

## ***Interactive comment on “Flow regime change in an Endorheic basin in Southern Ethiopia” by F. F. Worku et al.***

### **Anonymous Referee #2**

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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.

Major comments: - 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.

- 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.

- 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 quanti-

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tative 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.

- 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?

- 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.

- 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-

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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?

- 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.

- 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 reflect extrema.

Minor comments: 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.

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?

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

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

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.

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".

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

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

p. 1304 ln 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"

p. 1305 ln 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.

p. 1306 ln 10 - "not sufficient" - perhaps better "poor"

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

p. 1306 ln 16 - "unequal" - perhaps better "uneven"

p. 1306 ln 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?

p 1308, ln 5 "delineated" - perhaps better "divided"

p 1308, ln 7: "The identified regions are reasonable when verified from physical characteristics such as topography, land use land cover and climate." perhaps better "Identified

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regions correspond to . . ." or coincide with. . .

p 1308, ln 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"

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

p 1309, ln 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.

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

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

p 1311, ln 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.

p 1311, ln 29: "very few stations" - how many exactly?

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

p 1315, ln 20 - there is no fig. 9

p 1315, ln 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.

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

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line 14.

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

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

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.

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

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.

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