Dear Editor and Reviewers:

Thank you very much for the valuable comments provided. We have addressed all the comments as shown in the revised manuscript. We have restructured the paragraphs, added more text, references, redrawn and added figures and also expanded on the discussion.

Authors

Anonymous Referee #1

Received and published: 13 October 2010

Title: Stage Level, Volume, and Time-frequency Information Content of Lake Tana using Stochastic and Wavelet Analysis Methods Authors: Chebud, Y., Melesse, A. Manuscript submitted to HESS Disc.

The paper presents an approach for predicting lake water levels of Lake Tana of Ethiopia using a stochastic approach based on perturbation method, Monte Carlo and Wavelet analysis. A comparison is done with results obtained elsewhere with terministic approaches applied to the same case-study. The main comments are:

1. The manuscript is difficult to read. The introduction to the problem, methodology analysis of results and conclusions are too short for the general readability of most readers of HESS. All these sections need to be elaborated giving a formal introduction and explanation of the results obtained.

If space is the problem then I will recommend to remove some components (e.g. the wavelet component).

Response: We have now improved the organization of the manuscript and added more paragraphs.

As the wavelet analysis is part of the objective to understand the frequency content of Lake Tana, the topic is kept but the results are presented in a more elaborated way.

2. The first figure on pp 3 is without a number and caption. Please provide numbers and captions to all figures and refer them in the text.

Response: Thanks and corrected

3. Figure 2: I have difficulty in connecting the explanation with the figure. Both normal and log-normal plots look similar, and R2 is not terribly different.

Response: Thanks for the comment, decision is already made to use the normal distribution and hence avoid any confusion. The log-normal distribution was not included in the revised version.

4. Figure 3: it is not clear how the authors can conclude about the high periodicity of monthly data.

Response: This time an autocorrelation of the monthly data is plotted and added as figure 4b to show the periodicity.

5. The authors claimed that the mathematical formulation adopted in the manuscript follows that of Amemiya et al. I failed to find any connection with the referred paper. The authors need to have a serious editing of the literature provided. Currently, the literature provided is fairly inadequate. Stochastic approaches and wavelets have been applied in hydrology by many others.

Response: Thanks, the reference has been changed before the final type setting it is incorporated by mistake. The references related to this topic are referred after Willen Anders and Yevejvich that deal on stochastic hydrology and indicated on the revised version..

6. Equations 1 to 7 are the cornerstone of the presented methodology. This section does not have enough information to have clarity. There is very little reference (including the reference which I could not connect). Equation 2 and 3: for clarity the symbols for averages also should be defined.

Response: Thanks, references are indicated in the revised manuscript. As a matter of fact, these methods are general approaches to perturbation methods as it is well

elaborated on the referred material and they are common to all physical based models that intend to separate the mean from the noise. We have also added some clarity by adding more explanation on the terms used.

7. Figure 4a and 4b: these figures are difficult to read. Indeed the perturbation and the Monte Carlo method simulate the droughts of three mentioned years. Have they been compared with the actual drought level of these years? How is the performance of these models for other years? There is no reference for the results obtained with the 'waterbalance method'.

Response: Yes, the results are compared with the minimum and maximum lake stage measured at all the time including the drought years. Their performance is also included after assessing the root mean squared error as well as using a paired-t test for statistical significance for the water balance and stochastic models. The revised figures, now Figures 5a and 5b show the results. Text explaining the comparison results and statistics in included in the revised version.

8. The claimed complementarity of the wavelets to the stochastic approach is not obvious.

Response: It is elaborated this time more explicitly that the Monte Carlo and Perturbation approaches performed well for annual lake stage level simulation whose stage level is found somehow independent as observed from the autocorrelation. Whereas the Wavelet analysis is used for reconstructing periodic stage level which happened to be daily / monthly lake stage. In view of complementarity, the wavelet analysis observed to play significant role in daily or monthly lake level generation for forecasting while the Monte Carlo (found qualified in this paper) could use the generated signal for annual stage level prediction and risk analysis.

9. A location map could be useful.

Response: Thanks, added this time (Figure 1)

10. The conclusions are really a summary of the results obtained. The authors need

to provide a bulleted list of conclusions that can be substantiated from the study.
Response: We have now expanded on the conclusion reflecting your comments. The key aspect of each conclusion is written as a paragraph this time. This included the superiority of Monte Carlo over the perturbation method (the latter being propagating the error). An attention drawing phenomenon of seemingly symmetric behavior of Lake Tana before and after 1984 drought as observed in the frequency-time decomposition. Recommendation is drawn to use the Monte Carlo and Wavelet methods for system level operational planning of Lake Tana.

11. Other comments:

a. Equations are hard to read (may be pasted as pictures?)

Response: we have now rewritten the equations

b. Language editing is needed, which I am convinced that the authors themselves can do it.

Response: Editing is done on the revised version

Page 4 refers to Ameniya et al., which actually is Amemiya et al.

Response: Thanks and corrected

Recommendation: Major revision. The manuscript needs to be updated with the comments

1. provided, and should be re-submitted for re-review.

Anonymous Referee #2

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General comments: The paper comes along with a very interesting analysis of the water level fluctuations of Lake Tana in Ethiopia. It aims at a better understanding of extended drought periods which have been observed repeatedly and could not be reconstructed by water balance approaches. Given the uncertainties and failures of deterministic models, alternative ways of predicting Lake Tana water level fluctuations are required. To that end, different stochastic approaches were applied. Comprehension of the paper is substantially complicated by the poor quality of the English text.

The paper needs substantial reworking with respect both to presentation of the results and for language editing.

Response: We have now done a rigorous editing, formatting and reorganization as per the comments.

Specific comments:

1. According to paragraph 5.2, the wavelet analysis served two different purposes. One of these was investigating the effect of temporal resolution of the data on. However, that objective should be announced in paragraph 3.2.

Response: Thanks and the objective is revised this time

The second objective was to check for constancy of the spectral properties of the time series. However, most of the spectral power belongs to the low-frequency range with period lengths that exceed the length of the available time series by far (Fig. 6). This part of the spectrum can hardly be interpreted. The inferences given at page 5537, line 16-18 are hardly supported by Fig. 6. Moreover, Fig. 6 as well as the results by Chebud and Melesse (2009) (p.5531, l. 20-22) seem to be at odds with p. 5536, l. 27-28.

Response

- Graphical plots are labeled this time for ease of the interpretation as well as to support the inferences.
- The results in Chebud and Melesse (2009) are based on the assumption that the lake level occurrences are discrete and hence a discrete Fourier transform had to give discrete frequencies. In this analysis, the Wigner time-frequency resolution

requires an assumption of a continuous signal. Even then the signal continuity assumption has resulted in few observable frequencies with higher spectral power. So, it is the assumption and methodological change than the results.

2. The null hypothesis of the Kolmogorov-Smirnov test is a Gaussian distribution of the data. An error probability of less than 0.01 (p. 5535, l. 5-7) implies that the hypothesis of Gaussian distribution is rejected rather than being confirmed. However, it is well known that hardly any real-world data set would pass the Kolmogorov-Smirnov test when the sample size exceeds a few hundred data points. Has the Lilliefors correction been used for the Kolmogorov-Smirnov test?

Response: ????

3. P. 5535, l. 11-17: The autocorrelation analysis can give some hints on periodicities in the data set, but it is not a rigid test. It should only be interpreted with respect to the temporal correlation length, that is, the memory of the system.

Response: We agree and thanks for the comment.

Thanks. This time the monthly autocorrelation is added to show the extent of periodicity differences between the annual average stage level and the monthly / daily stage levels.

4. In the copy I got, the figures are too small, of poor quality and different lines can hardly be distinguished.

Response: The figures (5 of them) are redrawn and new figure is added

5. Fig. 3: Obviously a spline function is used for the plotted line. That should be avoided because it suggests a continuous function rather than discrete points.

Response: Thanks and it is corrected in this version.

6. Fig. 4a+b: As far as I got it, measured data of annual mean lake water level are not included in these figures. Please add.

Response: It was also included in the earlier version but was not readable due to the poor figure quality. In this version, we have clearly shown the observed values.

7. Fig. 5: I could not discern any differences between the blue and pink time series. That might be due to the poor quality of the figure. If not, I recommend to give, e.g., the Pearson correlation coefficient instead.

Response

The new figure clearly shows the difference between the two time series data.

8. Results of the study should be discussed more extensively with the literature. Half of the given references are textbooks or technical papers rather than referring to corresponding studies.

Response: We have added new literature reviews.

Technical corrections:

1. P. 5529, l. 13: Should be Q(t) (L3/T)

   Response: Thanks, corrected

2. P. 5529, l. 14-16: The small sketch (“free diagram”) should be discarded.

Response: Thanks, removed

3. Paragraph 5.2 and Fig. 5: What does the vertical meter bar indicate in that figure? There is a substantial inconsistency using “Cs/s” (in the figure caption), “cs/s” (in the figure) or “s/cs” (axis label in the figure). Why do you divide the reconstructed signal by the original signal (or vice versa)? At the x-axis, years should be given instead of days.

Response: The newly drawn figure clearly shows the changes and now it is more readable.

4. It should be “Kolmogorov-Smirnov test” instead of “Konglomorov-Simrnov test”

Response: Corrected

5. Fig. 5: Figure titles should be discarded, because they are redundant to the figure caption.

   Response: Thanks and it is corrected