Response to Reviewer #1

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

Worku et al. used 29 indicators of hydrologic alteration (IHA), climate and land cover change to study the changes in the natural flow regime in the Omo Gibe basin in the Southern Ethiopia. This is an interesting and much required study for this basin. Similar studies are also required for other basins where in situ data are scarce or unavailable. The research presented in this study is very relevant to this journal. This study is well structured and well presented. However, I have few concerns on how results are analyzed and conclusions are drawn. Below is the list of comments and concerns that have to be addressed.

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.

1. The major conclusion for this study is that dry season flows are increasing in the Omo-Gibe basin. Authors have analyzed stream flow trends from 12 stations and the results indicated that only 2 station show significant increasing trend (Table 3). How can authors conclude that overall trend in dry season flows are increasing when 10 out of 12 stations does not show significant increase in trend?

Authors' response:

We agree with the comment that when considering only the indicator of Dry Season Flow only two out of 12 stations showed a significant positive trend, making a conclusion on the increase of the indicator of "Dry Season Flow" difficult. However, there are multiple indicators that reflect on the magnitude of dry season flows. Table R1 below summarises the indicators that are relevant to the magnitude of low flows (dry season, 7-day minimum flow and BFI), showing also the number of stations with significant trend in each of the homogenous regions. This shows that there are regions that showed a more positive trend (region 1 and 4, both humid regions) and regions that did not show any trend (region 2 and 3).

We have revised the manuscript to make it clearer how we have reached our conclusion (page 1320 line 26).

Of the 17 indices considered, mainly those representing low flow magnitude, such as dry season flow, 7-day minimum flow, BFI and dry season FDC were found to show significantly positive trend, particularly in regions 1 and 4. Indices related to frequency of low flows in regions 1 and 4 also show a significantly decreasing trend, which reflects the increase in magnitude of low (dry season) flows.

Table R1: Number of stations showed significant positive trend at 5% significance test

Hydrological Indices	Region 1 (4 stations)	Region 4 (3 stations)	Region 2 (3 stations)	Region 3 (2 stations)
Dry season flow	1	1	0	0
7-day min. flow	2	2	1	0
BFI	2	2	0	1
FDC (Dry season)	3	2	0	2

2. It is hard to understand which stations are showing significant trend (by geographic location) and which stations are not. Can you identify stations with ids in Figure 1 and then discuss the results in trends so that readers can relate where significant trends are observed and how is the LCLU changing in those regions?

Authors' response:

We agree that it is not easy to interpret the geographic distribution of stations with positive/negative trends. We have included a map of the stations, with symbols showing the direction and the significance of the trend. As it is difficult to include symbols for all 17 indicators, we have grouped indicators showing trend in low flows and in high flows. We have included this as an additional figure, as Fig 1c has been updated based on the following comment to indicate the availability of data at stations.

3. Authors indicate that they have used stream flow data from 32 gauging stations (shown in Figure 1) which have records ranging from 14 to 46 years. It is not clear, which station has how many years of data. It would benefit readers if you can show in Figure 1 by classifying gauging station in Figure 1 by different colours based on the number of years of data available.

Authors' response:

We have updated the map in figure 1 to show the stations with length of the available recorded data in the revised manuscript. The flow data of all stations in the basin are categorised as being available from the 1960s, 1980s and 1980s, with different icons used in the map for each category. In addition, the stations with good quality and (almost) gap free data that were used for the Natural flow regime analysis are denoted as NFR-stations (The updated figure is included in this response letter). We will edit Fig.1 in the revised manuscript.

4. Are the multi-year data (stream flow) from each station used in this study continuous without any data gaps? How did you handle if the data available was not continuous and has data gaps? How did you handle such situation while dealing trends?

Authors' response:

It is mentioned in the manuscript that there are 32 stations in the basin, but only some of these have a long data record. We used only 12 stations for the trend analysis that had good quality data with only few data missing. Where there was missing data in these, we used multiple regression to fill missing data by data from nearby station in the same homogenous region. We will clarify this in the revised manuscript as follows (page 1306):

We have analysed the quality of data for randomness, independence or persistence (autocorrelation) and consistency both for streamflow and rainfall. We used only those stations which satisfied these criteria and had data without or with only few gaps. Where there were small gaps we used multiple regression to fill missing data using stations from the same homogenous regions

5. Author indicated that ... "from each region the stations with the best data in terms of quality and record length in excess of 20 years were selected for characterizing the natural flow regime and variability. For most stations the period of record available spanned from about 1982 to 2008, with the exception of the stations at Abelti and Asendabo, where data was available from 1963 and 1967 respectively." The MK trends results would change based on the length of the data used. For example, MK trend result for a data (1982-2008) could show significant positive trend but may show a different trend when data with different time period/ length (1963-2008) is used. So, I am wondering if the time period for each station or length of the data is different, how we can inter compare trend results from one station to another.

Authors' response:

We agree that there may be an effect on the result due to the use of a different time period, we reanalysed for the same period (1982-2008) all stations and checked the trend if there is any change on result. This showed that there are some slight differences of Sen's slope and Z (significance test), but overall it doesn't affect the significant test results, i.e., almost all the trend direction or significance remain the same as shown on Table 3A below. Numbers marked in yellow are those that have changed. We will edit the revised manuscript to reflect that we now used data for the same period as follows (page 1306):

For most stations the period of record available spanned from the early 1980's to 2008 and while data from some of the stations spanned from 1960's and 1990's to 2008, we used a common data period from 1982-2008 for all stations for the analysis.

6. Moreover, the time period of analysis or length is not same for all the variables in this study. For example, stream flow used is from 1982-2008; Rainfall from 1970-2008; Temp (1970-2008); ET (2000-2008); Water levels (1992-2008). How can we compare trends for these variables when the period of analysis is different? Authors should redo entire analysis by choosing a particular time period for all variables. Say 1982-2008. In case data is not available for at least 20 years, a different data set or variable should be used.

Authors' response:

As for the comment above we agree that the different time period for different variables in the trend analysis may affect the result. We have repeated the analysis for the common period of time as the reviewer suggests and will incorporate changes in the revised manuscript. We have analysed this for stream flow for the period of 1982-2008 as explained in response to comment number 5. We have also reanalysed the rainfall and temperature data for the same period and the results will be included in the revised manuscripts as shown in Table 4 and 5. However, for the ET and lake levels there is inadequate ground measured data in the basin. The remotely sensed ET and Lake level data is not available for the full period from 1982 to 2008. Table 4 and 5 have been edited as shown below. Numbers marked in yellow are those that have only slight change on figure, but not significant change, numbers marked in green are those that have changed in magnitude and direction of trend but not significant and those that marked by pink colour have significant change.

We will edit the revised manuscript in section 2.2, page 1307:

For this analysis we used data from 1982-2008, the same period for which streamflow data was available.

7. Page 1313, lines 27-29, authors indicate that P, PET, AET were analyzed for 70 spatially distributed points. However, previous research (Velpuri et al., 2013, Remote Sensing of Environment) indicates that point based estimates of P, PET, AET have more uncertainty than spatially averaged estimates. Moreover, daily point based estimates have lot more noise that can influence trends. Authors should use spatially averaged estimates instead of point based estimates.

<u>Authors' response:</u>

We have indeed used 70 spatially distributed points for P, PET and AET across all of five homogenous regions in the study area. These were subsequently averaged over each of the regions to determine the dryness index of the regions as shown on Fig. 2. This has been clarified in the manuscript.

The dryness index (DI=PET/P) and the evaporative index (EI=AET/P) were calculated using the mean annual value of PET, AET and precipitation (P) (mm/year) calculated at 70 spatially distributed points that represent all LULC. PET and AET were sampled from the 1 km^2 MODIS images, while values for P were determined using inverse distance weighting. The spatially distributed points were subsequently averaged over each homogenous region.

8. It is hard to believe the results of land cover change analysis unless both the land cover data are thoroughly validated. Both the LULC datasets used in this study are generated using different input datasets and different classification algorithms. Most often comparisons of such datasets do not agree with each other. How much of change do you attribute to the difference in data sources? Don't you think, if 83% of increase in grassland and cropland due to conversion of FL, GL and WL is real, it should show significant increase in flows from majority of the stations?

<u>Authors' response:</u>

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).

9. In the current manuscript, the discussion on the trends in IHA parameters for the 12 stations is presented as a whole. Instead, the trends in IHA parameters for the stations falling within a region vs LCLU change happening in the region should be examined to see the cause and effect of LULC change on each parameter trends. What I mean is that table 3 should be separated by regions. Then comparison should be made with the parameter trends (significant or not) vs the amount of LCLU change.

<u>Authors' response:</u>

Agree, at the end of this response letter, we try to show station in their regions (Fig. 1c) and the detail parameter of significance to help for this comment and previous comment #2 (Table 3). We used Table 3A and 3B to summarise the results of 17 parameters of 12 stations (Here, based on comments from reviewer 2, we decreased the number of indices to 17).

We have added the following clarification in section 4.1:

Spatial variation of natural flow regime was observed in the basin. Humid regions 1 and 4, which were dominated by FL, WL, WG and SL, have shown more significant positive trend in low flow magnitudes compared to dry sub-humid regions 2, 3 and 5 (Fig 1C) dominated by CL and GL. In the regions 1 and 4, FL, WL, WG, SL decreased by 64-73%, but CL and GL increased by 123-261%, where as in regions 2, 3 and 5, CL and GL increased by ranges of 21-38% only.

10. In section 2.2 add discussion on Evapotranspiration (PET and AET) and land cover datasets used.

<u>Authors' response:</u>

Agree, we will include the additional discussion on Evapotranspiration and land cover dataset used on revised manuscript as follows on section 2.2:

Land use land cover (LULC): For the analysis of LULC change and possible link to natural flow regime analysis, we used two sets of land cover data that coincide with the period of data of streamflow. These are the global land cover of NASA/NOAA Pathfinder Land (PAL) data set of 1981-1994 of 1 Km resolution from the Advanced Very High Resolution Radiometer (AVHRR) produced by the University of Maryland Department of Geography (UMDG) (Hansen et al., 1998) and the dataset of 2009 with a resolution of 300 m produced by the European Space Agency (ESA, 2010).

Potential and Actual Evapotranspiration (PET and AET): we used MODIS16 (Mu et al., 2011) remote sensed data of 1-Km resolution available the time period from 2000-2010 monthly data. This is to investigate possible trends in actual evaporation (AET) and potential evaporation (PET), and to understand if these reflect the trends found in the temperature data. The MOD16 ET datasets are estimated using Mu et al.'s Evapotranspiration (ET) algorithm. The ET algorithm is based on the Penman-Monteith equation (Monteith, 1965). It is considered the surface resistance as an effective resistance to evaporation from land surface and transpiration from the plant canopy. Obtained from ftp://ftp.ntsg.umt.edu/pub/MODIS/Mirror/MOD16/

11. Page 1305, Line 1, uses just 5% significance level instead of two significant levels.

Authors' response:

We will use only the 5% significant level as recommended and have revised the manuscript as "...at the 5% two tail significance level." For clarity, this can be found on page 1309 rather than 1305.

12. Page 1307, selection of homogenous regions should be numbered as an individual section (2.3?)

Authors' response:

We will correct this as suggested in the revised manuscript.

13. Page 1309, Lines 24-25, MOD16 is actual ET dataset. Does it provide potential ET data? Provide the link from where you downloaded the data.

Authors' response:

Yes, MOD 16 data set has both actual and potential evapotranspiration data. The Brief Introduction to MODIS Evapotranspiration Data Set (MOD16) by Mu et al. 2011 describes the data as follows: "The MOD16 global evapotranspiration (ET)/latent heat flux (LE)/potential ET (PET)/potential LE (PLE) datasets are regular 1km^2 land surface ET datasets for the 109.03 Million km^2 global vegetated land areas at 8-day, monthly and annual intervals. The dataset covers the time period 2000-2010."
The link to the data is: ftp://ftp.ntsg.umt.edu/pub/MODIS/Mirror/MOD16/, which is also included in the manuscript in the data availability section.

14. In table 3, can you classify all the indices into five categories as in Table 2. For easy comparison, please maintain the number for each indices same as in Table 2.

Authors' response:

We have updated Table 3 as suggested and the revised version as will be included in the manuscript is shown below.

15. Can you provide p-value in tables 4, 5, and 6?

Authors' response:

We will include the p value in the revised manuscript. The revised version of Table 4, 5 and 6 are found below.

16. Why have the authors presented flow duration curves (Figure 4) for pre-1995 and post 1995? This type of classification is not performed for IHA parameter trend analysis or for climate variable analysis. Then, why here? Although probability of exceedance has increased for post-1995 data, it could be due to climatic variability. It is not correct to draw conclusion that this increase in the probability (for 8 years) is due to land cover change.

Authors' response:

The objective of this paper is to analyse trends in streamflow, and subsequently if trends are detected to identify the possible drivers. These trends are assessed through trends in the IHA indicators over the selected time period. As the flow duration curve aggregates flows over time it in itself cannot 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 agree that it is incorrect to draw conclusions on land cover change on changes to the FDC alone, but suggest that the changes to the FDC contribute to drawing such conclusions in combination with trends identified in other IHA indicators. Our conclusions were not based solely on the change to the FDC, and we have revised the text to ensure this is clear.

In section 3.1, "... to analyse if there are any clear deviation changes to the distribution between the two periods, and if these corroborate trends found in other indeces."

In section 3.3, "The trends found in the IHA indeces, such as for the 7-day minimum flow, as well as changes to the dry season FDC between the first and second parts of the periods analysed reflect this increase in runoff. This would suggest the dominance of LULC change in the changing distribution of runoff over changes due to the climate effect."

17. Figure 5 indicates that mean annual dry season rainfall for Omo-Ghibe is decreasing. This is in contrast with the conclusion of the study that dry season flows are increasing. How do you explain this?

Authors' response:

Thank you for your comment. Indeed in Fig.5 there is some suggestion that areal rainfall of the basin (dry season) averaged over the whole basin may be decreasing. This trend was, however, not found to be significant for most stations as shown in Table 4, except at two stations. We agree that a significant decrease would seem to be in contrast with the conclusion on the increasing dry season flows. However, as discussed, the change in flows, in particular for lower flows during the dry season are also impacted by the actual evapotranspiration, which is related to multiple factors; including availability of water, land cover, and temperature.

We have not made any amendments to the manuscript in response to this question

18. Did you analyze the trends in the Lake Turkana inflow (Figure 6) obtained from Abera, 2012?

Authors' response:

We did analyse the trend of inflow to Lake Turkana from the model results of Abera, 2012. The period available is, however, too short (seven years) to reliably analyse trends.

We have added a short sentence in section 3.4 to clarify:

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.

19. Several sentences in the manuscript are confusing mainly due to poor choice of words. I would recommend a thorough English review.

Authors' response:

Thank you the reviewer for your recommendation, we will carefully go through the revised manuscript and improve as required.

20. It is difficult to understand that temperature in the basin is showing significant increase whereas PET trend is not showing any increase. Can you explain why? Provide more information on how PET data was derived.

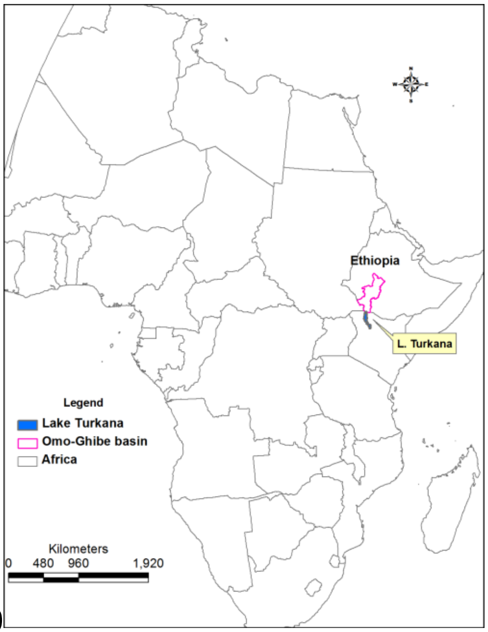
Authors' response:

Thank you for your comment. On could indeed expect that as temperature increases then evapotranspiration will also increase. This was, however, not the case in this analysis, which seems counterintuitive. As explained on page 1314 line 6-13, the land use/land cover changes, in combination with temperature changes will result in the changes to evapotranspiration being more complicated. In humid areas, LULC have a more dominant effect on potential evapotranspiration than does temperature change, while in an arid region changes to the climate will have more effect than LULC change (Yang et al., 2012;Tomer and Schilling, 2009;Zhang et al., 2001). It is well known that forest land has a higher evapotranspiration than cropland and grassland (Zhang et al., 2001), which means conversion of land use from forest/wood land to grass/cropland will decrease evapotranspiration.

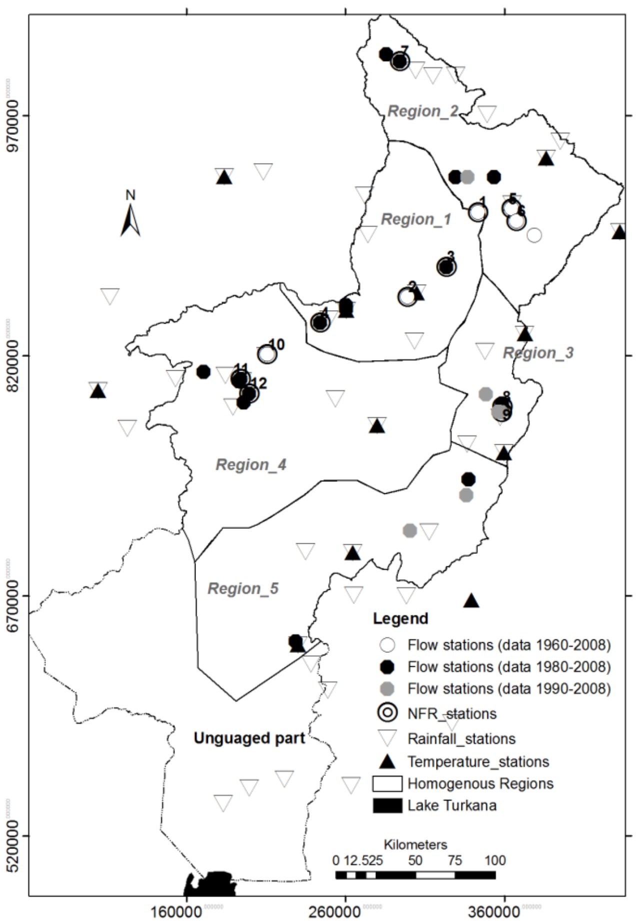
We derived potential evapotranspiration from the 1km^2 resolution MODIS 16 product (Mu et al., 2011). This uses a combined energy balance and aerodynamic method or Penman-Monteith equation (Monteith, 1965).

We have added a small sentence to make this clearer in section 2.4.1 (revised 2.5.1) on page 1309 as:

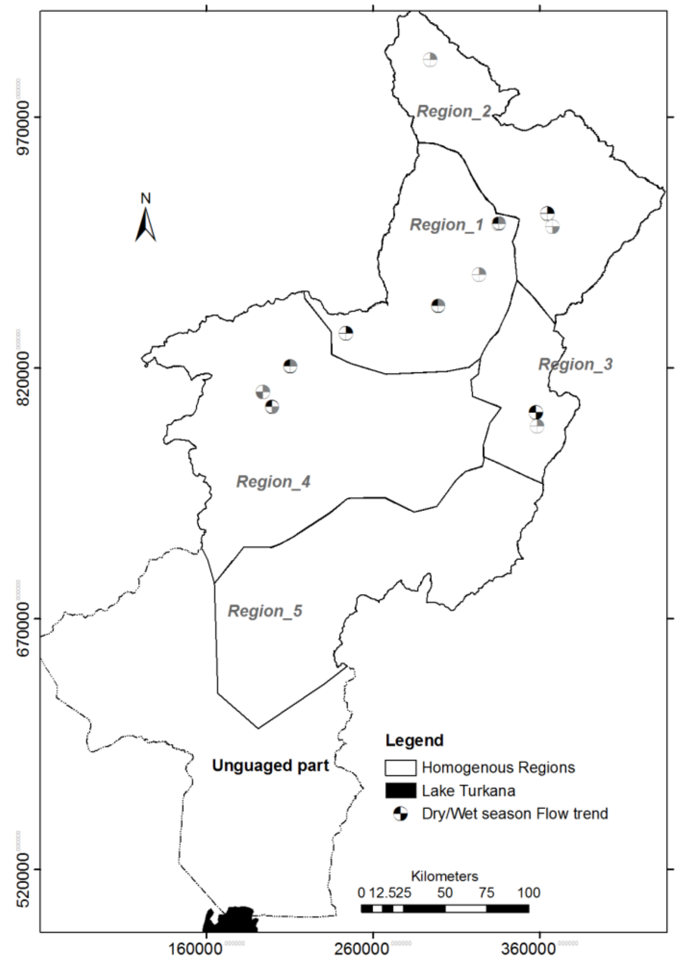
MODIS 16 evapotranspiration data uses a combined energy balance and aerodynamic method or Penman-Monteith equation (Monteith, 1965) based on remote sensed information (Mu et al., 2011).



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 (the first for signficant increasing high flow, the middle for significantly increasing dry flow and decreasing high flow and the later one for decreasing high flow, but insignificant)

Table 1: 17 Hydrological indices analysed at 12 stations and the number of stations that show a generally increasing or decreasing trend, as well as the number of stations at which the trend is significant at the α=5%.

	(1)	(2)	(3)	(4)	(5)
S.No.	Hydrological Indices	General increasing	Significant increasing @ α=5%	General decreasing	Significant increasing @ α=5%
	Magnitude				
1	Annual flow	8	2	4	1
2	Dry Season flow	9	2	3	0
3	Wet Season flow	6	2	6	1
4	7-day minimum flow	7	4	4	1
5	7-day max. flow	8	2	4	2
6	Base flow Index	8	5	4	0
7	FDC (Annual)	8	7	4	3
8	FDC (Dry season)	6	5	6	4
9	FDC (Wet season)	6	3	6	3
	Timing				
10	Date of min. flow	3	0	9	2
11	Date of max. flow	2	0	9	0
	Duration				
12	Ext.Low flow duration	2	0	9	0
13	High flow duration	4	1	7	2
	Frequency				
14	Ext.Low flow frequency	2	1	7	4
15	High flow frequency	5	1	2	1
	Flow Variability				
16	Low pulse count	3	0	6	2
17	High pulse count	5	2	2	0

Table 3A: Main hydrological indices trend and its significance level for all data in the range of 1982-2008 compared to longer period of data (1960s-2008), the bold figure shows significantly changed with 5 % significance level.

Hydrological Indices	REGION1								Region 2								Region 4			
	Abelti 1960s		Abelti 1980		Asendabo		Asendabo 1980		Megecha		Megecha 1980		Wabi		Wabi 1980		G.Shebe		G.Shebe 1980	
	Sen's slope, (trend)	Z	Sen's slope, (trend)	Z	Sen's slope, (trend)	Z	Sen's slope, (trend)	Z	Sen's slope, (trend)	Z	Sen's slope, (trend)	Z	Sen's slope, (trend)	Z	Sen's slope, (trend)	Z	Sen's slope, (trend)	Z	Sen's slope, (trend)	Z
Annual	0.88	1.11	26.37	1.15	0.09	0.49	3.06	1.33	-0.03	**2.19**	-0.12	0.417	0.16	0.98	1.65	0.584	0.01	0.01	5.18	1.32
Dry Season	0.7	**2.10**	19.11	**2.38**	0.11	1.66	2.69	1.83	0.01	0.04	-0.13	0.580	0.03	0.3	0.36	0.21	0.05	0.41	1.20	0.58
Wet Season	1.59	1.11	33.85	1.10	-0.19	0.6	5.34	0.63	-0.11	**2.41**	-0.02	0.042	0.58	1.3	5.00	0.770	-0.08	0.18	6.84	1.27
7-day min flow	0.23	**2.72**	7.42	**3.615**	0.07	**3.36**	**1.47**	**3.294**	0.01	**3.91**	-0.03	1.290	-0.02	1.18	-0.10	0.560	0.04	0.48	-0.07	0.09
7-day max flow	1.54	0.55	29.80	0.353	-0.19	0.3	3.30	0.417	-0.41	**2.83**	-1.96	1.760	2.06	0.84	28.70	0.770	0.6	0.61	3.35	0.210
Base flow Index	0.01	**2.19**	0.03	**2.65**	0.01	**2**	**0.03**	**3.75**	0.01	1.11	-0.01	0.830	-0.01	1.11	-0.01	0.056	0.01	1.93	-0.02	0.49
FDC (Annual)*	<0.0001	8.4	<0.0001	5.518	<0.0001	4.7	<0.0001	5.82	0.01	-2.55	0.287	1.06	0.89	-0.14	0.080	-1.751	<0.0001	5.03	<0.0001	6.84
FDC (Dry)*	<0.0001	14.7	<0.0001	19.29	<0.0001	8.4	<0.0001	8.27	0.003	-2.97	<0.0001	5.05	-1.43	0.15	0.131	-1.510	<0.0001	-3.98	0.16	-1.42
FDC (Wet)/*	0.023	2.3	0.590	0.539	0.28	1.07	0.001	3.361	0.016	-2.41	0.176	1.35	0.14	-1.46	0.008	-2.632	0.27	1.1	0.53	0.63
Date min flow	-0.42	0.78	-7	-0.617	-0.36	0.94	-3.00	-0.499	-1.83	**2.36**	-3.00	-0.208	-0.88	1.02	0.00	0	-0.91	1.55	-2.50	-0.37
Date max flow	0.01	0.04	-2	-0.639	-0.19	0.68	-4.00	-0.521	-0.31	1.07	-4.00	-0.79	-0.46	1.25	-3.50	-0.77	-0.5	0.74	-8.50	-1.24
Ext.Low flow duration	-0.17	0.86	-5.20	-3.620	-0.19	1.73	-5.20	-1.91	0.08	0.53	0.57	0.38	-0.22	0.42	0.81	0.65	-0.1	0.4	-0.0010	-0.18
High flow duration	0.01	0.05	-0.92	-0.370	-0.18	0.94	-3.57	-2.25	0.12	1.83	-0.06	-0.06	-0.15	1.34	-1.36	-0.96	-0.03	0.27	-1.300	-0.63
Ext.Low flow frequency	-0.03	1.99	-1.00	-2.940	-0.06	**2.03**	-2.00	-2.83	0.1	3.01	1.00	1.44	0.08	1.83	1.00	1	-0.14	**3.14**	-1	-2.15
High flow frequency	0.01	0.05	0.00	0.085	0.07	**2.34**	1.00	1.82	0.01	0.28	0.001	0.103	0.01	0.34	0.00	0.28	0.01	0.36	0	0
Low pulse count	-0.05	**2.14**	0.00	-0.820	-0.06	1.58	-1.00	-1.03	0.05	1.1	1.00	1.896	0.11	1.45	1.00	0.72	0.01	0.73	-0.001	-0.32
High pulse count	0.06	2.02	1.00	1.950	0.01	1.36	1.00	1.22	0.01	0.17	0.001	0.103	0.09	0.99	0.00	0.023	0.01	1.29	0	0

Table 2B: 17 Hydrological indices analysed at 12 stations and the number of stations that show a generally increasing (column A) or decreasing trend (Column C), as well as the number of stations at which the trend is significantly increasing (column B) and significantly decreasing (column D) at the α=5%.

S.No	Hydrological Indices	Region 1 (Id-1,2,3,4)				Region 2				Region 3				Region 4				Summary	
		A	B	C	D	A	B	C	D	A	B	C	D	A	B	C	D	Total B	Total D
	Magnitude																		
1	Annual	3	1	1	0	2	1	1	0	1	0	1	0	2	0	1	0	2	0
2	Dry Season	4	1	0	0	2	0	2	0	2	0	0	0	3	1	3	0	2	0
3	Wet Season	3	1	1	0	2	1	1	0	1	0	1	0	3	0	0	0	2	0
4	7-day min flow	4	2	0	0	1	0	0	0	2	0	0	0	2	2	1	0	4	0
5	7-day max flow	4	1	0	0	2	1	1	0	1	0	1	1	2	0	1	0	2	1
6	Base flow Index	4	2	0	0	0	0	3	0	2	1	0	0	2	2	1	0	5	0
7	FDC (Annual)*	3	3	1	1	2	1	1	0	1	0	1	1	3	3	0	0	7	2
8	FDC (Dry)*	2	2	2	0	1	1	2	1	2	2	0	0	1	1	2	0	6	1
9	FDC (Wet)/*	3	1	1	0	2	1	1	1	1	1	1	1	3	1	0	0	4	2
	Timing																		
10	Date min flow	0	0	4	0	0	0	2	2	2	0	0	0	1	0	2	0	0	2
11	Date max flow	2	0	2	0	0	0	3	0	0	0	2	0	0	0	3	0	0	0
	Duration																		
12	Ext.Low flow duration	1	0	3	1	3	0	0	0	0	0	2	0	0	0	3	0	0	1
13	High flow duration	0	0	4	3	1	0	2	0	1	0	1	0	1	1	2	0	1	3
	Frequency																		
14	Ext.Low flow frequency	2	0	2	2	3	0	0	0	2	0	0	0	3	2	0	0	2	2
15	High flow frequency	3	0	0	0	2	0	0	0	1	0	1	1	1	0	1	0	0	1
	Variability																		
16	Low pulse count	2	0	1	0	2	0	1	0	1	0	1	0	0	0	3	1	0	1
17	High pulse count	4	0	0	0	2	1	0	0	0	0	2	0	2	0	0	0	1	0

Table 3: Rainfall trend (sens's slope) in the dry and wet season and the significance level test (Z and P) for trends in the five homogenous regions with the α=5 % significance level for 21 stations and Lake Turkana

Season	**Dry Season**				**Wet season**		
Regions	**Stations**	**Sen's slope**	**Z**	**p**	**Sen's slope**	**Z**	**p**
Region 1	Asendabo	0.11	0.92	0.36	-0.12	-0.49	0.63
	Chekorsa	0.25	1.46	0.14	-0.16	-0.44	0.66
	L.Genet	0.04	0.30	0.76	-0.01	-0.05	0.96
	Jimma	0.15	0.59	0.56	0.07	1.91	0.06
Region 2	Butajira	0.12	0.63	0.53	0.38	**2.23**	0.03
	Gedo	-0.31	**-2.58**	0.01	-1.87	-4.91	<0.0001
	Weliso	-0.09	-1.08	0.28	-0.06	-0.33	0.74
	Welkite	-0.21	-0.96	0.34	-0.09	-0.42	0.68
Region 3	Areka	0.03	0.19	0.85	-0.285	-0.68	0.50
	Bele	0.01	0.10	0.92	-0.65	-0.83	0.41
	Hosana	0.085	0.06	0.95	-0.02	-0.12	0.91
	Walaita	0.19	0.97	0.33	0.44	1.36	0.17
Region 4	Bonga	-0.02	-0.10	0.92	0.25	0.46	0.65
	Mizan	-0.02	-0.06	0.96	-0.22	-0.94	0.35
	Sokeru	-0.05	-0.39	0.69	-0.37	-1.11	0.27
	Tepi	0.01	0.02	0.98	-0.665	**-2.17**	0.03
Region 5	Beto	0.3	**1.78***	0.07	0.81	**1.78***	0.07
	Jinka	0.08	0.69	0.49	0.07	1.35	0.18
	Kemba	0.035	0.10	0.92	-0.485	-0.78	0.44
	Keyafer	0.18	0.65	0.52	0.63	1.20	0.23
	Konso	-0.13	-0.90	0.37	-0.165	-0.42	0.67
Lake Turkana•			**2.11**				

Table 4: Monthly minimum, maximum and mean temperature trends (sen's slope), and its significance at α=5 % significance level test (Z and p) for 13 stations and on Lake Turkana in the study area

	Selected Stations	Maximum Temperature			Minimum Temperature			Mean monthly Temperature		
Regions		Sen's slope	Z	p	Sen's slope	Z		Sen's slope	Z	p
Region1	Jimma	0.054	**3.86**	0.0001	0.026	**4.18**	<0.0001	0.036	**4.93**	<0.0001
	Asendabo	-0.023	-1.94	0.053	0.076	**3.81**	0.0001	0.022	**2.71**	0.007
Region2	Welkite	0.25	**5.09**	<0.0001	0.056	**3.36**	0.0008	0.153	**5.14**	<0.0001
	Weliso	0.029	**3.40**	0.0007	0.076	**3.49**	0.0005	0.059	**3.78**	0.00016
	Butajira	0.08	**4.22**	<0.0001	0.025	0.001	1.000	0.046	1.8	0.056
Region3	Hosana	-0.013	-1.56	0.119	0.063	1.38	0.168	0.012	**2.48**	0.013
	Walaita	0.25	**3.4**	0.001	0.069	**4.0**	0.0001	0.072	**4.19**	<0.0001
Region4	Sokeru	0.015	0.84	0.399	0.026	1	0.315	0.007	1.11	0.268

	Chira	0.282	**2.37**	0.006	0.053	**3.89**	0.0001	0.053	**3.42**	0.00062
	Tepi	0.024	**2.90**	0.0037	-0.036	-1.33	0.184	-0.006	-0.73	0.464
Region5	Jinka	0.026	**3.48**	0.0005	0.024	**2.04**	0.04	0.023	**4.54**	<0.0001
	Sawla	0.045	1.80	0.071	0.025	1.32	0.186	0.022	1.63	0.104
	A.Minch	0.016	**2.16**	0.031	-0.054	**-2.46**	0.014	-0.014	-1.68	0.093
Lake	Turkana	0.08	**2.50**	0.01	0.29	**2.04**	0.04	0.17	**2.40**	0.02

Table 5: Remotely sensed data from MODIS16 actual- and potential evapotranspiration trend and significance test at 5 % in the homogenous regions of the study area and climate class based on UNEP 1997 Aridity Index (UNEP, 1997)

Regions	Actual Evapotranspiration		Potential Evapotranspiration		Aridity Index (P/PET)	Climate class
	Z	P	Z	P		
Region1	**-2.2**	0.03	-1.27	0.18	0.85	Humid
Region2	0.89	0.42	**-1.99**	0.048	0.53	Dry sub-humid
Region3	-1.3	0.17	-0.76	0.47	0.58	Dry sub-humid
Region4	**-2.9**	0.04	**-2.63**	0.006	1.01	Humid
Region5	0.15	0.81	-0.76	0.47	0.57	Dry sub-humid
Un-gauged	1.56	0.06	-0.71	0.49	0.24	Semi-Arid

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Response to Reviewer #2

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

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:

Agree, there are some issue in the introduction part to describe the hydrological variability which mainly important for ecological analysis. But, the natural flow regime or hydrological characteristics and change (trend) influence the wellbeing of ecosystem. It can be identified by statistical trend test whether there is a significant change on each of index. As we said, it is beyond the scope of this work. We will edit this imbalance in revised manuscripts, we edit the words variability and replace by change/trend accordingly on manuscript.

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.

<u>Authors' response:</u>

Agree, we will do quantitative analysis of rainfall residual versus lake level fluctuation to show whether multi-annual fluctuations in lake levels were related to periods of drought or anomalously wet rainy seasons. We have provided Fig. 5 monthly cumulative rainfall departure plotted against lake level. We included the result and discussion of analysis of Lake level fluctuation versus cumulative rainfall departure correspondence in section 3.4, 4.1 and 4.2. It can be observed from the figures as seasonal anomalies of rainfall contribute to lake level fluctuation and we will include on revised manuscript. The following sentence is added in section 4.1 to clarify:

However, there was high and low rainfall seasons which leads to longer and consecutive increase/decrease of cumulative rainfall departure, which is highly match with seasonal lake level fluctuations (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?

<u>Authors' response:</u>

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 based on 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 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 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 revealed 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 cannot 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:

Yes agree with the point of reviewer. It is clear, no or less significant trend in inflow compared to lake level trend. But, the seasonal variation of lake level and seasonality of inflow have some resemblance as shown on Fig. 6 (Peak and low periods). The long term decline (7 years) is a multi-annual variation related to periods of drought as stated on page 1316, line 7-16. Hence, the lake negative trend well derived by consecutive low flow or drought in the region as shown on Fig. 5 and 6. We revised as:

However, the declining lake level was not matched by inflow trend, rather, in this period there is observed less value of consecutive seasons or decreasing cumulative rainfall residual as shown on Fig. 5. to clear the doubts.

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.

<u>Authors' response:</u>

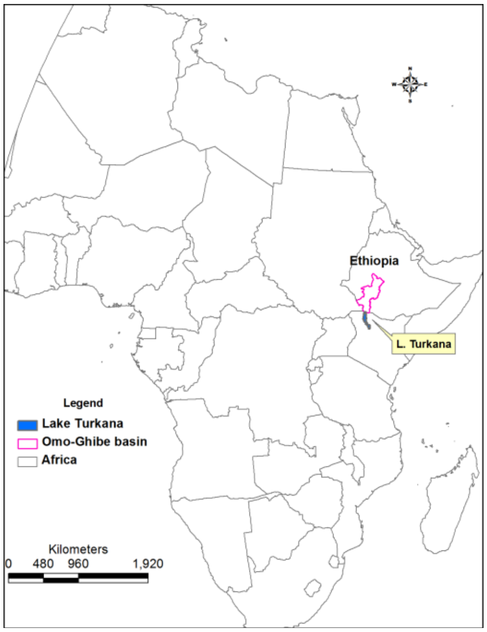
The caption has been updated to reflect the lack of rainfall in the last two years. The means of dry season and wet season was added to show the extreme seasons above or below the means (drought or flood time) and if such extreme events may correspond to increased/decreased lake level.

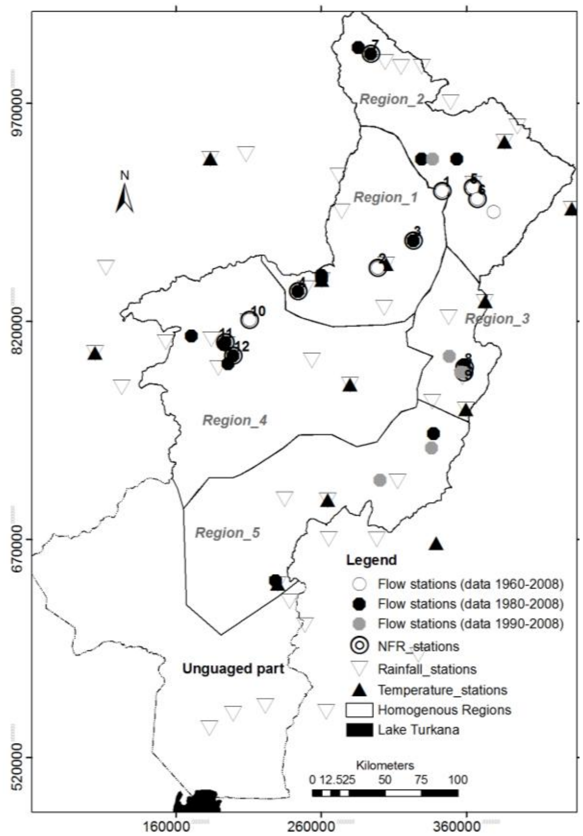
Reference

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





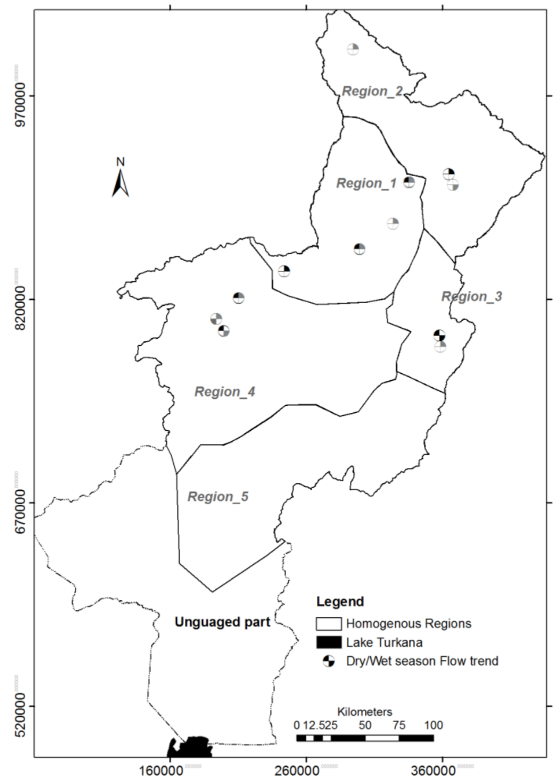


Fig.1 (a) Africa and Ethiopia map (MoWR, 2011), (b) Homogenous regions and gauging stations based on their length of record year and stations used for NFR analysis, rainfall and temperature stations, (C) NFR trend of dry and wet season (the first for signficant increasing high flow, the middle for significantly increasing dry flow and decreasing high flow and the later one for decreasing high flow, but insignificant)

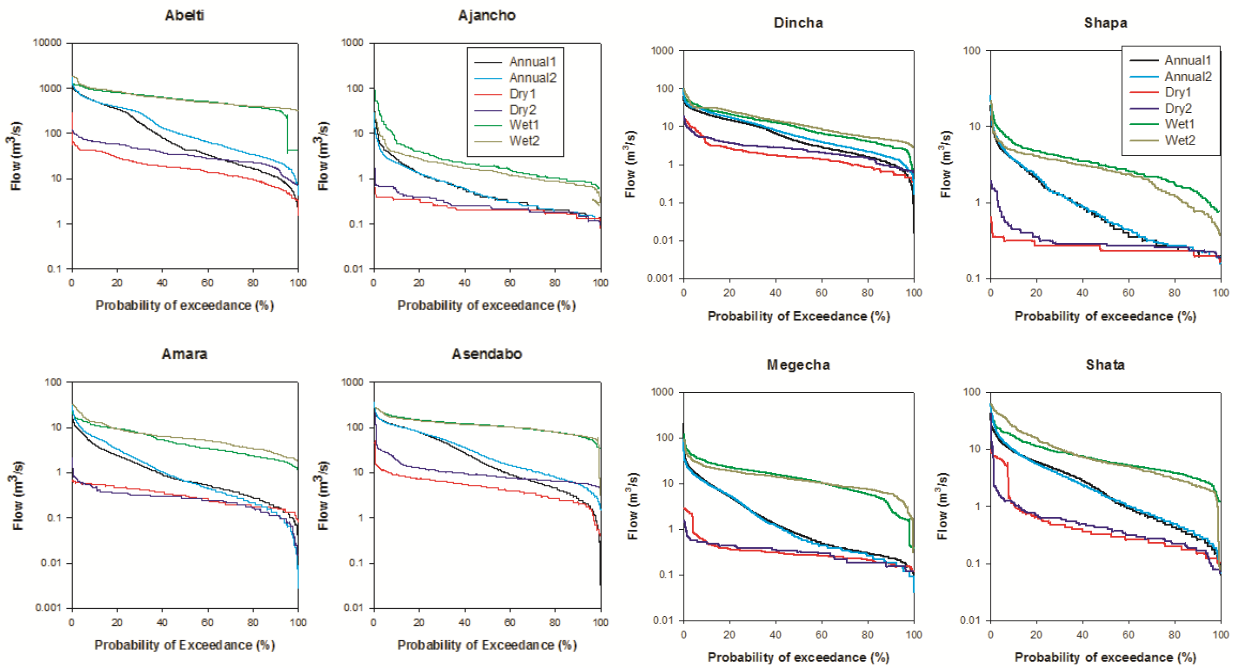


Fig. 4. (a) and (b) Flow duration curves for gauging stations used in the analysis, for Annual, Dry and Wet months change over two periods before 1995 (Annual1) and after 1995 (Annual2)

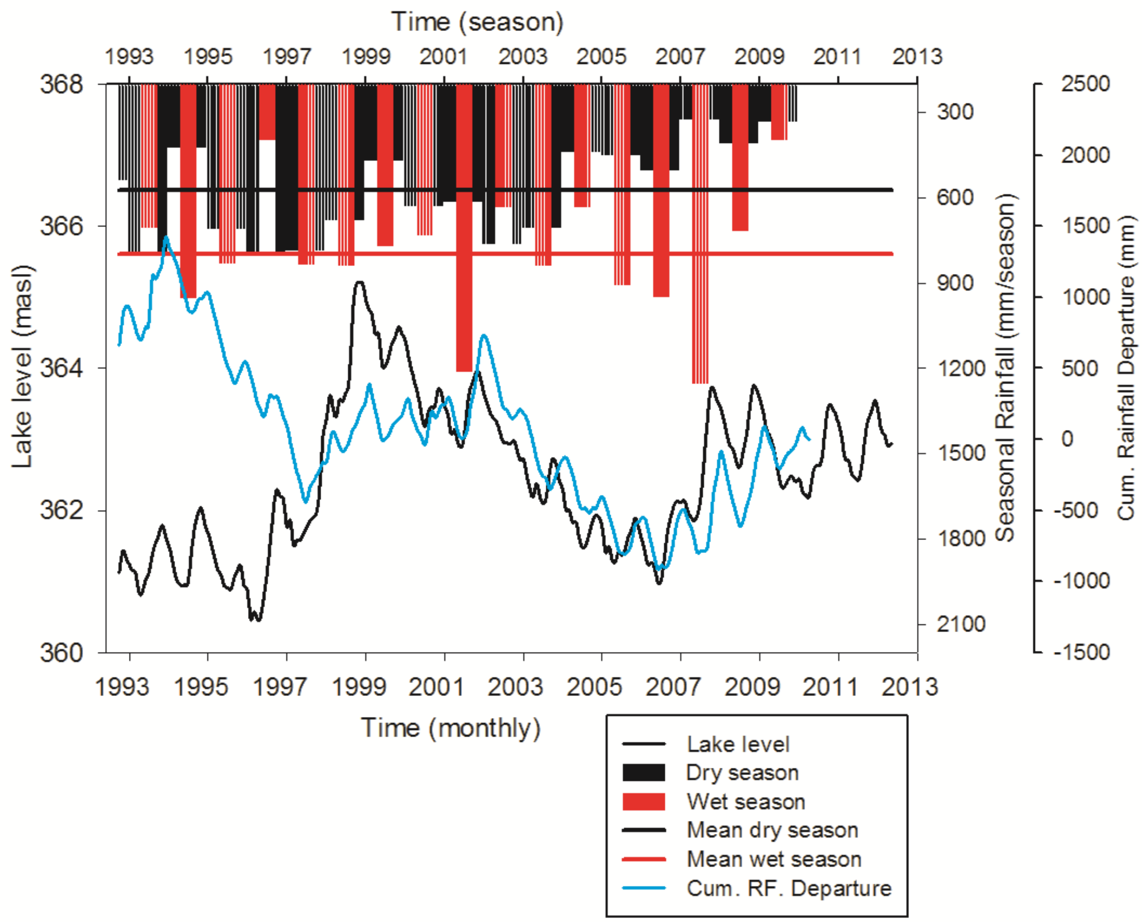


Fig. 5 Lake Turkana water level fluctuation from late 1992 to early 2012 derived from altimetry data and Omo-Ghibe basin areal rainfall. Areal rainfall is averaged for the dry (black) and wet season (red), with the mean rainfall for each season shown as a horizontal dotted line and areal monthly cumulative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

Table 3B: 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