Anonymous Referee #2

General comments

The manuscript illustrates a multi-scale drought analysis aiming at investigating the causes of the recent increase in the frequency of severe droughts in the Poyang Lake (China). In particular, drought identification and characterization is carried out through the Standardized Lake stage Index (SLI), which is computed similarly as the Standardized Precipitation Index (SPI) (McKee et al., 1993) by using monthly lake stage series instead of precipitation series. Also, the authors apply a simple water budget equation to investigate the influence exerted by changes in both climatic and hydrologic variables in the observed drought events.

Although the paper presents an interesting topic, it does not add any contribution to the current knowledge on drought analysis, both from a methodological and practical point of view. Indeed, the methodology does not show any novelty with respect to consolidated techniques; moreover, the case study is not original as well, as the Poyang Lake has been the subject of many studies investigating the effect of the modifications of the Three Gorges Dam on the Yangtze River and on the hydrological regime of the lake (e.g. Zhang et al., 2012 and 2014).

In addition, the proposed methodology, apart from being not novel, is, in my opinion, objectionable in several aspects, which may seriously compromise the foundation of the derived results.

Finally, from a stylistic standpoint, the paper requires an in-depth revision of the language.

Overall, I think that the paper should undergo major revisions. Specific comments follow.

Response:

Thank for your time to review and comment on the paper. From the comments, we realized that we may have unintentionally ignored some elements in describing the background of the present study, the methodology we used, the results and conclusions. With constructive comments from all the reviewers, we made substantial revisions to our manuscript (MS). We hope the revised MS could satisfy the criteria for publication.

Major comments

A first criticism concerns the data set used for computing the drought monitoring index (i.e. SLI), namely monthly lake stage data at the Hukou outlet (see p. 5641,

lines 7-8). Since the Poyang Lake is rather big, with a maximum area of 3860 km2, I believe that the lake stage data observed at the outlet are unlikely to be representative of the variation in the water level of the whole lake. Is Hukou the only water level gauge station within the lake? If other stations are available, it would be better to determine an average water level by spatially interpolating contemporary local values. As an alternative, for the considered case study, the lake volume or the lake surface area should be considered as the reference variables in order to achieve a reliable drought analysis. Of course derivation of such variables implies the availability of bathymetric or satellite maps of the lake. As a matter of fact satellite images of the Poyang Lake are available from various space missions, and several studies have already applied these images to investigate the spatio-temporal change of the Poyang Lake. Maybe the authors should take into consideration the possibility to capitalize on the results of these studies to make their analysis more grounded.

Response:

Upon the comment, in the revision we finally determined to use the lake stage data at Xingzi in replace of Hukou, based on careful consideration and comparison (Line 335-341). Thank you very much for the comment.

Another critical point is related to the adopted time scale for computing SLI, i.e. one month (see p. 5638, line 22). In the original paper by McKee et al. (1993), monthly precipitation are aggregated on a period of k months, where k is 3, 6, 12, 24, or 48 months, representing “arbitrary but typical time scales for precipitation deficits to affect the five types of usable water sources” (e.g. soil moisture, ground water, snowpack, streamflow and reservoir storage). Although the authors consider lake stage instead of precipitation series, I think that, given the considerable size of the lake, one month time scale may be inadequate to account for drought effects on the lake storage mechanism. Besides, as for the SPI, 1-month SLI may be misinterpreted unless the seasonal variation of the lake stage are properly taken into account. In fact during the dry months where water level is normally low, large negative or positive SLIs may result even though the departure from the mean is relatively small. Perhaps a 3-month SLI would be a better choice. In any case, a sort of sensitive analysis should be carried out to select the appropriate aggregation time scale, also involving comparison with other drought indices, such as 24, 36 or 48-month SPI series.

Response:

The time scale is the minimum time resolution for describing hydrological processes.

K-month is an averaging window for a time series and it would remove the events less than K months. Consider the ecological effects of lake variation and the consistency with conventional use of other standardized indices, we prefer to use monthly data. In addition, because the index uses the monthly average and the standard deviation, both of which are monthly dependent, it removes seasonal differences in lake stage (line 205-206).

Precipitation data from 13 weather stations within the Poyang Lake Basin (with an area of 162,225 km2) are grouped and averaged for Poyang Lake region, five sub-basins and the whole basin (see p.5640, lines 15-19). I have two criticisms on this point. First, 13 stations are not enough to properly describe the spatial variability of precipitation over such a large region. Besides, since the areal precipitation is computed as an average, the orographic effect of mountains is totally disregarded. There are several interpolation techniques which enable to compute areal precipitation by also considering the orography of the investigated area.

Response:

Upon the comment, with careful review of relevant papers, we find no interpolation techniques commonly accepted for precipitation. Instead, we updated precipitation with data from 73 stations in the revision (Figure 1b). Use of the data did make minor differences, and did not obviously change our results and conclusions. Orographic effect is of importance to sub-basin and we will keep this in mind in our ongoing study.

With reference to the water balance, apparently all the components (i.e. precipitation, evapotraspiration, inflow and outflow) are referred to the same time period. Once again, given the large extent of the region under study, a proper lag time should be considered both between the climatic variables and the inflow (to account for the delay in the response of the major five river basins supplying the lake), and between the inflow and the outflow (to account for the storage process within the lake). In my opinion, the authors should examine this aspect in depth and check whether considering a lag time could substantially change the results reported in Tables 2 and 3.

Response:

There 1-2 days lag between inflow and outflow, and the lag is negligible for the monthly data in the present study. One-month lag was determined with correlation

analysis between peak rainfall and peak discharge for the basin. This is in agreement with the reports by Senay et al.(2011) and Liu et al.(2013). The statements were added into the text (line 381-383).

Minor comments

The definition of drought magnitude applied in the present study should be clarified in the methodology, as in literature it can assume different meanings (see Keyantash and Dracup, 2002). In this study, drought magnitude is defined as the lower negative value in a sequence of consecutive negative SLI values preceded and followed by positive SLI values (see p. 5641, lines 10-13), which identifies a drought event. In other studies, drought magnitude is defined in terms of drought intensity, namely as drought severity divided by drought duration.

Response:

As you pointed out, there are even some inter-change uses of the two words. We agree with you and we adopted the definition by Keyantash and Dracup (2002) (line 231-233). Because the magnitude is a derivative of drought severity, we did not use it substantially in the revision. Thank you very much for the comment.

In Section 4.1, classification of identified drought events is made according to the magnitude, thus, for instance, a drought event is classified as “extreme” if there is at least one month within a drought period with SLI<-2. This approach sounds misleading with respect to the one commonly applied where dry and wet conditions are classified month by month, according to the values of the considered drought monitoring index. In particular, the proposed approach makes the comparison between drought events less straightforward. For instance at p. 5642, lines 21-25, the authors state: “Among the five droughts in the “moderate drought” category, the longest occurred from October 2003 to August 2004, lasting 10 months. Although it was classified as a moderate drought by magnitude, its drought severity was comparable to the second most severe one . . .”.

Response:

We re-examined the relationship between the drought severity and the drought intensity with the updated data of precipitation and lake stage. The results showed that they are well correlated (line 407-429). We highly appreciate it for the careful evaluation and the useful comment.

Technical comments

p. 5634, lines 6-7: “This study proposes to use a multi-scale hydroclimatic analysis for the determination, taking Poyang Lake as an example.”. Determination of what?

Response:

The sentence was removed in the revision.

p. 5634, lines 11-12: “At the lake region, water deficiency severed as the hydroclimatic foundation for the worsening droughts”. Awkward! Please rephrase the sentence.

Response:

The sentence was removed in the revision.

p. 5635, lines 17-18: “both inflow and outflow may alter with anthropogenic influences”. Awkward! Please rephrase the sentence.

Response:

The sentence was revised accordingly (line 70-71).

p. 5636, lines 15-17: “If the River’s blocking effect weakens, more lake water will flow out into the river (Shankman et al., 2006; Hu et al., 2007; Guo et al., 2012), thus making it more complicated to determine the controlling causes of the increased lake droughts.” The possible causes of the weakening of the river’s blocking effect should be mentioned here.

Response:

We made substantially revisions to the MS and the sentence was removed from the text.

p. 5636-5637, lines 29: “This study proposes . . .”. Again, determination of what?

Response:

The sentence was removed from the text and this paragraph was re-written in the revision.

p. 5637, line 4: change “hydrocliamtic” into “hydroclimatic”

Response:

Corrected as pointed out.

p. 5637, line 19: delete “normalized” before “monthly precipitation” as it is repeated right after.

Response:

Deleted as pointed out.

p. 5638, line 22: “. . . as did in Keyantash and Dracup (2002)”. I do not think that reference to the work by Keyantash and Dracup (2002) is appropriate here. Please check wheter Keyantash and Dracup used 1-month SPI in their study!

Response:

The section was substantially revised (Section 2.1) and the sentence was removed from the text.

p. 5641, line 26-27: “Because both the lake precipitation and evaporation occupy less than 2..

Response:

The section was substantially revised (Section 3.3) and the sentence was removed from the text.