and a T test? It is unexplained.

Response:

We provide a textbook as a reference for readers who may be not familiar with statistical analysis (line 402).

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.

Response:

The text was substantially revised. It would be highly appreciated if the reviewer could be more specific and constructive in comments.

2. Figure 2 vertical axes are not all labelled.

Response:

More descriptions were added into the caption of the figure to account for it.

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 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.

Response:

The section was revised substantially.

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.

Response:

The lake droughts are affected by both inflow and outflow. Only when outflow is larger than the inflow the lake shrinks; otherwise, it expands. It seems that the published works focusing on outflow due to the Yangtze River dominate in recent years. That is not to say, outflow plays a dominate role in forming the drought, as a whole of the story. One of the differences between the existing and the present study is that, one focus on short-time e.g. 1-2 autumn months, but the droughts span several months or longer.

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.

Response:

It is similar to a previous comment. In the revision, more concrete data, for example lake stage without TGD, were used to update the conclusion.

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.

Response:

It is similar to previous comments. Substantial revision was made to update the methodology and enhance the conclusion.

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.

Response:

It is a repeated comment. Multi-scale water budget analysis was designed. The relevant statements were revised for more accurate description.

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.

Response:

It is similar to previous comments. Substantial revision was made to highlight the new of the present study.