Response to Reviewer #2

Review comments on "Flow regime change in an Endorheic basin in Southern Ethiopia" by Worku et al.

We would like to thank the reviewer for the compliment, as well as for the thorough review of the manuscript. We have considered each comment carefully and in this document provide our response to each. Reviewer's comments are included in bold for easy reference. Where we have made changes to the manuscript we have included the changed text in this document. These are marked in red. Line numbers indicating where the text has been changed refer to those in the original manuscript.

The paper describes the results of analyses of temporal trends in a number of indices derived from hydrological and meteorological data, as well as from satellite data. The focus of the analyses is an endorheic hydrological basin in southern Ethiopia.

The paper is interesting to the broader audience, as it has a potential to, firstly, analyse a relatively comprehensive set of indices describing such aspects of hydrological time series as magnitude, timing, duration, frequency, and variability, which is not frequently encountered in literature. Secondly, the paper has a potential to explore consistency between datasets of various nature and origin in the context of explanation of the observed hydrological variability, again, the feat relatively rarely delivered in hydrological literature.

The paper is written in a clear and grammatically correct language (as far as a non-native English speaker can tell), and has good structure.

Authors' response:

We would like to thank the reviewer for the compliments, as well as for the thorough review of the manuscript. We have considered each comment carefully and in this document provide our response to each. The reviewer's comments are included in bold for easy reference. Where we have made changes to the manuscript we have included the changed text in this document. These are marked in red. Line numbers indicating where the text has been changed refer to those in the original manuscript.

Major comments:

1. The introduction section focuses strongly on the ecological implications of hydrological variability. However, the remainder of the paper does not deal with ecological aspects at all. Even in the conclusions section, there is no sign of interpretation of result within

ecological or hydro-ecological context. This imbalance has to be adjusted. I would suggest the introduction is re-focused on aspects of hydrological change and consistency between data sources, and the ecological meaning and role of indices is kept to the minimum.

Authors' response:

The natural flow regime characteristics and changes are often applied for the analysis of wellbeing of the ecosystem (ecology). It was our intention to highlight this in the introduction. However, we agree that this goes beyond the scope of the paper, and we have reduced the discussion in the introduction, notably in the paragraph from line 16-24 on page 1303. This paragraph has been reduced and rephrased to place less emphasis on the ecological aspects. The remaining sentence of the paragraph has been joined with the previous paragraph:

These changes will result in changes to hydrological characteristics such as magnitude, duration, timing, frequency and rate of change of flow rates, and it is important to study these as they provide indication of the wellbeing of the riverine ecosystem (Lytle and Poff, 2004).

2. Similarly, the focus of the introduction seems to be on description of variability and heterogeneity, while the paper presents mostly results of analysis of change (trends), and the only aspect of variability and heterogeneity is presented in the form of homogenous regions. Again, this imbalance should be adjusted.

Authors' response:

We have, as suggested in the previous comment, shortened the introduction. To focus more clearly on the work presented in the paper we have amended the sentences, replacing the words variability with change/trend.

3. On the basis of the analyses of 20-year of data, the authors detect the rising trend in Lake Turkana water levels. They attempt to explain it through the land cover change, and attribute fluctuations around trend to shorter-term variability (in abstract: "The long term trend of the increasing levels in lake Turkana is related to these trends in dry season flows, while shorter term fluctuations of the lake levels are attributed primarily to anomalies in consecutive wet and dry season rainfall"). Importantly, looking at Fig. 5, the overall trend is likely a residual of the decade-scale fluctuations, and to explain it, one would need to explain these fluctuations first. There is an attempt to do so in section 3.4, summarised by statement in conclusions that "Multi-annual fluctuations in lake levels were related to periods of drought or anomalously wet rainy seasons" but this is not quantitatively illustrated. Perhaps if the authors plotted and analysed running average rainfall or used a method of rainfall time series analysis accounting for persistence of anomalies (e.g. cumulative rainfall departure), the relationship between lake levels and hydrological inputs would be clearer. The statement that the LULC has any influence on lake levels can be only

justified after analysis of residuals of a quantitative relationship between rainfall/evaporation and lake water levels, or by analysis of sensitivity of lake water levels to dry season flows (i.e. showing that quantitatively, the increase in dry season flows is indeed of magnitude that could explain the rising water levels). This, however, has not been quantitatively done. The last part of the statement "changes in land use and land cover in the humid parts of the basin, which have led to changes in the hydrological processes, resulting in [increased] dry season flows, and subsequently to a rising trends in Lake Turkana" is thus not really supported by the data and analyses.

Authors' response:

We have extended the analysis with a quantitative analysis of the rainfall residual and plotted these against lake level fluctuations to show whether multi-annual fluctuations in lake levels were related to periods of drought or anomalously wet rainy seasons. This has been added to Fig. 5, showing monthly cumulative rainfall departure plotted against lake level. This shows that fluctuations in the lake levels are closely related to the cumulative rainfall departure. The analysis of Lake level fluctuation versus cumulative rainfall departure has been included in sections 3.4, 4.1 and 4.2.

The following sentence is added in section 4.1 to clarify:

Although the declining lake levels are not matched by the inflow trend (Fig. 6), the value of the consecutive cumulative rainfall departure (calculated as the cumulative of departure of rainfall from the mean value) does show a clear decline during this period, as shown in Fig. 5. The relatively short period for which the results of Abera, 2012 were available meant that a reliable analysis of trends could not be carried out.

The following sentence is added in section 4.1 to clarify:

However, consecutive high or consecutive low rainfall seasons could be seen, leading to consecutive seasons with increase/decrease cumulative residual rainfall. With the exception of the first period in Fig. 5 it can be seen that the pattern of the cumulative residual rainfall matches the seasonal lake level fluctuations (see Fig. 5).

4. There is a lack of agreement between various datasets in terms of direction of trends. While the authors clearly present this lack of agreement, they fail to critically discuss the possible causes. There is no discussion on quality and possible errors of the methods underlying the analysed data. For example, there is no discussion of potential errors arising from composing a time series of satellite data derived from various platforms, neither for lake levels, nor for LULC. Particularly the LULC dataset is a questionable one - both maps were derived globally by different analysts, using platforms of different resolution, and different LULC classes. Is there any independent data/information source to confirm that

the dramatic transformation of LULC detected using these datasets in the area is real, and not an artefact of the datasets?

Authors' response:

This comment was also made by the first reviewer, and we agree that the consistency of the land cover maps is an important issue. In the revised manuscript we have included additional detail on how the land cover information was validated to the extent possible. The following clarification was added to section 2.

The two original land cover maps differ in sources of satellite data, resolution and processing algorithms. This could lead to erroneous interpretation of land use change between the two periods. A limited validation of the LULC maps was carried out through site visits to areas that remain unchanged in the two maps (water area, bare land and highland forest areas), and a good agreement was found with the classes shown in the two maps, though there were some small differences in area coverage. Additionally the changes in LULC found in the maps corroborated with changes in land use reported in other basins in Ethiopia (Rientjes et al., 2011).

5. The analysis of hydrological and meteorological indices underlying the paper is comprehensive, but the results are not presented adequately. For example, for streamflow, the authors present but a table summarising the number of stations showing trends. This is not very informative. It would be much more beneficial to present graphics showing spatial location of stations, mean value of indices and magnitude and significance of trends.

Authors' response:

Again this comment was raised also by the first reviewer. To improve the interpretation of the geographic distribution of stations with positive/negative trends, we have included a map of the stations. The symbols on the map show the direction of the trend, as well as if this is significant. As it is difficult to include symbols for all 17 indicators, we have grouped indicators showing trends in low flows and in high flows. We have included this as an additional figure, as Fig 1 has been updated in response to the following comment to indicate the availability of data at stations. This additional figure is included in this response (see figure 1C) as well as in the revised manuscript.

6. The significance of trends was tested using Mann-Kendall test. Autocorrelation is usually strong in the climate and hydrological data and it increases chance of "false positives", i.e. detecting trend while in fact there is none. To account for autocorrelation either pre Whitening (Storch 1995), modified MK test (Hamed and Rao, 1998) or boot strap version

of MK test should be used. Was autocorrelation tested for? How was the influence of autocorrelation in data on the significance of trend accounted for?

Authors' response:

We agree that autocorrelation is important and should be considered in the trend test. We have analysed data quality for randomness (by run test), independence or persistence (through a test of autocorrelation), and consistency check (through the double mass curve test). We used only those stations which fulfil these tests. Yue and Pilon (2004) have compared the power of the MK and bootstrap-based MK tests for trend analysis. Their finding showed that with serially uncorrelated data, the MK and bootstrap-based MK tests, which consider the tie in the data, have the same power. Many have revealed that, there is a risk of underestimating the real trend if we apply pre-whitening or the modified MK test for data that is serially uncorrelated (Yue and Wang, 2002; Yue et.al., 2002b). We did autocorrelation test and used only those stations with uncorrelated data for trend analysis, hence, we believe MK test is acceptable. We have added a phrase in the revised manuscript in section 2.4.1 (in the revised manuscript this is section 2.5.1) to clarify.

This test applies to serially independent data and was applied only to stations found to be serially uncorrelated in screening the data

7. The authors use low- and high pulse counts as an expression of flow variability. This is somewhat unorthodox measure of variability, and probably strongly correlated to measures of frequency of events. In fact, the measures of duration, frequency and variability as presented in table 2 are probably highly correlated. While it is entirely justifiable to have such a high variety of indices in a hydro-ecological study, in the study reported in the paper, all these indices create superfluous information. Perhaps it would be beneficial to scale down the detail of the study at the benefit of more clarity in interpretation of results, i.e. present one or two indices in each of the categories.

Authors' response:

Indeed we agree that in the IHA there are many indices (67 in total) that may be highly correlated. The 29 we selected were selected as these emphasise the hydrological characteristics of the basin, but as the reviewer suggests some may be superfluous in the context of the present analysis. In the revised manuscript we have reduced to 17 indices, focusing on those that are important for the magnitude trend analysis. We selected only two indices in the categories for timing, duration, frequency and flow variability. The 17 selected indices are listed in Table B below. The manuscript has been revised throughout to reflect the reduction in indicators considered.

8. Section 3.2 Changes in climate variability does not address the issue of climate variability at all. Rather, it describes changes in several metrics of climate that reflects extrema.

Authors' response:

We have changed the title of section 3.2 to better reflect the content:

3.2 Changes to precipitation, temperature and evaporation

Minor comments:

1. Is there a difference between metrics, parameters, indicators and indices as used in the paper? If so, it should be expressed clearly and these terms should be used consistently depending on their meaning. If not - perhaps one term only should be used. "Magnitude, timing, duration, frequency and variability" are characteristics not metrics.

Authors' response:

We have updated the manuscript to consistently used characteristics for the characteristic; magnitude, timing, duration, frequency and variability.

2. p. 1302 line 3: "Although this data has not been validated against observed data in the basin due to the lack of measurements from for example flux towers (Trambauer et al., 2013), it can be applied for detecting trends." What is the basis for stating that?

Authors' response:

Thank you for the comment. As shown by Kim, et al. (2012) and Mu et al. (2011), the MODIS 16 actual and potential evapotranspiration have variable performance from region to region. The research showed that they performed poorly in grassland and in arid area, whereas, performed well on forest land as validated with data of ground measurement of flux tower. But, this accuracy may not affect the trend of the data as the validation accuracy is affected spatially than temporal. In this perspective, it can be used to detect trends at a given place irrespective of its magnitude. We used as independent indication of trend of evapotranspiration which can give evidence on natural flow regime and land use land cover change by detecting the statistical trend without focusing on magnitude.

Editorial comments:

1. p. 1302 line 3: "... climatological fluxes such as precipitation, evaporation and runoff ..." runoff is not a climatological flux.

Authors' response:

We have rephrased this as:

climatological fluxes such as precipitation and evaporation as well as runoff

2. ibid.: "... Sensitive to change in fluxes ... resulting in variability ..." Although "change" is not used here in the meaning of "long-term change", I would suggest rephrasing to avoid confusion around change vs. variability.

Authors' response:

We have rephrased this as:

sensitive to variation in fluxes such as precipitation and evaporation and runoff fluctuation, resulting in variability of river flows as well as of water levels in end-point lakes that are often present.

3. p. 1302 line 8 - can something be relatively pristine? It is either pristine, or not. The second part of the sentence does not have any relevance to the first part. Please rephrase.

Authors' response:

The sentence has been revised and split into two:

Little water resources infrastructure has been developed in the basin to date, and it is considered pristine. The basin is endorheic and is the main source of flow to Lake Turkana in the East-African rift valley.

4. p. 1302 line 9 here and elsewhere "increasing trend" is a very confusing expression. It describes a trend that is getting stronger and stronger in time. I don't think the authors mean this. Perhaps they should use "positive trend".

Authors' response:

We have revised the manuscript throughout to use "positive trend".

5. p. 1302 line 11 - the reader does not know at this stage which metrics were tested.

Authors' response:

We have revised this as the five groups of hydrological characteristics (see also previous comment on the consistent use of metrics and characteristic:

Of the five groups of hydrological characteristics in the IHA (magnitude, timing, duration, frequency and variability),

6. p. 1302 line 15 "The impact ..." which impact?

Authors' response:

We have rephrased this to clarify

The change in the basin hydrology is…

7. p. 1304 line 19: "the model" - IHA is not a model. It is a software package, isn't it? Please clarify the use of "model", or change "model" to "software"

Authors' response:

We have rephrased this in the revised manuscript as "IHA software" instead of "IHA model."

8. p. 1305 line 12 - "We analyse ..." My impression was that IHA was used as a tool to derive indices describing NFR. The authors do not use it to "identify driving forces". In fact the driving forces are identified by the authors only in qualitative terms, using qualitative interpretation of fragmentary information.

Authors' response:

We agree that the IHA were used only to analyse the hydrological indices to describe the NFR. We have rephrased the sentences:

We analyse the temporal and spatial characteristics of the NFR change using the IHA and identify the driving forces of these changes..

9. p. 1306 line 10 - "not sufficient" - perhaps better "poor"

Authors' response:

Changed as suggested.

10. p. 1306 line 11 - "five homogenous regions" at this point leaves the reader baffled. Perhaps mention that you will describe the methodology later.

Authors' response:

We have added a comment that this will be explained:

Five homogenous regions were determined (see Fig.1b; the derivation of these five regions will be described in the methodology),

11. p. 1306 line 16 - "unequal" - perhaps better "uneven"

Authors' response:

Changed as suggested

12. p. 1306 line 20 - "we have identified stations with adequate data quality in terms of randomness, trend, persistency and homogeneity" - confusing statement - randomness, persistency of trends are not characteristics of data quality. What do the authors mean?

Authors' response:

We have rephrased the sentence as:

As with the streamflow data, we have identified stations with good quality data after testing the data for randomness, persistence (independence) and homogeneity

13. p 1308, line 5 "delineated" - perhaps better "divided"

Authors' response:

Changed as suggested

14. p 1308, line 7: "The identified regions are reasonable when verified from physical characteristics such as topography, land use land cove and climate." perhaps better identified regions correspond to ..." or coincide with ...

Authors' response:

The sentence has been rephrased as:

The identified regions coincide with physical characteristics such as topography, land use land cover and climate.

15. p 1308, line 16: "The natural flow regime ..." perhaps better "The natural flow regime is analysed based on metrics characterising flow magnitude, seasonality, duration, frequency of events and variability"

Authors' response:

Sentence has been changed as suggested.

16. p 1309, line 5: "structures for water resource development" are not human activities, but results of such.

Authors' response:

Sentence has been rephrased and shortened.

Drivers that could affect natural flow regimes are mainly climate variability and human activities such as construction of water retention structures (Beavis *et al.*, 1997), deforestation and clearing of land cover, expansion of agricultural land (Masih *et al.,* 2011), urbanisation and catchment change and increased abstraction of water for irrigation and industries, impoundment of water (Alemayehu *et al.*, 2007), and modification of the morphology of the riverine system (Van Steeter and Pitlick, 1998).

17. p 1309, line 16: "Monthly rainfall in the Omo-Ghibe basin is characterised in a dry season (October–May) and a wet season (June–September)." - could not get the meaning of this sentence, please rephrase.

Authors' response:

Thank you for comment; we have rephrased the sentences as:

Rainfall in the Omo-Ghibe basin is characterised by a dry season from October to May and a wet season from June to September

18. p 1310, line 10: "hereafter known as" perhaps better: "hereafter referred to as ..."

Authors' response:

Changed as suggested.

19. p 1311, line 13: "most of which significantly as shown by annual Flow Duration Curve" flow duration curve does not show significance of trend.

Authors' response:

In this paper trends in flow characteristics were analysed through trends in selected IHA parameters. As the flow duration curve aggregates flows over time it in itself can indeed not be used to detect trend. However, by dividing the period in to roughly two halves and analysing the FDC over the two periods we can see if there are any changes to the FDC that corroborate trends found in the other indicators. We have removed the sentence suggesting that trends were detected in the flow duration curves. Differences in the FDC between the two periods are discussed at the end of the paragraph.

20. p 1311, line 23: "These curves are developed for two 15yr periods (from 1970 to 1995 and 1996 to 2008)" - these periods are 26 years and 13 years respectively, not 15 years.

Authors' response:

We have corrected this in the manuscript. However, on suggestion of the first reviewer we have shortened the period over which the data was analysed, resulting in two periods of equal length of 13 years.

21. p 1311, line 29: "very few stations" - how many exactly

Authors' response:

Thank you for the comment, we have revised the manuscript as:

...trends are found to be significant at only two stations,...

22. p 1312, line 6 "Indicators associated to frequency" - perhaps better: "indicators describing frequency"?

Authors' response:

Changed as suggested.

23. p 1315, line 20 - there is no Fig. 9

Authors' response:

We have corrected this; it should have been Fig. 6

24. p 1315, line 20 "the correspondence of the pattern in the inflows to the variability of lake levels is clear." - No, not at all. There is very little correspondence in Fig. 6 between inflows, which are dominated by seasonality and do not show any visible trend, and lake levels, which are dominated by trend and show some seasonality.

Authors' response:

This comment has been addressed in response to the general comment number 3. We now include the cumulative residual rainfall in Fig. 5 (see response to comment no. 3 above).

25. p 1318, line 24-25: Abbreviations are not explained. Perhaps should be introduced in line 14.

Authors' response:

Although these abbreviations have been defined earlier we have added them again here as suggested for clarity.

26. p. 1332 Table caption does not reflects that the table lists stations where trend is significant at 10%

Authors' response:

Thank you. On the suggestion of the 1st reviewer, we have now omitted the 10 % significant test from Table 3. The revised table is included below.

27. p. 1333 the column describing direction of trend is not really necessary, as the direction is indicated by the sign of Sen's slope. Also, significance could be indicated more conventionally by a * or bold font

Authors' response:

We have revised Table 4 as suggested by the reviewer.

28. p. 1337 perhaps a small map of Africa with Ethiopia clearly marked could be added. Otherwise the map in the left-hand side of the figure is understandable only to those readers who are very familiar with the shape of Ethiopian borders.

Authors' response:

We have included a small African map with Ethiopian map in the revised manuscript. This is shown in Fig.1a below.

29. p. 1340 legend in figure a and b could be ordered (annual 1, annual2, dry1, dry2, wet1, wet2)

Authors' response:

We have revised the legend as suggested as shown below (Fig 4).

30. p. 1341 - was there no rainfall in 2011-2013? If not, the fact that data for these years are not shown should be marked. Also, I would suggest adding moving averages rather than the means.

Authors' response:

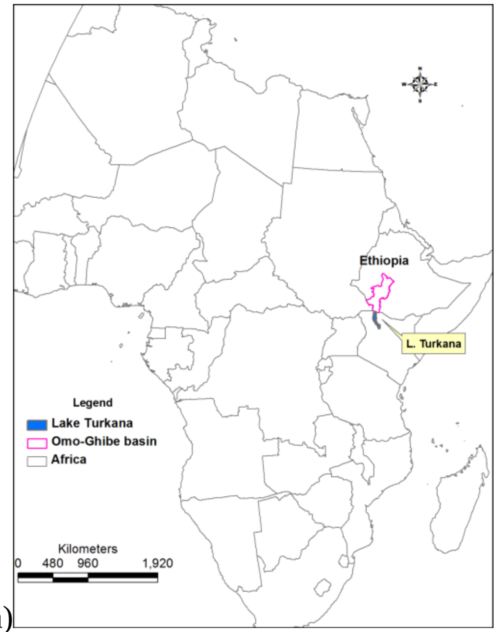
The caption has been updated to reflect the lack of rainfall in the last two years. The means of dry season and wet season was added to show the extreme seasons above or below the means (drought or flood time) and if such extreme events may correspond to increased/decreased lake level. We have included the cumulative residual rainfall in the figure to show how these match the pattern in the lake levels. We do not believe the moving average of the rainfall add much clarity.

Reference

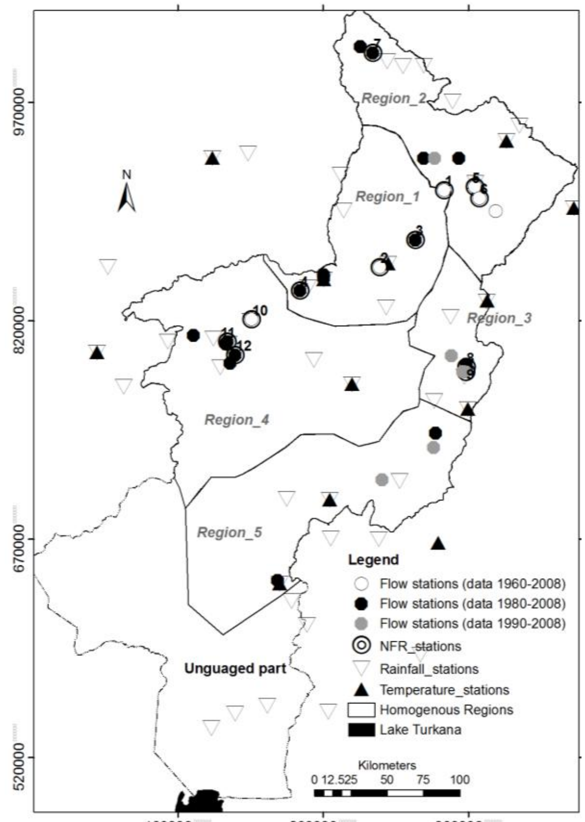
Yue S, Pilon P. 2004. A comparison of the power of the t-test, Mann-Kendall and bootstrap tests for trend detection. Hydrological Sciences Journal 49:1–37.

Yue S, Wang CY. 2002. Applicability of pre-whitening to eliminate the influence of serial correlation on the Mann-Kendall test. Water Resources Research 38: WR000861.

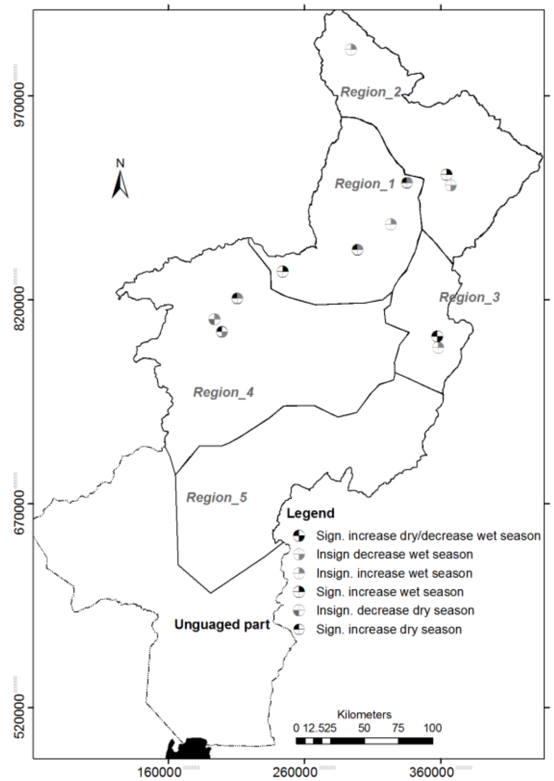
Yue S, Pilon P, Phinney B, Cavadias G. 2002b. The influence of autocorrelation on the ability to detect trend in hydrological series. Hydrologic Processes 16: 1807–1829



a)



b)



c)

Fig.1 (a) Africa and Ethiopia map (MoWR, 2011), (b) Homogenous regions and gauging stations based on their length of record year and stations used for NFR analysis, rainfall and temperature stations, (c) NFR trend of dry and wet season (sign. increase dry is for significant increase dry season flow; insign. decrease wet season is for insignificant decrease wet season flow).

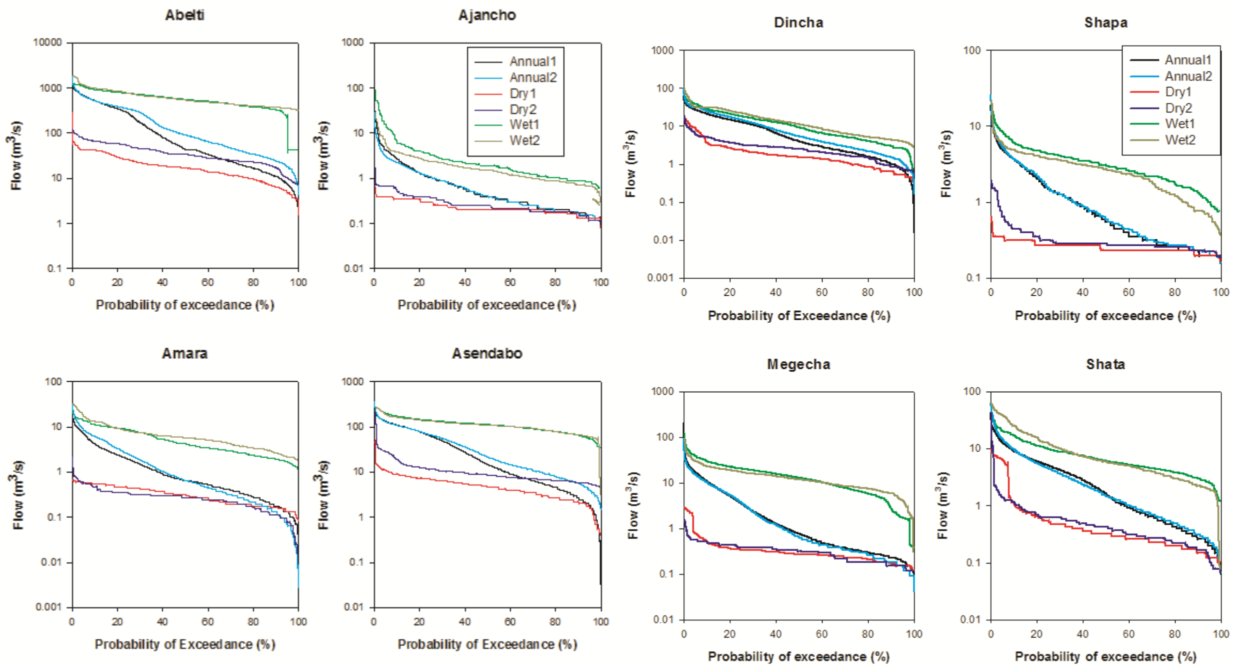


Fig. 4. (a) and (b) Flow duration curves for gauging stations used in the analysis, for Annual, Dry and Wet months change over two periods before 1995 (Annual1) and after 1995 (Annual2)

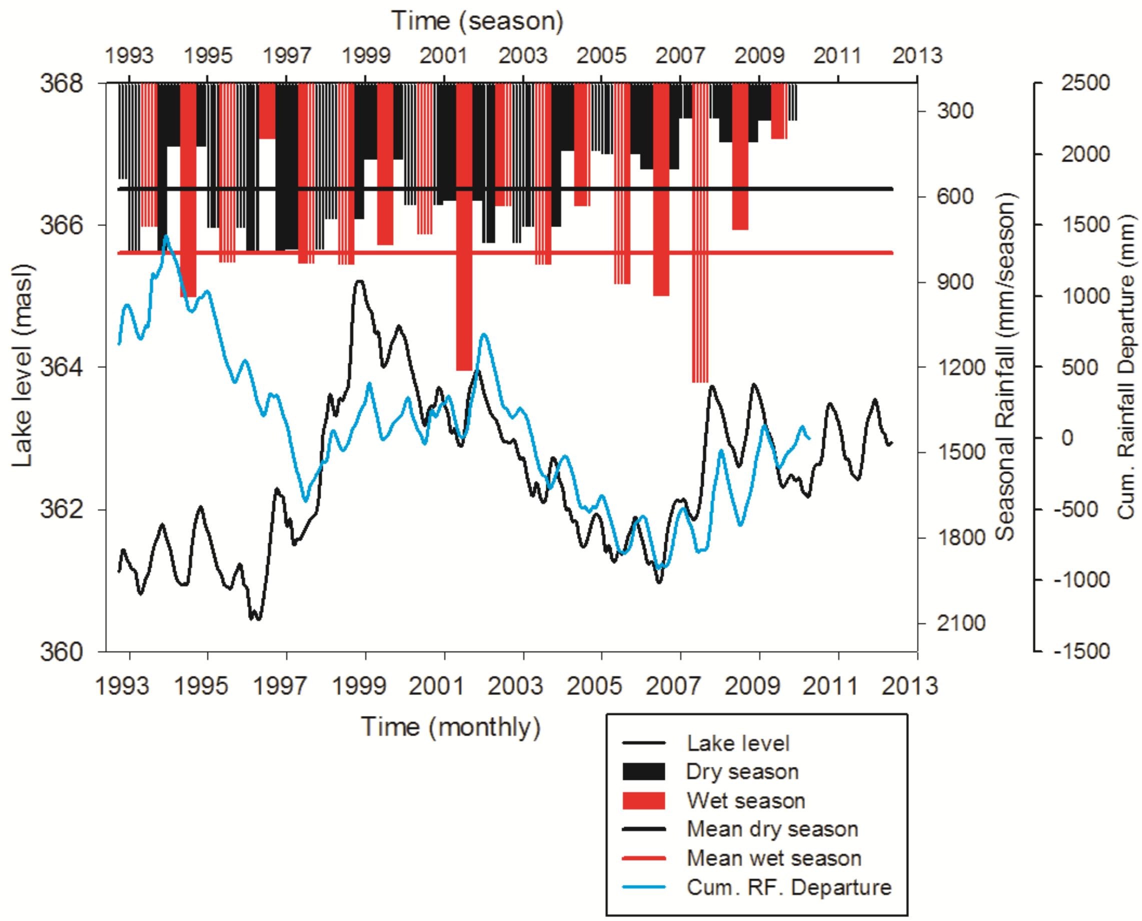


Fig. 5 (a) Lake Turkana water level fluctuation from late 1992 to early 2012 derived from altimetry data and Omo-Ghibe basin areal rainfall. Areal rainfall is averaged for the dry (black) and wet season (red), with the mean rainfall for each season shown as a horizontal black/red line for the dry season and wet season respectively. The areal monthly cumulative rainfall departure in the Omo-Ghibe basin is shown in blue.

Table B (Appendix): Main hydrological indices trend and its significance level for each of the stations in the homogenous regions, the bold figure shows an indices are significantly changed (decreasing or increasing) with 5 % significance level.

	REGION1								**Region 2**						**Region 3**				**Region 4**					
	Abelti		**Asendabo**		**Bidru**		**G.Seka**		**Amara**		**Megecha**		**Wabi**		**Ajancho**		**Shapa**		**Dincha**		**G.Shebe**		**Sheta**	
Hydrological Indices	Mean value	z	Mean value	z	Mean value	z	Mean value	z	Mean value	z	Mean value	z	Mean value	z	Mean value	z	Mean value	z	Mean value	z	Mean value	z	Mean value	z
Annual	188.4	1.1	40.2	1.3	0.5	-1.1	3.8	**2.5**	1.9	**2.7**	3.2	0.4	32.5	0.6	1.1	-1.1	1.4	0.6	8.9	1.1	58.1	1.3	3.7	-1.1
Dry Season	60.2	**2.4**	17.1	1.8	0.2	1.1	1.3	1.4	0.6	1.8	1.2	0.6	10.5	0.2	0.5	1.1	0.7	0.6	4.1	**2.4**	20.4	0.6	1.5	1.1
Wet Season	444.8	1.1	86.3	0.6	1.1	-1.1	7.3	2.0	4.4	**2.8**	9.1	0.0	76.8	0.8	2.3	-1.1	2.8	0.3	15.7	1.1	110.9	1.3	6.8	1.1
7-day min flow	13.3	**3.6**	2.9	**3.3**	0.1	0.1	0.4	1.2	0.2	0.5	0.1	-1.3	1.4	-0.6	0.2	1.2	0.2	1.0	1.0	**3.8**	6.3	-0.1	0.3	**2.0**
7-day max flow	840.4	0.4	168.2	0.4	3.9	0.8	15.2	**2.9**	11.0	**2.5**	24.4	-1.8	247.6	0.8	8.2	**-2.7**	7.1	0.6	37.2	0.9	267.7	0.2	21.5	-0.2
Base flow Index	0.1	**2.6**	0.1	**3.8**	0.1	1.8	0.1	1.1	0.1	1.5	0.1	-0.8	0.1	-0.1	0.2	**2.0**	0.2	1.1	0.1	**2.0**	0.1	-0.5	0.1	**2.0**
FDC (Annual)*	188.4	5.5	40.2	5.8	0.5	**-4.0**	3.8	**5.0**	1.9	**7.0**	3.2	1.1	32.5	-1.8	1.1	**-4.2**	1.4	1.6	8.9	**6.7**	58.1	6.8	3.7	**2.1**
FDC (Dry)*	60.2	19.3	17.1	8.3	0.2	-0.6	1.3	-0.6	0.6	**-4.1**	1.2	5.0	10.5	-1.5	0.5	**5.0**	0.7	**5.5**	4.1	**3.1**	20.4	-1.4	1.5	-0.8
FDC (Wet)/*	444.8	0.5	86.3	3.4	1.1	-1.7	7.3	1.6	4.4	**3.9**	9.1	1.4	76.8	-2.6	2.3	**-2.2**	2.8	**4.7**	15.7	0.8	110.9	0.6	6.8	**3.2**
Date min flow	78.4	-0.6	87.7	-0.5	176.5	-0.2	69.5	-1.8	107.2	**-2.0**	93.2	**-2.1**	120.7	0.0	102.3	1.2	55.6	1.1	97.0	0.8	110.2	-0.4	0.3	-0.9
Date max flow	235.0	-0.6	233.6	-0.5	227.4	0.3	233.1	0.1	239.0	-1.0	210.3	-0.8	223.2	-0.8	224.0	-1.1	235.6	-1.1	232.0	-0.8	235.0	-1.2	224.5	-1.0
Ext.Low flow duration	31.6	-3.6	11.3	-1.9	11.2	1.3	8.4	-0.3	13.5	0.0	15.1	0.4	15.3	0.7	11.2	-0.4	15.8	-0.3	10.0	-0.4	23.0	-0.2	7.1	-1.4
High flow duration	19.6	-0.4	18.0	-2.3	9.4	**-3.7**	10.9	**-3.6**	19.3	0.5	12.6	-0.1	8.8	-1.0	5.9	1.5	6.0	-0.2	10.8	**2.0**	19.3	-0.6	6.7	-1.5
Ext.Low flow freqency	2.3	-2.9	2.8	-2.8	4.3	0.3	5.4	0.1	2.9	0.7	4.3	1.4	3.7	1.0	6.2	-1.1	5.3	-0.9	3.8	**-3.7**	1.8	-2.2	6.2	-0.9
High flow freqency	2.4	0.1	5.9	1.8	9.2	1.8	6.8	1.1	2.7	1.1	6.0	0.1	7.1	0.3	8.1	1.4	9.9	**-2.8**	10.4	-1.3	5.2	0.0	10.4	0.6
Low pulse count	3.2	-0.8	5.0	-1.0	5.0	0.5	7.1	0.2	4.4	-1.1	6.4	1.9	5.6	0.7	9.8	0.0	5.8	-0.5	6.6	**-3.8**	4.6	-0.3	7.1	1.1
High pulse count	4.7	2.0	3.6	1.2	8.8	0.1	8.7	0.8	3.8	**2.2**	7.4	0.1	9.1	0.0	5.0	-1.1	10.7	-0.7	7.2	1.2	5.1	0.0	8.0	0.2