

Interactive  
Comment

## ***Interactive comment on “Inter-annual and seasonal trends of vegetation condition in the Upper Blue Nile (Abbay) basin: dual scale time series analysis” by E. Teferi et al.***

### **Anonymous Referee #2**

Received and published: 23 March 2015

The authors performed an elaborate analysis of vegetation-activity trends in the Upper Blue Nile basin. Their manuscript represents serious research efforts and contributes to a better understanding of vegetation dynamics in the region. As such, their work fits within the scope of ESD. I do, however, have some critical remarks about their methodology and I suggest to revise the work flow and the manuscript accordingly.

#### General comments:

1 – The 1982–2006 GIMMS data is not state-of-the-art (as mentioned on P174 L17). The team of Pinzon and Tucker released version 3g of their dataset, spanning 1982 until 2012. The processing chain changed to better facilitate trend analysis. I was

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



surprised to see how much this affected trends in some regions and I thus recommend to ask for this newest dataset and to redo your trend analyses. Please find the paper on the dataset here: <http://www.mdpi.com/2072-4292/6/8/6929>

2 – I have concerns about the use and interpretation of the HANTS output. The Fourier components themselves should not be interpreted as land-surface phenology (LSP) metrics, as opposed to what the manuscript suggests on P182 ff. In the Methods, the description is correct: the time series is decomposed into various harmonic functions, each described by a phase and amplitude. Component 0 represents the mean NDVI but component 1 and higher hardly represent biophysical processes. A change in one of these components cannot be interpreted as a phenological change but the sum of all components should be analysed for changing LSP metrics using common methodologies to derive them (e.g. following <http://dx.doi.org/10.1111/j.1365-2486.2009.01910.x> or others). I suggest to update the seasonality analysis accordingly.

3 – I find the description of some off-the-shelf methodologies (HANTS, LM, MK) rather long. Concise descriptions with references to corresponding literature would suffice. Their parameterization, on the other hand, is not always clear (e.g. BFAST).

4 – What is the incentive for reducing the temporal resolution of the data (P177 L5)? How was the monthly mean calculated for MODIS data and why the mean instead of the maximum value, like in the compositing technique of the source data? Was the information in GIMMS and MODIS quality flags regarded before aggregation?

5 – The introduction gives a general overview of vegetation-activity studies but lacks a bit the problem statement for the study site: why is this basin of special interest?

Specific comments:

P178 L16 "For the median trend, the breakdown bound is approximately 29%" Please add a reference for this statement.

P181 and P186 The terminology "coarser scale" (3.1) and "fine scale" (3.2) is ambigu-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



ous.

P182 L26 and P190 L5 If `_only_` Fourier component 0 changes, the shape of the curve remains unaffected and LSP metrics like start-of-season or length-of-season as defined by common extraction methodologies (e.g. %-amplitude threshold, max-increase, max-curvature) would not change. This renders the statements on P182 L26-L28 and P190 L5-7 incorrect or at least ambiguous.

P183 L8 "A decreasing phase angle means a shift to a later time of the year" This statement is correct but it does not necessarily affect the LSP metrics in the same way, because these are the result of the combined changes in various components. As mentioned in my second comment, I do not see the arguments for analyzing individual Fourier components instead of the additive harmonic function.

P184 L25 ff Change "capacity" to "scope". Furthermore, this paragraph (until P185 L12) is not very strong. The listed potential causes are very generic and trivial. It would help if the authors include a more site-specific analysis of potential drivers. This analysis, however, should be in the Discussion section and omitted from the Results.

P188 L16 "The trend break analysis ... was not able to detect trend breaks" I suggest rephrasing along the lines of "The trend-break analysis did not indicate significant trend breaks". This finding is, given the short time span (2001-2011) not strange. In the case of a trend break, the resulting segments would be short and the significance of the slope would likely be low due to the limited number of observations. The interpretation (L19 ff) of this monotonicity should be done with care and not be compared to monotonicity in the much longer GIMMS time series: a monotonic trend within the MODIS time span can have the same driver as one segment of equal length within the GIMMS time span.

P189 L23 In line with some previous comments, I doubt if this figure of 59.5% reflects actual LSP changes.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



P190 L18 "capacity" -> "scope"

P190 L18 - P191 L7 There is some repetition here.

P195 L4 The final conclusion is a bit out of the blue given the earlier statement that it is beyond the scope of the study to interpret driving factors (P190 L18). I suggest to remove latter statement on the scope and to conclude that the findings were interpreted as being linked to human activities. In the same conclusion, are inter-annual trends meant instead of intra-annual trends?

---

Interactive comment on Earth Syst. Dynam. Discuss., 6, 169, 2015.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

